Three Essays in Empirical Corporate Finance

by

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Looking back on four years of my PhD journey, amidst the challenges of the pandemic, it was indeed a difficult but transformative experience. As I reflect upon this moment and browse through the over eighty-thousand-word thesis, I can hardly fathom how that young man, who barely grasped the fundamentals of econometrics, embarked on this journey. Looking back, there are many to whom I owe my gratitude.

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If asked now why I chose to pursue a PhD, I would respond, "Because it is there!" The path to success requires relentless effort. While not without challenges, these years have been pivotal for the formation of my core values. Looking back, choosing to pursue a PhD was indeed a remarkable decision.

The road of life stretches far ahead, and comrades must continue to strive.

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Chapter 1. INTRODUCTION

This thesis comprises three empirical essays in the field of corporate finance, all of which utilize the difference-in-differences methodology to mitigate potential endogeneity issues. The first essay within this thesis focuses on examining the influence of pay transparency on employee productivity. I introduce the staggered adoption of pay secrecy laws at the state level to measure the state pay transparency level. Pay secrecy laws were initially introduced to address gender and race pay gaps by promoting transparency and prohibiting pay secrecy practices (Kim, 2013, 2015). These laws aim to empower employees by providing information on salary and enabling them to address and potentially reduce pay disparities (Cullen and Perez-Truglia, 2018b; Cullen and Pakzad-Hurson, 2019).

Employers implement pay secrecy policies to prevent wage comparisons and employee dissatisfaction (Colella et al., 2007; Kim, 2015). However, some argue that pay secrecy contributes to pay discrimination (Kim, 2013, 2015; Baker et al., 2019). Therefore, in the United States, there is an increasing emphasis on pay transparency to narrow gender and ethnicity pay gaps (Trotter et al., 2017; Heisler, 2021). The impact of increased pay transparency on employee productivity is ambiguous. On one hand, it can improve morale by addressing issues of prejudice and favoritism (Cullen and Perez-Truglia, 2018), and reduce uncertainty regarding compensation employees expect for their efforts (Hsieh et al., 2019). It can also promote gender equality and lead to increased productivity and retention (Bennedsen et al., 2019). In summary, increased pay transparency can have a positive effect on employee productivity.

On the other hand, increased pay transparency may reduce job satisfaction and result in complaints or resignations due to wage comparisons. This is because individuals not only care about absolute income but also their relative income (Colella et al., 2007). Lower-paid employees may be dissatisfied, while higher-paid employees may perceive efforts to mitigate inequality as threats (Card et al., 2012; Breza et al., 2018). Dissatisfied employees may engage in destructive behaviors, and highly valued employees may leave for better opportunities elsewhere (Hitz and Werner, 2012; Kim and Marschke, 2005), which aligns with "Inequity Aversion Theory" (Adams, 1965; Cowherd and Levine, 1992) and "Relative Deprivation Theory" (Martin, 1981). Therefore, increased pay transparency could also have a negative effect on employee productivity.

To enhance precision and minimize confounding factors, this essay introduces a novel measure to proxy employee productivity. It divides EBITDA, excluding incomes and incorporating expenses unrelated to employees or dominated by managers, by the number of employees. This essay stands as the first to uncover a significant correlation between heightened pay transparency and diminished employee productivity across a substantial and diverse sample of U.S. firms. This finding lends support to the notion that wage comparisons can erode job satisfaction. On average, firms headquartered in states where pay secrecy laws have been implemented experience a 1.58% decline in employee productivity compared to the overall sample mean.

Furthermore, this analysis reveals that the impact of pay secrecy laws is more significant in companies based in states characterized by lower levels of social capital. This finding underscores the heterogeneous nature of the effects and provides valuable insights into the mechanisms through which these laws operate. Moreover, this essay uncovers a compelling relationship between increased pay transparency resulting from the implementation of pay secrecy laws and a decrease in employee salaries. This discovery offers a potential explanation for the observed decline in productivity.

This essay makes the following contributions. First, in response to concerns regarding pay secrecy practices and their potential for pay discrimination (Kim, 2013, 2015; Cullen and Perez-Truglia, 2018; Baker et al., 2019), my empirical research aims to investigate the causal effects of pay transparency on employee productivity. To address endogeneity concerns and isolate exogenous variation in peer group pay (Gao et al., 2021), I employ a difference-in-differences framework using the staggered adoption of pay secrecy laws and incorporate high-dimensional fixed effects. This study extends the existing literature on pay secrecy law adoption and salary transparency, contributing to a broader understanding of the topic. Previous research by Kim (2013, 2015), Baker et al., (2019), Bennedsen et al., (2019), Mas (2017), Duchini et al., (2020), and Gao et al. (2021) sheds light on pay inequality and gender pay gaps. However, the effects of pay secrecy laws on firm-level outcomes and generalized employee productivity have not been thoroughly explored.

Second, to accurately capture employee productivity, I draw inspiration from previous studies and utilize individual profits as a proxy, constructing a less noisy and more accurate measure of broader employee output. My findings are consistent with alternative measures used in previous studies (Kale et al., 2016; Lins et al., 2017; Gao et al., 2018), enhancing my confidence in this novel proxy.

Third, my analysis reveals that increased pay transparency is associated with lower employee productivity, providing empirical evidence supporting the notion that wage comparisons can reduce job satisfaction (Card et al., 2012). This finding contrasts with the positive influence of pay transparency on innovation found by Gao et al. (2021), which focused on patents and citations for minority inventors. This is because my study focuses on a broader measure encompassing all employees instead of only innovators, which enables a comprehensive evaluation of employee incentives and behaviors that plausibly have direct effects on productivity (Faleye et al., 2013).

The second essay in the thesis examines the effects of restricted executive mobility on institutional shareholding. It introduces the concept of the Inevitable Disclosure Doctrine (IDD), which imposes stricter restrictions on managerial mobility to protect trade secrets. Restricted executive mobility imposes higher costs on managers whose current positions are at risk. This situation leads to increased career concerns and a stronger inclination to engage in opportunistic behaviors that can improve their current employer's perception of their abilities (Kothari et al., 2009; Gao et al., 2018; Ali and Li, 2019). Additionally, limitations on external employment opportunities for managers reduce the pool of potential replacement CEOs, making it difficult for companies to identify and recruit qualified successors, thus necessitating the retention of the current CEO (Grande-Herrera, 2019). As a result, this disruption in the labor market's disciplinary mechanism enables the occurrence of executive opportunistic behaviors (Li et al., 2017; Kim et al., 2020; Ali and Li, 2019; Li et al., 2018; Gao et al., 2018; Islam et al., 2020; Na, 2020). Consequently, it can be argued that restricted executive mobility undermines the quality of corporate governance.

To address the challenges posed by restricted executive mobility and enhance corporate governance, companies may choose to increase institutional shareholding (Chung and Zhang, 2011). This is based on the recognition of the monitoring role played by institutional investors. Therefore, it can be inferred that following restricted executive mobility, institutional shareholding may increase. However, institutional investors need to carefully evaluate the costs and benefits associated with monitoring, and exercise prudence in their actions. Thus, the quality of corporate governance, which is influenced by opportunistic behaviors, becomes a significant factor in the decision-making process of institutional investors. This suggests that, following restricted executive mobility, institutional shareholding may decrease.

This study provides the first empirical evidence on the impact of recognizing the IDD on institutional investors' equity holdings. Firms in states acknowledging the IDD experience an average 2% decrease in institutional shareholding compared to the sample period mean. This decline primarily stems from reduced corporate governance indicated by agency costs. Notably, activist and long-term institutional investors exhibit sensitivity to executive mobility constraints, reinforcing their monitoring incentives. Additionally, the study demonstrates that IDD recognition's influence on institutions is more pronounced in knowledge-intensive industries, highlighting the impact of executive mobility on institutional shareholding due to trade secret protection. These findings shed light on institutional investors' motivations to target portfolio companies, aiming to mitigate monitoring costs and fulfill fiduciary responsibilities. The study contributes to the related literature by examining the outcomes associated with restricted executive mobility. It also expands understanding of the IDD's economic effects, particularly on ownership structure, and complements existing research on institutional investor engagement in corporate governance strategies.

The third essay of the thesis revisits the causal link between takeover threats and corporate default risk, incorporating a fresh perspective by examining the implementation of Second-Generation State-level Antitakeover Laws alongside traditional proxies for takeover threat. An active takeover market serves as an external disciplinary mechanism for managers, promoting managerial replacements in cases of poor performance (Manne, 1965; Fama and Jensen, 1983; Jensen and Ruback, 1983; Scharfstein, 1988; Lel and Miller, 2015). However, anti-takeover protections weaken this mechanism by shielding managers and increasing their managerial entrenchment, resulting in heightened agency costs of equity. Balachandran et al. (2022) argue that reduced takeover likelihood, caused by weakened disciplining mechanisms, exacerbates agency conflicts, leading to diminished cash flows for debt payments and increased default risk (Driss et al., 2021).

Conversely, takeover threats can have a negative impact on default risk. Garvey and

Hanka (1999) find that companies influenced by second-generation state-level anti-takeover laws exhibit a reduction in reliance on debt. The decreased likelihood of hostile takeovers diminishes managerial career concerns and allows managers greater discretion in capital structure decisions, leading to a decrease in debt issuance (Grossman and Hart, 1980; Knoeber, 1986; Scherer, 1988; Stein, 1988; Jung et al., 1996). Furthermore, the financial leverage ratio can serve as an indicator of default risk (Traczynski, 2017; Cathcart et al., 2019), as default occurs when a company's asset value falls below its debt face value (Merton, 1974). Strengthened anti-takeover protection can potentially reduce default risk, benefiting debtholders, in line with the trade-off theory of capital structure (Kraus and Litzenberger, 1973).

This study is the first to show that firms incorporated in states influenced by Control Share Acquisition (CS) laws experience a decrease in default probability. Specifically, I find that these firms reduce their default risk by 18.2% on average compared to the mean default risk during the sample period. Additionally, I observe an increase in agency costs of equity, indicating that weakened external monitoring mechanisms prompt managers to exercise discretion in reducing debt usage. I suggest that agency costs of equity may serve as the possible link between takeover threats and default risk. This effect is particularly prominent in companies with high institutional shareholding, indicating that the decline in default risk for affected firms is a result of deteriorated corporate governance and heightened agency conflicts. Moreover, I report that the reduced default risk following the adoption of CS laws undermines shareholder interests, and managers' tendency to underinvest following the adoption of these laws suggests a preference for a "enjoy a quiet life" among executives.

This study contributes to the existing literature by expanding the research on determinants of corporate default risk. While previous studies have examined factors such as stock liquidity (Brogaard et al., 2017; Nadarajah et al., 2020), innovation performance (Hsu et al., 2015), and incentive structure (Bennett et al., 2015), I focus on the effects of anti-takeover protection on default risk. My findings differ from Balachandran et al. (2022) who argue that anti-takeover protection increases default likelihood derived from managerial opportunistic activities, which harms shareholder. In contrast, I conclude that decreased takeover threats are associated with a lower probability of default. Furthermore, I find that the reduced default risk resulting from the adoption of CS laws negatively affects shareholder benefits as well, contradicting Balachandran et al. (2022). I propose an alternative perspective that increased usage of anti-takeover provisions leads managers to prioritize their own well-being "enjoy a quiet life", harming shareholders but benefiting debtholders (Bertrand and Mullainathan, 2003; Klock et al., 2005; Chava et al., 2009; Qiu and Yu, 2009; Gormley and Matsa, 2016). To support my hypothesis, I find evidence of managers' underinvestment following the adoption of CS laws. Therefore, my study contributes to understanding how managers' risk exposure influences their decisions and highlights that traditional agency conflicts related to "private benefits" may not be the primary driver of activities deviating from shareholders' interests.

Finally, this study extends the investigation into the effects of CS laws, which have received limited attention thus far. I demonstrate that CS laws, along with business combination laws, contribute to variations in corporate governance and have a negative impact on default risk.

In the three essays presented, I employ the difference-in-differences methodology, utilizing staggered treatments as natural experiments to facilitate causal investigations. By employing a difference-in-differences design, I take advantage of the occurrence of multiple shocks that affect different firms at various time points. Specifically, I compare the effects before and after the implementation of legislation changes in states where such changes were implemented (the treatment group), contrasting them with the effects observed in states where no such changes occurred (the control group) (Gormley and Matsa, 2016; Klasa et al., 2018; Ali et al., 2019). This methodology enables me to address potential biases related to the timing of the laws (Bertrand and Mullainathan, 2003) and overcome alternative explanations that may arise in settings with a single shock, where contemporaneous events could influence my findings (Roberts and Whited, 2013). Additionally, I introduce a one-year lag for the state-level laws and doctrines, allowing sufficient time for affected companies to adjust their firm-level outcomes and further mitigating concerns of reverse causality.

It is noteworthy that these laws were not primarily designed to impact my variable of interest, thus suggesting that this effect is likely an unintended consequence, rendering my utilization of these laws particularly valuable. My analysis incorporates state-by-year and industry-by-year fixed effects, which enhances the robustness of my findings. Specifically, since numerous companies are headquartered in states that differ from the ones in which they are incorporated, I have the opportunity to include the state-by-year fixed effect. In summary, by incorporating these high-dimensional fixed effects, I can alleviate concerns pertaining to unobserved heterogeneity associated with the industry, state, or observation year of the firms (Gormley and Matsa, 2016).

In order to ensure the validity of my analysis, I employ a comprehensive set of diagnostic or robustness tests. The foundational assumption of the difference-in-differences methodology is that, in the absence of the law or doctrine, the treatment group and control group would exhibit similar trends. I demonstrate that the trends in dependent variables prior to the treatment are indeed comparable between the two groups of firms. Moreover, I conduct a placebo test by randomly assigning states to adopt the law or doctrine, ensuring equal probabilities of adoption across all states. This guarantees that any observed differences between and within states are not systematic. My results reveal that the actual coefficient estimate from the baseline regression lies comfortably in the tail of the distribution of coefficient estimates derived from 1,000 simulated placebo tests. This finding eliminates the possibility that my results are merely due to chance.

To address potential self-selection bias arising from firm-specific characteristics that could influence my results, I perform a Propensity Score Matching (PSM) test. For each year, I match treatment firms with control firms based on the firm characteristics used as control variables in my baseline regression. I estimate the probability of being assigned to the treatment or control group using a logit regression that incorporates all control variables, year, state, and industry fixed effects. Utilizing the propensity scores obtained from this logit estimation, I conduct matching within a caliper of 0.01 without replacement. The results, after controlling for sample selection bias through the PSM method, support my baseline findings, emphasizing that my conclusions are not driven by systematic differences.

Cengiz et al. (2019) have highlighted potential econometric concerns related to the aggregation of discrete difference-in-differences (DiD) estimates using ordinary least squares (OLS). These concerns include heterogeneous treatment effects and the possibility of negative weights assigned to specific treatments. To ensure a more precise examination of the relationship, I employ stacked difference-in-differences (DID) estimates as an additional robustness check. The stacked DID method aims to transform the staggered adoption setting into a two-group, two-period design. In this transformed design, the difference-in-differences estimate captures the average effect of the treatment on the treated, while taking into account the relative sizes of the group-specific datasets and the variance of treatment status within those datasets. This approach uses more stringent criteria for admissible, clean control groups. And separate datasets are stacked in event-time, which is equivalent to a setting where the events happen contemporaneously. Finally, I find consistent results, which confirms that my difference-in-differences estimates are not sensitive to heterogeneous treatment effects.

The subsequent sections of this thesis are structured as follows. The first essay examines the impact of state-level pay secrecy laws on employee productivity and is titled "Pay Transparency and Employee Productivity: Evidence from State-level Pay Secrecy Laws."

The second essay investigates the relationship between executive mobility and institutional ownership, drawing evidence from the Inevitable Disclosure Doctrine, and is titled "Executive Mobility and Institutional Ownership: Evidence from the Inevitable Disclosure Doctrine." The final essay critically evaluates the causal link between takeover threats and default risk and is titled "Takeover Threat and Default Risk: A Causal Re-evaluation."

Chapter 2 PAY TRANSPARANCY AND EMPLOYEE PRODUCTIVITY: EVIDENCE FROM STATE-LEVEL PAY SECRECY LAWS

Abstract

My study presents a causal analysis of the impact of pay transparency on employee productivity. I employ the difference-in-differences methodology, utilizing the staggered adoption of pay secrecy laws at the state level to address potential endogeneity concerns. This is the first paper to reveal a significant association between enhanced pay transparency and reduced employee productivity, even after accounting for high-dimensional fixed effects. This supports the notion that wage comparisons can diminish job satisfaction. To improve precision and minimize extraneous factors, my study introduces a novel proxy for employee productivity by dividing EBITDA, excluding incomes and incorporating expenses unrelated to employees or dominated by managers, by the number of employees. This approach builds upon previous productivity measures and enhances my understanding of employee productivity dynamics. Additionally, I observe that the effects are more pronounced in companies headquartered in states with lower levels of social capital, highlighting the heterogeneous impact and shedding light on the mechanisms through which pay secrecy laws operate. Furthermore, I discover that the increased pay transparency resulting from the implementation of pay secrecy laws leads to decreased employee salaries, offering a potential explanation for the decline in productivity. Finally, my results exhibit robustness across a range of tests, confirming the validity of my findings.

2.1. Introduction

It is widely acknowledged within the corporate landscape that "pay secrecy rules and practices" are prevalent among companies. These rules typically encompass contractual agreements and internal regulations that strongly discourage or even prohibit employees from disclosing their wages to their colleagues (Gely and Bierman, 2003; Bierman and Gely, 2004; Edwards, 2005). In a 2017 survey conducted by the Institute for Women's Policy Research, it was found that 25 percent of private sector employees operate in environments where salary discussions are formally prohibited, while an additional 41 percent work in environments that discourage such discussions.

The implementation of pay secrecy policies by employers is driven by the belief that they can mitigate employee dissatisfaction by curbing wage comparisons (Colella et al., 2007; Kim, 2015). Notably, there are multiple influential factors, aside from gender, that determine salaries, making wage comparisons among workers less informative and challenging for employees to quantify (Colella et al., 2007; Gely and Bierman, 2003). However, opposing views argue that pay secrecy rules and practices contribute to pay discrimination (Kim, 2013, 2015; Baker et al., 2019). In an effort to narrow the gender and ethnicity pay gaps, the recent legal and regulatory environment in the United States has increasingly emphasized pay transparency, making individual employee salaries more visible to their peers within the organization (Trotter et al., 2017; Heisler, 2021).

Consequently, it is of utmost importance to examine the actual effects of pay transparency. However, establishing a causal relationship between pay transparency and corporate outcomes is empirically challenging, even when compensation data is available (Heckman, 1998), due to the difficulty in isolating exogenous variations in the pay of the relevant peer group (Gao et al., 2021). Therefore, in this study, I introduce Pay Secrecy Laws to investigate the association between pay transparency and firm-level employee productivity. To draw causal inferences, I employ the difference-in-differences framework.

Pay secrecy laws are initially introduced with the aim of narrowing the gender and race pay gaps by promoting pay transparency and prohibiting companies from implementing pay secrecy rules and practices (Kim, 2013, 2015). These laws function by providing a shock of information through increased pay transparency, which in turn shifts the bargaining power of female employees in their favor against the company (Cullen and Perez-Truglia, 2018b; Cullen and Pakzad-Hurson, 2019). When pay is no longer a secret, employees are able to identify and address pay gaps, leading to potential actions taken to reduce them (Kim, 2013, 2015).

The effects of increased pay transparency following the adoption of pay secrecy laws on employee productivity can be ambiguous. From a morale perspective, when compensation is kept secret, employees are more likely to attribute any issues to unconscious prejudice, wage compression, favoritism, or discrimination, which can result in employee disengagement (Cullen and Perez-Truglia, 2018). Increased pay transparency can mitigate such morale-related impacts. And from the standpoint of monetary incentives, increased pay transparency can reduce labor market frictions by reducing the uncertainty employees face regarding the compensation they expect for their efforts (Hsieh et al., 2019). Additionally, companies affected by pay secrecy laws may improve gender equality, leading to increased productivity and retention among workers who value a fair working environment (Bennedsen et al., 2019).

However, opponents express concerns that enhanced pay transparency may reduce employee productivity. Card et al. (2012) illustrate that increased salary transparency can decrease job satisfaction for employees both below and above the median salary level. This is because individuals not only care about absolute income but also their relative income, leading to potential complaints or resignations by workers who are dissatisfied with wage comparisons (Colella et al., 2007). This aligns with the "fair wage-effort hypothesis" (Akerlof and Yellen, 1990), suggesting that increased pay transparency resulting from pay secrecy laws may reduce job satisfaction among lower-paid employees, while higher-paid employees may perceive attempts to mitigate inequality as threats (Card et al., 2012; Breza et al., 2018). According to "inequity aversion theory" (Adams, 1965; Cowherd and Levine, 1992) and "relative deprivation theory" (Martin, 1981), dissatisfied employees may engage in value-destroying behaviors to address salary inequities. Additionally, star and majority inventors may leave their firms or be recruited by competitors following the implementation of pay secrecy laws, as their pay is likely above the average level within their respective firms (Hitz and Werner, 2012; Kim and Marschke, 2005).

To investigate the competing views, I employ a difference-in-differences estimation strategy by introducing the staggered implementation of pay secrecy laws across different states in the US during the sample period of 1977-2019. The difference-in-differences estimation compares the changes in firms headquartered in states that adopted pay secrecy laws with those in firms headquartered elsewhere (Gormley and Matsa, 2016; Klasa et al., 2018; Ali, Li, and Zhang, 2019). The staggered adoption provides a unique feature that helps address biases associated with the timing of the laws (Bertrand and Mullainathan, 2003). By considering multiple exogenous shocks affecting different firms at different times, I can overcome a common identification challenge faced by studies with a single shock, such as potential noise coinciding with the shock that directly affects the outcome variable. Moreover, the underlying motivation for the enactment of pay secrecy legislation is to reduce salary disparities based on gender or race. As the primary intention of these laws is not to influence employee productivity, the effect I investigate is most likely an unintentional consequence (Gao et al., 2021).

The adoption of pay secrecy laws in a state is influenced by various political factors, including legislative support, influential decision-makers, and public opinion regarding pay secrecy (Ramachandran, 2012; Kim, 2015). To account for these factors, I include both state-of-location-by-year and industry-by-year fixed effects in my analysis. Specifically, many companies are headquartered in a state different from the one in which they are incorporated, allowing me to incorporate the location-state-by-year fixed effect. In total, the inclusion of these high-dimensional fixed effects strengthens my confidence that the coefficient I find is not driven by unobserved heterogeneous variations related to the firm's industry, location, or year of observation (Gormley and Matsa, 2016). Additionally, it is worth noting that employers initially implement pay secrecy rules and practices with the belief that they can reduce employee dissatisfaction by restricting wage comparisons (Gely and Bierman, 2003; Bierman and Gely, 2004; Edwards, 2005; Colella et al., 2007; Kim, 2015). Therefore, it is unlikely that a majority of firms would actively lobby or influence the passage of pay secrecy laws, as managers generally prefer to maintain a non-transparent pay policy (Gao et al., 2021).

To measure employee productivity, I adopt a proxy variable based on the Bureau of Labor Statistics' definition, which considers the ratio of employee output to employee input. Specifically, my measure, denoted as EP_1, represents EBITDA (earnings before interest, taxes, depreciation, and amortization) excluding incomes and incorporating expenses unrelated to employees or dominated by managers, divided by the number of employees. This measure extends previous studies in this area (Kale et al., 2016; Gao et al., 2018; Lins et al., 2017).

Using the aforementioned approach, my analysis reveals a decrease in employee productivity following the adoption of pay secrecy laws. On average, firms headquartered in states that have adopted these laws experience a decrease in the employee productivity by 1.58% relative to the sample mean. In my baseline regression setting, I introduce a lag of one year for the pay secrecy law indicator to allow sufficient time for affected companies to adjust their employee productivity, thus mitigating concerns of reverse causality. Furthermore, I find that this negative effect occurs two years after the passage of pay secrecy laws, providing further alleviation of concerns regarding reverse causality.

Considering heterogeneous treatment effects, Cengiz et al. (2019) emphasize the potential econometric concerns that there are issues in aggregating discrete DiD estimates by OLS. I thus employ a stacked difference-in-differences estimation as a robustness check, following Gormley and Matsa (2011), Deshpande and Li (2019), and Cengiz et al., (2019), and yields consistent results, ensuring that my difference-in-differences estimates are not sensitive to heterogeneous treatment effects.

To ensure the validity of my analysis, I conduct a series of tests. The identification assumption underlying the difference-in-differences methodology is that, in the absence of the law, treatment group and control group would exhibit parallel trends. Specifically, in my study, the change in employee productivity for firms headquartered in states that adopted pay secrecy laws would have been the same as that for firms headquartered in states that did not adopt these laws. I demonstrate that the pre-treatment trends in employee productivity are indeed indistinguishable between the two groups of firms. Additionally, I implement a placebo test by randomly assigning states to adopt pay secrecy laws, ensuring that each state has an equal chance of adopting these laws, thus guaranteeing that any differences observed between and within states are not systematic. my results indicate that the actual coefficient estimate from the baseline regression lies well in the right tail of the distribution of coefficient estimates generated from 1,000-simulation placebo tests. This finding eliminates the possibility that my results are purely driven by chance. Finally, to address self-selection bias resulting from firm-related characteristics that may influence my results, I perform a Propensity Score Matching (PSM) test. For each year, I match treatment firms with control firms based on the firm characteristics used as control variables in my baseline regression. I estimate the probability of being assigned to the treatment or control group using a logit regression that incorporates all control variables, year, state of headquarters, and industry fixed effects, as in my baseline regression. Using the propensity scores derived from this logit estimation, I conduct matching within a caliper of 0.01 without replacement. The results after controlling for sample selection bias using the PSM method support my baseline findings, highlighting that my conclusions are not driven by systematic differences between firms with high and low levels of pay transparency.

Furthermore, I perform a set of robustness checks to confirm the robustness of my main findings. Firstly, I explore alternative measures of employee productivity in my analysis. Additionally, I extend the sample period and exclude firms headquartered in states (Louisiana, New Jersey, and Minnesota) that adopted pay secrecy laws towards the end of my original sample period. I also exclude companies headquartered in California because of the impact of the 2010 California mandate requiring the disclosure of municipal salaries on compensation reductions and turnovers of top administrators. Finally, I account for the Inevitable Disclosure Doctrine (IDD), which may affect employee productivity (Gao et al., 2018), to ensure that my findings are not biased due to omitted variables. The results of these robustness checks consistently support the negative effect of pay secrecy laws on employee productivity.

To provide further evidence that the effect of pay secrecy laws on employee productivity is indeed linked to pay secrecy in the workplace, I examine the strength of this effect for companies headquartered in states with varying levels of social capital. I show that the effect of pay secrecy laws is stronger for companies headquartered in states with worse social capital measured as state-level voter turnout in US elections. Furthermore, I find that firm-level average employee salaries, as a measure of explicit employee welfare, decline following the increased pay transparency resulting from the adoption of pay secrecy laws. This finding illustrates a potential channel through which the adoption of pay secrecy laws affects employee productivity.

My study makes several significant contributions. First, in response to growing concerns surrounding the widespread use of pay secrecy rules and practices that have been accused of pay discrimination (Kim, 2013, 2015; Cullen and Perez-Truglia, 2018; Baker et al., 2019), my empirical research aims to investigate the causal effects of pay transparency on employee productivity. To address related endogeneity concerns and the difficulty in isolating exogenous variation in peer group pay (Gao et al., 2021), I employ a difference-in-differences framework by introducing the staggered adoption of pay secrecy laws and incorporate high-dimensional fixed effects.

Therefore, by extending the existing literature on the effects of pay secrecy law adoption and salary transparency, I contribute to a broader understanding of this topic. Kim's (2013, 2015) findings illustrate that the adoption of pay secrecy legislation contributes to reducing gender pay inequalities, particularly for women with graduate or college degrees. Baker et al. (2019) support this view by investigating the impact of public sector salary disclosure laws on university faculty salaries in Canada. Similarly, Bennedsen et al. (2019)

examine a 2006 legislative change in Denmark that mandates firms to provide disaggregated wage information, revealing a significant reduction in gender pay gaps. Furthermore, Mas (2017) finds that the 2010 California mandate requiring municipal salary disclosure leads to an average compensation decline of approximately 7 percent and a 75 percent increase in the resignation probability of top administrators. Duchini et al. (2020) demonstrate that while pay transparency reduces gender pay differentials, it results in pay compression from above and pay freezes in lower-paid occupations for male employees.

Although this series of studies sheds light on pay inequality and pay gaps, the actual effects of pay secrecy laws on firm-level outcomes have not been thoroughly explored. Gao et al. (2021) provide pioneering research on the influence of pay transparency on innovator productivity, but the effects on generalized employee productivity have not been considered. my analysis fills this gap by investigating changes in generalized employee performance, which provides a comprehensive reflection of employee incentives and behaviors (Faleye et al., 2013), following the adoption of pay secrecy laws.

Second, according to the Bureau of Labor Statistics, employee productivity is measured by the ratio of employee output to employee input. To capture employee productivity more accurately, encompassing all staff within a company and enhance my understanding of employee productivity dynamics, I draw inspiration from previous studies. For example, Kale et al. (2016) measured employee productivity as EBITDA per employee and sales plus changes in inventories per employee to analyze how changes in labor market conditions influence the disciplining effect of debt on employee productivity. Gao et al. (2018) used income before extraordinary items per employee as a proxy for employee productivity to investigate the relationship between employee turnover likelihood and employee productivity. Therefore, I utilize individual profits as a proxy for employee productivity and introduce a novel one. I divide EBITDA, excluding incomes and incorporating expenses unrelated to employees or dominated by managers, by the number of employees to construct a less noisy and more accurate measure of broader employee output. The outcomes exhibit coherence and conformity when employing the aforementioned measures (Kale et al., 2016; Gao et al., 2018), which enhances my confidence in my novel proxy. As a result, I present an extension to the determinant factors of employee productivity, thereby broadening my understanding in this realm.

Third, my analysis, incorporating high-dimensional fixed effects, first reveals that increased pay transparency is associated with lower employee productivity, providing empirical evidence that supports the view that wage comparison can reduce job satisfaction (Card et al., 2012). This finding contradicts the results of Gao et al. (2021), which illustrate the positive influence of pay transparency on innovation. The disparity can be explained by their focus on patents and citations as outcomes for inventors, especially minority inventors, who represent a minority of all staff and occupy higher positions in the wage distribution. In contrast, my study focuses on a broader measure encompassing all employees, which enables a comprehensive evaluation of employee incentives and behaviors that plausibly have direct effects on productivity (Faleye et al., 2013).

Finally, my findings carry significant policy implications. Currently, nine states have adopted pay secrecy laws, while the remaining states are still engaged in deliberations regarding their implementation. my paper contributes to the recent body of research on the economic consequences of pay secrecy laws, thereby deepening my understanding of their impact.

The subsequent sections of this study are structured as follows: Section 2.2 provides a comprehensive literature review, encompassing relevant studies in the field. In Section 2.3, the hypothesis is formulated, and predictions are outlined. The methodology employed in this study is presented in Section 2.4. Section 2.5 offers a detailed account of the data sources used and the construction of variables. In Section 2.6, an in-depth examination of the effects of pay transparency on employee productivity is conducted, supported by empirical evidence. Section 2.7 presents the empirical results of a series of robustness and diagnostic tests, including dynamic difference-in-differences estimation, stacked difference-in-differences estimation, PSM test, placebo test, and others. Finally, the paper concludes in Section 8.

2.2. Literature review

2.2.1 Employee welfare

Coase (1937) initially proposed the stakeholder theory, which conceptualizes companies as a more cost-effective alternative to expensive transaction modes. Thus, companies are established as intermediaries between customers and suppliers to reduce transaction costs associated with negotiation, contracting, coordination, enforcement, and fulfillment of obligations under a set of contracts. This perspective implies that, apart from stockholders and creditors, the scope of company claimants extends to encompass customers, suppliers, providers of complementary services and products, distributors, and employees (e.g., Demsetz, 1983; Jensen and Meckling, 1979; Williamson, 1979; Klein et al., 1978; Fama and Jensen, 1983).

Consequently, companies are expected to consider employee welfare, which refers to the commitment of companies to provide superior employment benefits and working conditions to enhance employee loyalty and improve productivity. It encompasses explicit contractual claims such as wages, which have legal binding and take precedence over the claims of bondholders and stockholders, as well as implicit agreements between firms and stakeholders that are typically non-contractual and of uncertain legal standing (Cornell and Shapiro, 1987). However, the payout of these implicit claims is unclear from a valuation standpoint, as their value depends heavily on the financial stability of the firm and its track record of fulfilling them (Gao et al., 2018).

Understanding the relationship between employee welfare, which determines employee satisfaction, and shareholder value has evolved over time. Traditional motivational theory, exemplified by Frederick Taylor, posits that money is the primary incentive for higher performance. According to this theory, employees are perceived as inputs, analogous to raw materials, engaged in unskilled activities. Consequently, companies tend to prioritize cost efficiency in extracting maximum productivity while minimizing expenses. Moreover, measures assumed to improve employee satisfaction, such as higher wages or reduced working hours, are viewed as less efficient and less profitable within this framework.

In contrast, modern management theory recognizes that individuals work for a variety of reasons, including the pursuit of happiness, contentment, and a desired lifestyle. Drawing upon this theory, managers can implement strategies to cater to the needs of their staff members and support their long-term skill development. Unlike the traditional view, employees are regarded as strategic assets capable of providing additional value to the company, particularly in knowledge-based sectors like technology and pharmaceuticals. Accordingly, prioritizing employee welfare becomes crucial for fostering employee engagement, which ultimately leads to enhanced performance and increased shareholder value. Supporting this argument, Levine (1992) and Wadhwani and Wall (1991) find that higher wages result in improved productivity.

Furthermore, better working conditions typically contribute to the establishment of a solid reputation for companies, ensuring continued engagement of stakeholders (Brammer and Pavelin, 2006). Maintaining a favorable reputation is essential for the survival and future performance of companies (Clarkson, 1995). Kotha et al. (2001) demonstrate that internet businesses with stronger reputations experience faster sales growth and higher market value. Roberts and Dowling (2002) observe a positive relationship between a firm's reputation and return on assets (ROA), with the benefits persisting over time. Fombrun and Shanley (1990) and Shamsie (2003) provide support for the positive association between reputation and financial performance. Additionally, layoffs, often considered detrimental to employee satisfaction and harmful to a firm's reputation (Flanagan and O'Shaughnessy, 2005), have been shown to impact firm performance (e.g., Chen et al., 2001a, 2001b; Pouder et al., 1999). Consequently, a loss of reputation, particularly among employees and non-financial stakeholders, leads to a devaluation of implied claims for new stakeholders, resulting in diminished future cash flows and company value (Cornell and Shapiro, 1987; Bowen, DuCharme, and Shores, 1995).

Employee satisfaction is commonly believed to reduce the likelihood of strikes that can significantly erode a firm's value (Newman, 1980; Becker and Olson, 1986; Bhana, 1997). Imberman (1979) distinguishes between pre-strike costs, such as reduced productivity due to worker dissatisfaction, during-strike costs, including decreased profits and management time during the bargaining process, and long-term costs, such as the loss of skilled workers and potential permanent loss of clients and suppliers. Gandz et al. (1980) add that strikes entail additional expenditures like stockpiling, shutdowns, start-ups, and training strikebreakers, with costs increasing in line with the strike's duration, consistent with Chermesh's (1982) view. These arguments underscore the significant costs incurred by companies due to strikes resulting from employee dissatisfaction.

Lastly, as businesses have evolved, human capital has emerged as one of the most valuable assets, making the maintenance of high employee treatment standards increasingly critical. Zingales (2000) argues that "New" enterprises operate in a more competitive environment that demands greater human capital. This heightened competition necessitates increased innovation and quality improvement, motivating organizations to focus on employee well-being (Turban and Greening, 1997; Lawler, 1997). Consistent with this observation, Falato et al. (2012) suggest that the gradual rise in corporate cash holdings over time can be attributed to the shift towards intangible capital in the US economy over the past few decades.

However, it is important to note that employee welfare can also be perceived as a costly investment that may not fully satisfy the requirements of shareholders in terms of expected marginal returns. This can be exemplified by certain instances where employee welfare benefits are overprovided and employee satisfaction can lead to significant negative abnormal returns (Meyer et al., 2001; Filbeck, 2001). Furthermore, investing in human capital comes with inherent risks and managerial challenges. For instance, the loss of key personnel can introduce additional risk factors associated with the loss of organizational capital, such as declining performance in the product market or increased financial distress (Levhari and Weiss, 1974). These factors can negatively influence investors' perception of a firm's production efficiency and growth prospects.

Given the irreplaceable nature of key talent, effective talent management becomes crucial for sustainable value creation (Eisfeldt and Papanikolaou, 2013). This implies that retaining the right talent and implementing systems that foster skill development pose significant challenges (Israelsen and Yonker, 2017). Liu and Xi (2021) investigate the effects of key talent outflow and stock price crash risk, finding that a higher risk of stock price crashes can result from a greater likelihood of losing critical stakeholders like key talent. Similarly, Liu and Ni (2021) illustrate that an increased risk of key talent departures can generate negative news, leading to stock price crashes. To make it visible, the resignation of Sir Jonathan Paul ("Jony") Ive, Apple's former Chief Design Officer and considered Steve Jobs' "spiritual partner," led to an $8 billion decrease in firm value, confirming the impact of key talent departures on stock prices.

In particular, my study focuses on employee welfare, specifically benefiting female employees. Corporate family-friendly policies provide working women with opportunities to prioritize their families without having to choose between raising a family and pursuing their careers. Several countries have implemented such policies, resulting in higher labor participation rates compared to countries without such policies, such as the United States. This is because that family-friendly policies, including paid parental leave, part-time employment options, and childcare support, have been shown to increase the percentage of working mothers (Hofferth, 1996; Joech, 1997; Gornick et al., 1998).

Moreover, Perry-Smith and Blum (2000) demonstrate that family-friendly corporate strategies contribute to market share growth and increased earnings for businesses. Jones and Murrell (2001) provide support for this view, reporting a positive abnormal return for firms recognized on Working Mother Magazine's list of "America's Most Family-Friendly Companies." To be included on the list, companies must offer equitable salaries, on-site childcare, promotional opportunities for women, and other family-friendly amenities. Edmans (2011) examines the link between employee satisfaction and long-run stock returns by analyzing a value-weighted portfolio of the "100 Best Companies to Work For in America." The study concludes that companies with high levels of employee satisfaction outperform in the long run, even though intangible assets, such as employee well-being, are not adequately accounted for in stock values.

Furthermore, Budig et al. (2012) find that publicly financed childcare and moderately long paid parental leaves can increase women's incomes and reduce the gender wage gap in many industrialized countries. While critics contend that implementing family-friendly policies could potentially reinforce gendered divisions of labor (Bergmann, 1997; Singley and Hynes, 2005). For instance, by predominantly offering part-time employment in low-paid occupations traditionally held by women, granting paid parental leave exclusively to women (e.g., women receiving disability payments for childbirth in certain U.S. states), and maintaining a gender wage gap favoring men, these policies may inadvertently reinforce the notion that women, rather than men, are primarily responsible for family care. Singley and Hynes (2005) propose that high-paying sectors should allow part-time employment, paid parental leave should be a "use it or lose it" requirement for both parents, and families should not experience a reduction in fathers' wages if they take such leave.

Overall, these arguments highlight the multidimensional relationship between employee welfare, shareholder value, and organizational performance. The evolving understanding of this relationship emphasizes the importance of considering employee welfare as a strategic asset and investing in practices that promote employee satisfaction, as it can lead to improved performance, increased shareholder value, and long-term success for businesses.

2.2.2 Employee engagement

Employee engagement encompasses the degree of motivation exhibited by employees to contribute to the overall success of an organization and their willingness to exert discretionary effort in accomplishing tasks crucial to achieving organizational objectives (Karsan and Kruse, 2011). The study conducted by Macey and Schneider (2008) presents a comprehensive model elucidating the concept of employee engagement, which establishes a connection between trait engagement (pertaining to positive personality attributes) and state engagement (reflecting a sense of energy and absorption, including factors such as satisfaction, involvement, commitment, and empowerment). This linkage subsequently gives rise to "behavioral engagement" or "extra-role behavior." Unlike actions focused on maintaining the status quo, engagement behaviors revolve around initiating or fostering change by undertaking additional responsibilities or pursuing novel approaches. It is important to note that while engaged employees generally exhibit higher levels of happiness compared to their disengaged counterparts, employee satisfaction and engagement should not be conflated. Engagement surpasses mere loyalty to the company or contentment with the employment arrangement—attributes commonly measured by the majority of firms. Instead, engagement is characterized by passion and unwavering commitment, exemplified by the willingness to invest one's efforts and discretionary contributions to advance the employer's success (Erickson, 2005), as exemplified earlier.

Spiegelman and Berrett (2013) argue that engaged employees contribute to superior customer service, enhanced work quality, increased productivity, and innovation, resulting in improved customer satisfaction, sales, earnings, and ultimately, shareholder returns. Consequently, high-performing companies tend to cultivate a culture that encourages employee involvement in decision-making, goal setting, and problem-solving activities, which subsequently enhances employee performance (Hellriegel et al., 1998).

Prior literature extensively supports the positive relationship between employee productivity and corporate financial performance (Tunio et al., 2020). Karsan and Kruse (2011) examined 39 publicly held companies that utilized employee engagement as an index measured through employee surveys. Their analysis of total shareholder returns over a five-year period revealed that organizations within the top 25% in terms of employee engagement exhibited an average total shareholder return of 17.93%, while those in the bottom 25% had an average return of -4.13%, indicating a notable difference of 22 percentage points. Hatane (2015) similarly highlighted the positive impact of employee satisfaction and productivity on corporate monetary performance using the partial least square statistical method. Additionally, research suggests that an organization's workforce positively influences its financial performance and market value (Bontis et al., 2005). Hence, establishing a productive employee-company relationship is advantageous and contributes to long-term organizational performance improvement.

According to the Bureau of Labor Statistics, employee productivity is gauged by the ratio of output (goods and services produced) to the input (labor working) required for production processes. Schoar (2002) and Brynjolfsson and Hitt (2003) calculate a firm's output as sales plus changes in inventories. Building upon previous literature, Kale et al. (2016) illustrate the use of EBITDA (operating income before depreciation and amortization) per Employee as a productivity measure capturing the value added by employees. In addition to increasing output levels, heightened employee effort may also reduce expenses or improve quality, thus enhancing the firm's profit margin. Consequently, EBITDA per Employee captures the impact on output as well as any influence on expenses and profit margins.

2.2.3 Pay discrimination

Gender and ethnic wage disparities are prevalent, indicating the existence of significant gender and racial pay discrimination. A substantial portion of pay gap can be attributed to the fact that women often occupy lower-paid occupations compared to men. Kim (2013) contends that women have experienced significant advancements in certain professional domains, like law and medicine, over the last three decades, yet they remain predominantly clustered in traditionally low-paying roles such as office support and service vocations (England, 2010). Research consistently demonstrates the challenges women face in securing high-paid executive positions (Smith, 2012). Hence, the lower wages earned by women can be attributed to their overrepresentation in lower-paid jobs and underrepresentation in higher-paid ones.

Alternatively, according to neoclassical economists, the lower productivity of women and their consideration of family responsibilities when selecting occupations contribute to the wage disparities. Women may opt for lower-paying employment to accommodate their family needs, often seeking jobs that offer flexible working hours, such as nursing and teaching. Supporting this perspective, O'Neill (2004) reveals that women with young children are more inclined to work part-time compared to men in similar circumstances. Moreover, women tend to avoid occupations requiring rapidly evolving specialized skills, such as physics, due to the expectation of taking time off to raise families.

Critics of these neoclassical arguments account for additional variables, including education level, working hours, part-time employment, presence of young children at home, and marital status, in order to explain variations in productivity beyond gender. However, even after controlling for these factors, women consistently earn less than men (Blau and Kahn, 2007). To challenge the notion that women are less focused on their careers or lack ambition, researchers have discovered that even when accounting for employment aspirations and occupational choices, the gender wage gap persists (Blau and Ferber, 1991). Weinberger and Joy (1997) go further by incorporating controls for college major, university attended, and grade point average, countering the assertion that women gravitate toward less lucrative disciplines, such as liberal arts, instead of engineering or sciences. Their findings indicate that women earn less than men even when attending the same college, pursuing the same major, and achieving the same GPA.

Furthermore, audit and correspondence research provide evidence that, even with equivalent qualifications, men are favored over women in hiring processes. Neumark

(1996) conducted a study where resumes of equally skilled men and women were left in restaurants, revealing that men received higher callback rates in establishments offering higher wages, while women fared better in lower-paying establishments. Bergmann (1996) explains this phenomenon by highlighting the tendency to highlight men's qualifications while disregarding those of women. Consequently, women with children are advised lower compensation due to perceptions of reduced competence and commitment to their jobs. Additionally, they are less likely to receive employment recommendations or be considered for management promotions (Correll et al., 2007).

In conclusion, Kim (2013) suggests various measures to address wage discrimination, including unionization of women, implementation of comparable worth policies, enactment of pay secrecy legislation, affirmative action, stronger non-discrimination laws, and the adoption of family-friendly policies. These initiatives are believed to contribute to the improvement of the gender wage gap.

2.2.4 Pay secrecy and pay transparency

Pay secrecy rules and practices, including contractual and internal provisions that prohibit or discourage employees from disclosing their wages to colleagues, are commonly implemented by employers (Gely and Bierman, 2003; Bierman and Gely, 2004; Edwards, 2005), with the aim of reducing employee dissatisfaction resulting from wage comparisons (Colella et al., 2007; Kim, 2015). Notably, salary determinants extend beyond gender, making wage comparisons between workers uninformative due to various challenging-to-quantify factors (Colella et al., 2007; Gely and Bierman, 2003).

Card et al. (2010) develop the relative income model, employing a randomized manipulation of access to coworker salary information, to elucidate the effect of peer salaries on job satisfaction. Card et al. (2012) provide a concrete example by referring to an experiment where a subset of randomly chosen employees at the University of California were informed about a new website disclosing the pay of university employees. Surprisingly, both below-median and above-median earners reported lower job satisfaction, with below-median earners demonstrating a significant increase in the likelihood of seeking alternative employment. The authors argue that employers have an incentive to maintain pay secrecy, as the costs for lower-paid employees outweigh the benefits for higher-paid peers, consistent with the concave function of relative pay in the inequality aversion model of Fehr and Schmidt (1999), leading to more substantial negative effects on low-wage earners than positive effects on high-wage earners.

Hitz and Werner (2012) argue that employees earning above their firm's average compensation may resist disclosing their salaries, potentially leading to more severe consequences such as employee attrition following the implementation of pay secrecy laws. Moreover, Kim and Marschke (2005) document that such employees are highly sought after by competing firms.

Numerous studies have been conducted to complement the perspective that individuals value not only their absolute income but also their relative income. These studies encompass various domains such as happiness (e.g., Luttmer, 2005; Solnick and Hemenway, 1998), health and longevity (e.g., Marmot, 2004), and reward-related brain activity (e.g., Fliessbach et al., 2007). They provide substantial evidence supporting the hypothesis that negative comparisons carry more weight than positive comparisons in terms of an individual's perceived job satisfaction.

Although pay secrecy rules and practices have also been criticized for enabling pay discrimination (Kim, 2013, 2015; Baker et al., 2019), Lawler (1965) posits that an open pay system fosters a strong performance-reward relationship, motivating job performance and enhancing pay satisfaction, a viewpoint supported by Futrell and Jenkins (1978). In terms of morale, when compensation is kept a secret, employees are more likely to suspect other factors such as unconscious bias, wage compression, favoritism, or discrimination, leading to disengagement (Cullen and Perez-Truglia, 2018). Conversely, increased pay transparency can mitigate such morale-related impacts. And from a monetary incentives perspective, heightened pay transparency reduces labor market frictions by reducing uncertainty regarding the expected compensation for employees' efforts, motivating them to invest more in human capital and exert greater effort (Cullen and Perez-Truglia, 2018; Hsieh et al., 2019). This implies the positive effects of pay secrecy rules and practices.

2.2.5 Pay secrecy laws

In an effort to address gender and ethnicity pay gaps, recent legal and regulatory environment in the United States has increasingly focused on promoting pay transparency within companies, making individual salaries more visible to colleagues (Trotter et al., 2017; Heisler, 2021). The intention behind pay transparency is to create a "shock of information" that empowers female employees in their negotiations with the company, enabling them to identify and take action to reduce pay gaps (Cullen and Perez-Truglia, 2018b; Cullen and Pakzad-Hurson, 2019; Kim, 2013, 2015).

The National Labor Relations Act (NLRA) of 1935 provided the first legal protection against pay secrecy, prohibiting employers from retaliating against non-supervisory employees who discuss wages or working conditions with colleagues. However, the National Labor Relations Act (NLRA) failed to address all instances in which employers prohibit or discourage wage discussions among employees, suggesting that it did not effectively resolve the issue of pay transparency (U.S. Department of Labor, 2014). Moreover, the remedies provided under the NLRA are relatively mild, limited to back wages minus any earnings from other employment, leading to widespread disregard of the law by employers (Freeman and Medoff, 1986). Consequently, pay secrecy rules and practices remain prevalent after the enactment of the NLRA, often implemented through contractual agreements and internal policies within firms (Gely and Bierman, 2003; Bierman and Gely, 2004; Edwards, 2005). Empirical evidence supporting this viewpoint is provided by Burn and Kettler (2019), who found no significant impact on the gender wage gap, job tenure, or labor supply following the implementation of the National Labor Relations Act.

In the 1980s, pay secrecy laws were introduced in several states to further protect employees from being restricted in discussing wages with coworkers and promote pay transparency (Kim, 2012). For example, Michigan passed a law in 1982 that prohibited employers from: 1) requiring as a condition of employment non-disclosure by an employee of his or her wages; 2) requiring an employee to sign a waiver or other document that purports to deny an employee the right to disclose his or her wages; or 3) discharging, formally disciplining, or otherwise discriminating against an employee for job advancement on the basis of having disclosed his or her wages. Over time, seven other states adopted similar laws, including Illinois (2003), Vermont (2005), Colorado (2009), Maine (2009), Louisiana (2013), New Jersey (2013), and Minnesota (2014) (U.S. Department of Labor, 2014). In contrast to the scope of protection provided by the National Labor Relations Act (NLRA), which exclusively safeguards non-supervisory employees, the significance of pay secrecy laws lies in their inclusion of supervisors as well (Kim, 2013).

Nevertheless, critics of pay secrecy laws contend that these regulations may incur various costs. Firstly, they argue that such laws may generate social discomfort as they challenge prevailing social norms in the United States surrounding pay secrecy. Secondly, there is a potential for increased costs to employers if a higher number of employees resort to legal action. Lastly, it is posited that when employees become aware of their co-workers' salaries, those who earn below the average may become dissatisfied, while those who earn above the average may not necessarily experience a corresponding increase in satisfaction (Card et al., 2012).

The enactment of pay secrecy laws within a state is contingent upon political dynamics encompassing factors such as legislative backing, the influence of key decision-makers, and the prevailing public sentiment concerning pay secrecy. The likelihood of state-level adoption of pay secrecy laws hinges on the relative strength of political parties in a given jurisdiction during a specific period. For instance, Maine successfully passed its pay secrecy law in 2009 following the passage of the Lilly Ledbetter Fair Pay Act and the public outrage sparked by Ledbetter's legal case, with support from Republican senators Collins and Snowe from Maine (Ramachandran, 2012; Kim, 2015).

Employers commonly implement pay secrecy policies, as they believe that restricting wage comparisons can reduce employee dissatisfaction (Colella et al., 2007; Kim, 2015). Consequently, it is unlikely that a significant number of firms would actively lobby for or influence the passage of pay secrecy laws, as managers generally prefer to maintain a non-transparent pay policy (Gao et al., 2021).

2.2.6 Empirical analysis of pay secrecy laws

Kim's (2015) research reveals that the implementation of pay secrecy laws leads to a 3% increase in total compensation for female workers and reduces the gender pay gap by over 5%, particularly among women with higher education. Baker et al. (2019) examine the influence of public sector salary disclosure laws on university faculty salaries in Canada, demonstrating their efficacy in narrowing gender pay gaps. Similarly, Bennedsen et al. (2019) investigate the impact of a 2006 legislative change in Denmark, which mandates firms to provide detailed wage information, and find a significant reduction in gender pay gaps.

Recent analysis conducted by Gao et al. (2021) also supports the notion that these laws mitigate pay gaps by 2% to 3% in the hourly wages of scientists and engineers. Moreover, the authors observe increased inventor productivity following the staggered adoption of state-level pay secrecy laws. This is attributed to factors such as reducing compensation uncertainty among incumbent minority inventors (Cullen and Perez-Truglia, 2018; Hsieh et al., 2019), attracting minority inventors from other states (Akcigit et al., 2016), and fostering greater diversity within inventor teams (Drach-Zahavy and Somech, 2001; Hong and Page, 2001; Berliant and Fujita, 2011).

Regarding executives, Mas (2017) discovers that pay disclosure in the public sector leads to an average compensation decline of approximately 7% and a significant 75% increase in the quit rate compared to managers in cities where salaries were already disclosed. Notably, wage cuts were more substantial in cities with higher initial compensation but were not observed in cities where compensation was initially disproportionate to underlying factors. This response is more indicative of public aversion to excessive compensation rather than the effects of increased accountability.

2.3. Hypothesis development

Pay secrecy laws are initially implemented with the primary objective of reducing gender and race pay gaps (Kim, 2013, 2015). These laws aim to promote pay transparency by prohibiting companies from enforcing pay secrecy rules and practices that have been criticized for contributing to pay discrimination (Kim, 2013, 2015; Cullen and Perez-Truglia, 2018; Baker et al., 2019). The effectiveness of these laws lies in their ability to create a shock of information through increased pay transparency, thereby shifting the bargaining power of female employees in favor of equitable compensation (Cullen and Perez-Truglia, 2018b; Cullen and Pakzad-Hurson, 2019). For instance, once pay information is no longer concealed, employees can identify pay gaps and take appropriate measures to reduce such disparities.

It is believed that the enhanced pay transparency resulting from the implementation of pay secrecy laws can have implications for employee incentives and behaviors, consequently influencing employee productivity, as actions taken by employees are likely to directly impact productivity (Faleye et al., 2013). From a morale perspective, keeping compensation information confidential increases the likelihood of employees suspecting other causes, such as unconscious bias, wage compression, favoritism, or discrimination, which ultimately leads to disengagement (Cullen and Perez-Truglia, 2018). In contrast, increased pay transparency can alleviate such negative effects on morale. Furthermore, from the standpoint of monetary incentives, heightened pay transparency can reduce labor market frictions by decreasing the uncertainty employees face regarding the compensation they can expect for their efforts (Hsieh et al., 2019). Moreover, companies that demonstrate improvements in gender equality following the adoption of pay secrecy laws can further enhance the productivity and retention of workers who value a fair working environment (Bennedsen et al., 2019). Consequently, increased pay transparency is expected to improve employee productivity.

<u>H1(a): Firms headquartered in states that recognize pay secrecy laws are likely to experience an increase in employee productivity following their adoption.</u>

However, advocates for increased pay transparency present counterarguments and cast doubt on its positive implications for employee productivity. They argue that wage comparisons among workers are inconclusive and lack informative value due to the presence of various challenging-to-quantify factors that extend beyond gender (Colella et al., 2007; Gely and Bierman, 2003). Consequently, employers who adopt pay secrecy policies believe that restricting wage comparisons can mitigate employee dissatisfaction (Colella et al., 2007; Kim, 2015). These perspectives suggest potential negative effects associated with pay transparency.

Supporting this perspective, based on the "fair wage-effort hypothesis" (Akerlof and Yellen, 1990), increased pay transparency resulting from pay secrecy laws may decrease the job satisfaction of lower-paid employees, while higher-paid employees may also feel threatened by attempts to mitigate inequality within the firm (Card et al., 2012; Breza et al., 2018). And according to theories of inequity aversion (Adams, 1965; Cowherd and Levine, 1992) and relative deprivation (Martin, 1981), dissatisfied employees may engage in value-destroying activities to protest salary inequity. Card et al. (2012) illustrate that increased salary transparency can reduce job satisfaction for employees with salaries both below and above the median, as individuals care not only about absolute income but also about their relative income. Consequently, workers who are unpleasantly surprised by wage comparisons might voice complaints or consider leaving their positions (Colella et al., 2007). Finally, the enforcement of pay secrecy laws may trigger the departure of esteemed and prominent innovators from their current organizations, with the possibility of being enticed by rival firms, given their remuneration that typically surpasses the mean compensation within their respective enterprises (Hitz and Werner, 2012; Kim and Marschke, 2005).

H1(b): Firms headquartered in states that recognize pay secrecy laws are likely to experience a decrease in employee productivity following their adoption.

My study aims to examine the impact of pay secrecy laws on employee productivity by emphasizing the role of pay transparency in reducing employee suspicions (Cullen and Perez-Truglia, 2018) and decreasing uncertainty regarding employees' expected compensation (Hsieh et al., 2019). Assuming that the adoption of pay secrecy laws primarily aims to narrow the gender pay gap, I anticipate a more pronounced effect on firms headquartered in states with lower levels of social capital.

Putnam (2001) provides a definition of social capital, characterizing it as the collective value derived from social networks and the norms of mutual support and reciprocity. Social capital is associated with the formation of affective bonds and connections among individuals, which have positive effects on resource acquisition and the establishment of trust within the organizational context (Adler and Kwon, 2002; Guiso, 2008). This facilitates the identification of opportunities and the allocation of scarce resources within the organization (Greene and Brown, 1997). Therefore, the concept of social capital underscores the importance of community engagement and the establishment of social networks in fostering trust, information sharing, and cooperation among individuals (Gupta et al., 2020).

Regions with high levels of social capital tend to promote cooperation, community-oriented behavior, trust among individuals, and reduced self-interest (Guiso et al., 2004). This can be attributed to the fact that deviating from social norms in these areas carries reputational consequences. In contrast, environments characterized by poor social capital can have adverse effects on employee behavior, leading to reduced commitment to the organization and an increase in opportunistic behaviors (e.g., Schutjens and Völker, 2010; Gupta et al., 2018; Habib and Hasan, 2017; Huang and Shang, 2019). Consequently, employees perceive management in states with high social capital as more trustworthy, leading to increased dedication and effort in their work (Gupta et al., 2020). Employees in such contexts are less likely to attribute compensation disparities to external factors or question the fairness of wage comparisons. They may believe that numerous difficult-to-quantify factors, beyond gender, influence salary differences, making wage comparisons inconclusive among workers (Colella et al., 2007; Gely and Bierman, 2003). Moreover, managers in states with abundant social capital are more inclined to treat employees fairly, thereby mitigating instances of pay discrimination.

In summary, regions with lower social capital tend to have higher levels of pay discrimination and gender pay gaps. I argue that the treatment effect of pay secrecy laws will be more pronounced in companies located in states with lower levels of social capital if the enhanced employee productivity resulting from the adoption of these laws is indeed a result of curbing pay secrecy practices aimed at narrowing the gender pay gap.

H2: The impact of pay secrecy laws on employee productivity is more pronounced in firms headquartered in states with relatively poorer social capital.

I further proceed to investigate the underlying mechanism connecting the adoption of pay secrecy laws with employee productivity. Pay secrecy laws are introduced with the primary objective of addressing gender and race pay gaps by prohibiting companies from enforcing pay secrecy rules. Kim (2015) presents empirical evidence demonstrating that the implementation of pay secrecy laws leads to a notable increase of 3% in total compensation for female workers and a reduction of more than 5% in the gender pay gap, particularly among women possessing college or graduate degrees. However, Duchini et al. (2020) report contrasting findings indicating that pay transparency primarily diminishes gender pay differentials through the implementation of pay compression from higher salary levels. Specifically, the introduction of pay transparency triggers a decrease in male salaries, while the translation of changes in women's occupational composition into visible salary increases remains limited. Their research further suggests that the reduction in men's salaries is attributed to nominal cuts at the upper end of the wage distribution and wage freezes in lower-paid occupations. As a result, it is widely posited that pay transparency exerts downward pressure on average employee salaries.

Within the theoretical framework of Coase's stakeholder theory (1937), organizations are expected to consider the welfare of employees, thereby motivating them to provide superior employment benefits and working conditions. As a result, employee welfare engenders heightened loyalty and subsequently contributes to enhanced productivity. Notably, explicit contractual claims, such as wages, carry legal enforceability and enjoy a hierarchical superiority over the interests of bondholders and stockholders. Furthermore, modern management theory contends that employee welfare assumes particular significance in motivating employee engagement, ultimately leading to enhanced performance and increased shareholder value. Empirical studies conducted by Levine (1992) and Wadhwani and Wall (1991) lend support to the notion that higher wages are associated with heightened productivity.

In sum, following the adoption of pay secrecy laws and increased pay transparency, employee wages can generally be decreased. Therefore, I argue that average employee salary serves as a plausible channel linking the adoption of pay secrecy laws to employee productivity.

H3: Firms headquartered in states that recognize pay secrecy laws are likely to experience a decrease in average employee salary following their adoption.

2.4. Methodology

To empirically examine the impact of staggered adoption of state-level pay secrecy laws on employee productivity in the United States, I employ a difference-in-differences estimation strategy, following the methodology outlined by Bertrand and Mullainathan (2003). This approach accounts for the presence of staggered treatments, allowing me to compare the before-after effects of pay secrecy legislation on states that experienced the change (the treatment group) with states that did not undergo such a change (the control group). By considering multiple exogenous shocks affecting different states and firms at different time points, this strategy mitigates the possibility of reverse causality, which is a common identification challenge in settings with a single shock where potential noise coincides with the shock directly impacting the dependent variable (Roberts and Whited, 2013).

My identification strategy aligns with previous studies, including Bertrand and Mullainathan (2003), Gormley and Matsa (2016), Klasa et al. (2018), Ali et al. (2019), Gao et al. (2021), and others, which have successfully employed similar approaches to draw causal inferences. In order to test my hypotheses, I conduct subsequent ordinary least squares (OLS) regressions:

$$\boldsymbol{EP}_{\,i,k,l,t} = \boldsymbol{\alpha} + \boldsymbol{\beta PSL}_{k,t-1} + +\boldsymbol{X}_{i,k,l,t-1} + \boldsymbol{\theta' FirmFE} + \boldsymbol{\varphi' IndustrybyYearFE} + \boldsymbol{\rho' StatebyYearFE} + \boldsymbol{Error}_{i,k,l,t} \qquad \textbf{(1)}$$

where $\boldsymbol{i}$ indexes firms; $\boldsymbol{k}$ indexes state of location; $\boldsymbol{l}$ indexes state of incorporation; $\boldsymbol{t}$ indexes years; $\boldsymbol{FirmFE}$, $\boldsymbol{IndustrybyYearFE}$ and $\boldsymbol{StatebyYearFE}$ are firm, four-digit-SIC industry-by-year and state-of-incorporation-by-year fixed effects respectively.

$\boldsymbol{EP}_{\boldsymbol{i,k,l,t}}$is the dependent variable of interest—employee productivity, which is EBITDA excluding incomes and expenses related to employee effort instead of dominated by managers per employee (EP_1). $\boldsymbol{PSL_{k,t-1}}$ is a dummy variable that equals one if a pay secrecy law has been passed by time $\boldsymbol{t-1}$ in state $\boldsymbol{k}$ and zero otherwise. $\boldsymbol{X_{i,k,l,t-1}}$ denotes the set of time-varying control variables. I lag all independent variables by one year to further mitigate the issue of reverse causality. Standard errors are clustered by state of location. $\boldsymbol{Error_{i,k,l,t}}$ is an error term. The coefficient of interest in this model is $\boldsymbol{\beta}$. As explained by Imbens and Wooldridge (2009), the employed-firm fixed effects lead to $\boldsymbol{\beta}$ being estimated as the within-firm 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.

The primary motivation behind the implementation of pay secrecy legislation is to address and alleviate disparities in salary based on gender or race. It is worth noting that the legislation was not originally intended to directly impact employee productivity. Therefore, the effect I are investigating in this study is likely an unintended consequence (Gao et al., 2021), which further make my utilization of the pay secrecy laws desirable.

The adoption of pay secrecy laws in a particular state is influenced by the relative strength and influence of different parties involved at a given time. Political factors, including legislative support, the presence of influential decision-makers, and public opinion on pay secrecy, play significant roles in determining the passage of such laws (Ramachandran, 2012; Kim, 2015). To account for these related factors, I incorporate location-state-by-year and industry-by-year fixed effects in my analysis. Specifically, many companies are headquartered in a state other than the one in which they are incorporated, which provides me the opportunity to include the location-state-by-year fixed effect. By including these fixed effects, I can effectively control for the potential influence of factors associated with the firm's location, industry, and year of observation, thereby enhancing the robustness of my findings. Previous studies by Gormley and Matsa (2014, 2016) have demonstrated the efficacy of using high-dimensional fixed effects in controlling for such common factors.

Employers initially implement pay secrecy rules and practices with the belief that restricting wage comparisons can help reduce employee dissatisfaction (Gely and Bierman, 2003; Bierman and Gely, 2004; Edwards, 2005; Colella et al., 2007; Kim, 2015). This suggests that the majority of firms are unlikely to actively lobby or influence the passage of pay secrecy laws, as managers generally prefer to maintain a non-transparent pay policy (Gao et al., 2021). In conclusion, the aforementioned discussion instills confidence in the validity of my chosen methodology.

2.5. Data sources and variable construction

2.5.1 Sample and data sources

The construction of my sample initiates with the compilation of a comprehensive inventory of common stocks in the United States from 1977 to 2019, encompassing data from both the Compustat Industrial files and the Center for Research in Security Prices

(CRSP) stock file. Consistent with prior studies by Gormley and Matsa (2016), Klasa et al. (2018), Ali et al. (2019), among others, the sample period encompasses a minimum of five years before and after the enactment of each law. This timeframe enables the observation of persistent effects on employee productivity resulting from these laws. The accounting data utilized in this study are sourced from the merged CRSP/Compustat database. The adoption dates of pay secrecy laws at the state level are derived from Gao et al. (2021). Information pertaining to the headquarters of the firms is obtained from Compustat database. Additionally, I exclude financial firms (SIC codes 6000–6999) and utility firms (SIC codes 4900–4999), as well as firms located or headquartered outside the United States. Furthermore, firm-year observations with missing or negative values for total assets or sales are eliminated. To address extreme values, financial ratios are winsorized at the 1% level. Overall, the sample consists of 2,259 companies headquartered in states that have adopted pay secrecy laws, and 7,857 companies headquartered in states that have not adopted such laws. Furthermore, there are 17,524 firm-year observations from states that have adopted pay secrecy laws and subsequent to their adoption.

2.5.2 Measures of employee productivity

My study aims to investigate the impact of pay transparency on employee productivity. Previous research by Cullen and Perez-Truglia (2018) and Hsieh et al. (2019) suggests that the adoption of pay secrecy laws influences the incentives and behaviors of employees. Therefore, employee productivity, which is considered a reliable indicator of employee incentives and behaviors since their actions have a direct impact on productivity (Faleye et al., 2013), should be influenced. The Bureau of Labor Statistics defines employee productivity as the ratio of employee output to employee input. I utilize the number of profits generated by each individual as a proxy for employee productivity, which serves as my dependent variable.

I employ three different measures to capture firm-level employee productivity. Firstly, following the approach of Gao et al. (2018), I utilize income before extraordinary items divided by the number of employees as a measure of productivity (EP_2). Additionally, in line with the methodology of Kale et al. (2016), I also employ EBITDA per employee as an alternative proxy for employee productivity (EP_3), which captures the cash flow value added by each employee.

Moreover, aside from enhancing output levels, increased employee effort may lead to reduced expenses or improved quality, thereby contributing to an increase in the firm's profit margin (Kale et al., 2016). To refine my measure of employee productivity, I construct my own measure, denoted as (EP_1), by dividing EBITDA, excluding incomes and incorporating expenses unrelated to employees or dominated by managers, by the number of employees. Specifically, I incorporate expenses such as income tax and depreciation and amortization, which are highly influenced by executive decisions. I exclude extraordinary items and non-operating income, as they are less likely to be influenced by employee efforts. Finally, I take the natural logarithm of all these employee productivity measures to enhance accuracy and reduce noise."

2.5.3 Adoption of pay secrecy laws

In 1935, the implementation of the National Labor Relations Act (NLRA) marked the first significant legal protection regarding pay secrecy concerns. This act aimed to safeguard non-supervisory employees from employer retaliation when discussing their wages or working conditions with colleagues. However, it did not comprehensively address all situations where employers prohibit or discourage wage discussions among employees, indicating that the NLRA did not fully resolve the issue of pay transparency (U.S. Department of Labor, 2014). Consequently, despite the NLRA, many companies continued to enforce pay secrecy rules and practices using contractual agreements and internal policies (Gely and Bierman, 2003; Bierman and Gely, 2004; Edwards, 2005). Supporting this argument, Burn and Kettler (2019) found no significant impact on the gender wage gap, job tenure, or labor supply following the enactment of the NLRA.

To further address pay secrecy and promote transparency, pay secrecy laws were introduced after the 1980s. These laws aimed to protect employees from restrictions on discussing their wages or working conditions with coworkers. For instance, in 1982, Michigan passed a law prohibiting employers from: 1) making wage non-disclosure a condition of employment for employees, 2) requiring employees to sign waivers or other documents denying their right to disclose wages, and 3) discharging, formally disciplining, or otherwise discriminating against employees who disclose their wages for career advancement purposes. Currently, a total of eight states have adopted similar pay secrecy laws, including Michigan (1982), Illinois (2003), Vermont (2005), Colorado (2009), Maine (2009), Louisiana (2013), New Jersey (2013), and Minnesota (2014) (U.S. Department of Labor, 2014). Notably, these state-level laws also cover supervisors and managers (Kim, 2013).

The applicability of pay secrecy laws is determined by the state in which companies are headquartered. To address concerns of reverse causality, the Pay Secrecy Law (PSL) indicator is lagged by one year, allowing sufficient time for companies to adjust employee productivity. Thus, the independent variable, denoted as ***PSL_1***, is a binary indicator. Specifically, ***PSL_1*** equals 1 if a pay secrecy law has been passed in an affected state by time t-1, while it remains 0 for the years preceding adoption and the year of adoption itself. For states where such laws are not in effect, the indicator variable remains 0 throughout all years (Gormley and Matsa, 2016; Klasa et al., 2018; Ali et al., 2019).

2.5.4 Control variables

Moreover, I have taken into account an extensive set of control variables, and comprehensive definitions for these variables can be found in ***Appendix B***. In my baseline regression model, I include these control variables to ensure robustness and accuracy of my analysis. These variables include: ROA (operating income before depreciation divided by lagged total assets), PPE (gross property, plant, and equipment normalized by total assets), leverage (long-term debt plus debt in current liabilities divided by lagged total assets), capital expenditures (capital expenditures normalized by lagged total assets), firm age (number of years since a firm's initial appearance in the Compustat database), SG&A (selling, general, and administrative expenses divided by lagged total assets), Dsale (annual sales growth rate), Tobin's Q (market value of equity plus the book value of total assets minus the book value of equity minus balance sheet deferred taxes, normalized by the book value of total assets), and cash holdings (cash and short-term investments normalized by total assets).

To address the potential influence of outliers, I employ a winsorization technique, which truncates extreme values of all continuous variables at the 1st and 99th percentiles. By doing so, I minimize the impact of extreme observations on my analysis. Detailed summary statistics for all variables can be found in ***TABLE 2.1***.

Table 2.1. Summary statistics

This table reports summary statistics for the 1977-2019 period. The accounting data utilized in this study are sourced from the merged CRSP/Compustat database. The adoption dates of pay secrecy laws at the state level are derived from Gao et al. (2021). I exclude financial firms (SIC codes 6000–6999) and utility firms (SIC codes 4900–4999), as well as firms located or headquartered outside the United States. Firm-year observations with missing or negative values for total assets or sales are eliminated. There are 103,771 firm-year observations for the firm-level analysis. Variable definitions are provided in Appendix B. All continuous variables are winsorized at the 1st and 99th percentiles.

Variable	n	Mean	S.D.	Min	0.250	Mdn	0.750	Max
PSL	103771	0.170	0.370	0	0	0	0	1
EP_1	103771	3.260	1.310	0.070	2.420	3.170	3.990	7.480
ROA	103771	0.040	0.110	-1.270	0.010	0.050	0.100	0.340
PPE	103771	0.550	0.380	0.010	0.260	0.470	0.760	1.890
Leverage	103771	0.240	0.210	0	0.070	0.220	0.370	0.980
Capex	103771	0.080	0.090	0	0.030	0.050	0.100	0.580
Firm age	103771	2.460	0.880	0.690	1.790	2.560	3.140	4.040
SG&A	103771	0.340	0.270	0.020	0.140	0.280	0.460	1.620
Dsale	103771	0.170	0.440	-0.790	0	0.0900	0.230	4.320
Tobin's Q	103771	1.500	1.290	0.300	0.780	1.100	1.720	11.40
Cash	103771	0.130	0.150	0	0.020	0.0700	0.190	0.930

2.6. Investigation on effects of pay transparency on employee productivity

2.6.1 Adoption of pay secrecy laws and employee productivity

The baseline regression results are summarized in ***Table 2.2***. In column (1), I present the results without including any control variables. The coefficient on the ***PSL*** indicator is -0.05461, which is statistically significant at the 1% level. This suggests a negative effect of adopting pay secrecy laws on the employee productivity. In terms of economic significance, this corresponds to a decrease of 1.68% relative to the sample mean of 3.260 for employee productivity. Moving to column (2), I report the regression results after incorporating various control variables. The coefficient on the ***PSL*** indicator is -0.05156, remaining negative and statistically significant at the 1% level, reinforcing the negative effect observed in the absence of control variables. In economic terms, this translates to a 1.58% decrease relative to the sample mean for employee productivity of 3.260. Additionally, my findings indicate that employee productivity is negatively associated with control variables such as the PPE ratio, firm age, SG&A expenses, cash holdings, and the R variable, while it is positively associated with control variables such as the ROA ratio, leverage ratio, capital expenditures, sales growth, and Tobin's Q.

My study provides novel insights by revealing that increased pay transparency is linked to lower employee productivity, even after accounting for high-dimensional fixed effects. This finding supports and contributes additional empirical evidence to existing theories. Specifically, my research contributes to a more comprehensive understanding of the impact of pay transparency by highlighting its role as a disruptive source of information that shifts bargaining power in favor of female employees, challenging the status quo within companies (Cullen and Perez-Truglia 2018b, 2019). Moreover, my study recognizes that individuals not only value their absolute income but also consider their relative income in relation to their peers (Card et al., 2012). Drawing upon the "fair wage-effort hypothesis" proposed by Akerlof and Yellen (1990), wage comparisons can significantly diminish job satisfaction for employees both below and above the median income level, potentially leading to expressions of discontent or even resignations (Colella et al., 2007). Consequently, this decrease in employee productivity can be attributed to two related theories: inequity aversion (Adams, 1965; Cowherd and Levine, 1992) and relative deprivation (Martin, 1981). These theories suggest that dissatisfied employees may resort to engaging in activities that undermine the value or success of the organization in order to address perceived salary inequities.

2.6.2 Alternative measures of employee productivity

My innovative approach to measuring employee productivity draws inspiration from previous studies. Kale et al. (2016) employed EBITDA per employee as an indicator to examine the impact of labor market dynamics on the disciplinary effect of debt on employee productivity. Gao et al. (2018) utilized income before extraordinary items per employee as a proxy to explore the relationship between employee turnover likelihood and productivity. By incorporating these measures of productivity, I aim to evaluate the robustness of my findings.

In ***Table 2.2***, columns (3), (4), (5), and (6) present the results of the OLS regression analysis. Even with the inclusion of the same control variables as our baseline regression for both supplementary measures, the coefficients associated with PSL remain consistently and significantly negative. These outcomes demonstrate the consistency and alignment observed when using the aforementioned measures introduced by Kale et al. (2016) and Gao et al. (2018), thereby further bolstering the robustness of my findings. Consequently, this contributes to the understanding of determinant factors that influence employee productivity, thereby enhancing comprehension of this domain.

Table 2.2. Effect of pay secrecy laws on employee productivity

This table reports coefficients from OLS regressions of a firm's employee productivity on the indicator for whether the firm's state of location has adopted pay secrecy laws, firm fixed effects (FE), state-of-incorporation-by-year FE, and standard industrial classification industry-by-year FE. PSL is a dummy variable that equals one if a pay secrecy law has been passed by time t-1 in state k and zero otherwise. Specifically, in column (1) and (2), the dependent variable is employee productivity measured as EBITDA removing components likely to be unrelated to employee productivity and dominated by managers divided by the number of employees (EP_1). In column (3) and (4), the dependent variable is employee productivity measured as Income before extraordinary items divided by the number of employees (EP_2). In column (5) and (6), the dependent variable is employee productivity measured as EBITDA divided by the number of employees (EP_3). The sample includes firm-year observations from 1977–2019. I winsorize continuous variables at the 1st and 99th percentiles. Variable definitions are provided in Appendix B. Standard errors are adjusted for clustering at the state of location level. T values are reported in parentheses. *** denotes significance at the 1% level; ** denotes significance at the 5% level; * denotes significance at the 10% level.

	Dependent Variable					
Independent Variables	(1) EP_1	(2) EP_1	(3) EP_2	(4) EP_2	(5) EP_3	(6) EP_3
PSL	-0.054***	-.0515 ***	-0.050**	-0.028*	-0.03979*	-0.034**
	(-2.911)	(-3.081)	(-2.013)	(-1.750)	(-1.968)	(-2.232)
ROA		2.341***		10.214***		3.569***
		(23.451)		(72.176)		(24.977)
PPE		-0.237***		-0.330***		-0.226***
		(-3.651)		(-7.103)		(-3.161)
Leverage		0.292***		-0.247***		0.268***
		(11.446)		(-7.851)		(8.953)
Capex		2.745***		-0.360***		-0.079
		(30.448)		(-8.494)		(-1.559)
Firm age		-0.027		-0.032***		-0.026*
		(-1.497)		(-3.138)		(-1.691)
SG&A		-0.946***		-1.254***		-0.838***
		(-28.817)		(-40.560)		(-29.655)
Dsale		0.012		-0.067***		0.120***
		(1.048)		(-5.735)		(7.283)
Tobin's Q		0.065***		-0.031***		0.062***
		(10.627)		(-8.304)		(9.191)
Cash		-0.004		0.296***		0.133***
		(-0.099)		(7.271)		(2.754)
Constant	3.268***	3.288***	2.221***	2.135***	3.01794***	3.047***
	(1,025.583)	(56.469)	(583.865)	(61.495)	(943.826)	(44.989)
Observations	99,939	99,939	81,953	81,953	97,853	97,853
R-squared	0.829	0.872	0.768	0.896	0.825	0.874
Company FE	YES	YES	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES	YES	YES
State-Year FE	YES	YES	YES	YES	YES	YES

2.6.3 Role of social capital

Pay secrecy laws are implemented with the goal of reducing gender and race pay gaps by prohibiting companies from enforcing pay secrecy rules (Kim, 2013, 2015). More specifically, these laws function by introducing pay transparency as a shock of information, shifting the bargaining power in favor of female or minority employees, and fostering actions to address pay differentials (Cullen and Perez-Truglia, 2018b; Cullen and Pakzad-Hurson, 2019). In other words, once pay information is no longer a secret, employees have the opportunity to identify and respond to pay disparities (Kim, 2013, 2015).

My analysis focuses on the impact of adopting pay secrecy laws on employee productivity, highlighting how increased pay transparency can influence the behavior and incentives of employees (Faleye et al., 2013). For instance, it is argued that implementing pay secrecy laws can enhance employee productivity, as it reduces suspicions of other factors such as unconscious bias, wage compression, favoritism, or discrimination that arise when compensation information is kept a secret (Cullen and Perez-Truglia, 2018). Additionally, increased pay transparency can decrease labor market frictions by reducing uncertainty regarding expected compensation, thereby influencing employee efforts (Hsieh et al., 2019).

However, it is important to note that increased pay transparency can also have negative effects on employee productivity, as employees may become dissatisfied with wage comparisons (Card et al., 2012), in line with the "fair wage-effort hypothesis" (Akerlof and Yellen, 1990). Moreover, theories of inequity aversion (Adams, 1965; Cowherd and Levine, 1992) and relative deprivation (Martin, 1981) suggest that dissatisfied employees may engage in value-destroying behaviors to address perceived salary inequities.

In this section, to further support my argument that the impact of adopting pay secrecy laws on employee productivity is related to pay transparency, which decreases pay discrimination, I introduce the concept of state-level social capital and explore potential heterogeneous treatment effects.

Gupta et al. (2020) illustrate that social capital, defined as the collective value of social networks and norms of mutual aid and reciprocity (Putnam, 2001), is based on the premise that participation in community organizations and the development of social networks foster trust, information sharing, cooperation, and reciprocity. This is because that social capital is intricately linked to the development of affective bonds and interpersonal connections among individuals, yielding favorable consequences for resource acquisition and the cultivation of trust within the organizational milieu (Adler and Kwon, 2002; Guiso, 2008). This dynamic facilitates the discernment of opportunities and the judicious allocation of limited resources within the organizational framework (Greene and Brown, 1997).

Furthermore, areas with high social capital tend to have more cooperative, community-focused, trusting, and less selfish individuals, imposing higher penalties for deviating from social norms (Guiso et al., 2004). This can be attributed to the fact that deviating from social norms in these areas carries reputational consequences. In contrast, environments characterized by low social capital can have adverse effects on employee behavior, leading to reduced commitment to the organization and an increase in opportunistic behaviors (Schutjens and Völker, 2010; Gupta et al., 2018; Habib and Hasan, 2017; Huang and Shang, 2019).

Specifically, in areas with high social capital, employees perceive management in states with high social capital as more trustworthy, leading to increased communication, cooperation, dedication and effort (Gupta et al., 2015; 2020). This aligns with Putnam's (2000) work. Employees in such contexts are less likely to attribute compensation disparities to external factors or question the fairness of wage comparisons. They may believe that various difficult-to-quantify factors, beyond gender, contribute to salary differences, making wage comparisons inconclusive among workers (Colella et al., 2007; Gely and Bierman, 2003). Moreover, managers in states with abundant social capital are more inclined to treat employees fairly, thereby mitigating instances of pay discrimination.

In summary, regions with lower social capital tend to have higher levels of pay discrimination and gender pay gaps. I argue that the treatment effect of pay secrecy laws will be more pronounced in companies located in states with lower levels of social capital if the enhanced employee productivity resulting from the adoption of these laws is indeed a result of increased pay transparency, which decreases pay discrimination.

Hence, enhanced social capital diminishes the propensity of employees to attribute pay differentials to other causes and doubt their compensation in relation to their exerted efforts. They may contend that beyond gender, an array of elusive determinants are deemed to impact salary distributions, impeding employees from quantifying them precisely, and wage comparisons among workers lack informative value (Colella et al., 2007; Gely and Bierman, 2003). Lastly, employees exhibit reduced inclination towards engaging in actions that undermine corporate value creation, as elucidated earlier.

Considering the above, I have a strong rationale to expect that the treatment effect will be more pronounced in companies with lower social capital if the improved employee productivity following the adoption of pay secrecy laws is indeed a result of restricting pay secrecy practices aimed at narrowing the gender pay gap. Pay secrecy laws apply to the state in which companies are headquartered, as firms typically locate their core business activities close to their headquarters (Howells, 1990; Pirinsky and Wang, 2006; Breschi, 2008). Hence, I estimate the level of social capital in the state where the company's headquarters are based and explore potential heterogeneous treatment effects.

Following previous research such as Alesina and La Ferrara (2000) and Guiso et al. (2004), I measure state-level social capital using voter turnout in US elections for the highest office. Higher voter turnout reflects greater civic participation and indicates a higher level of social capital in a state. Data on voter turnout is obtained from the United States Elections Project (McDonald, 2014), representing the percentage of the voting-eligible population that voted for the highest office in a given election year. The numerator in the computation refers to the count of individuals who cast their votes for the highest office during a specific election. In turn, the denominator employed in this calculation represents the count of voting-eligible population. McDonald adopts a comprehensive approach to determine the number of voting eligible people for each state. This approach involves commencing with the voting-age population and subsequently subtracting individuals deemed ineligible to vote (such as non-citizens, felons based on state regulations, and mentally incapacitated individuals). Additionally, individuals within the military or residing overseas are subsequently added back to the count. In order to enhance the robustness of my analysis, I further assess social capital by quantifying the percentage of the Voting-Age Population (VAP) that partakes in voting for the highest office during a specific election year. Further elaboration on the data and its construction can be found at http://www.electproject.org/home.

Following the approach of Gao et al. (2021), I examine the heterogeneous treatment effects of state-level pay secrecy laws on employee productivity, conditional on the 1980 state's voter turnout in US elections (two years before the first state passed a pay secrecy law). Specifically, I replicate my baseline regression by dividing the sample into the top 50 percent and bottom 50 percent based on the 1980 state's voter turnout. The results in ***Table 2.3*** are consistent with my hypothesis that the treatment effect is stronger in companies headquartered in states with relatively lower social capital if the decline in employee productivity following the adoption of pay secrecy laws is indeed attributed to restricting pay secrecy practices aiming to narrow the gender pay gap. Overall, these findings support my argument that the negative effect of pay secrecy laws on employee productivity is directly linked to pay secrecy practices and rules in the workplace, rather than being driven by unobserved heterogeneity.

Table 2.3. Cross-sectional variation in the effect of pay secrecy laws on employee productivity

This table reports coefficients from OLS regressions of a firm's employee productivity on the indicator for whether the firm's state of location has adopted pay secrecy laws, firm fixed effects (FE), state-of-incorporation-by-year FE, and standard industrial classification industry-by-year FE, using subsamples to investigate heterogeneous effects. Specifically, in column (1), I repeat my baseline regression by replacing the sample to top 50 percent of the whole sample based on the 1980 state's percentage of the voting-eligible population (VEP) that voted for the highest office in US elections. In column (2), I repeat my baseline regression by replacing the sample to bottom 50 percent of the whole sample based on the 1980 state's percentage of the voting-eligible population (VEP) that voted for the highest office in US elections. In column (3), I repeat my baseline regression by replacing the sample to top 50 percent of the whole sample based on the 1980 state's percentage of the Voting-Age Population (VAP) that voted for the highest office in US elections. In column (4), I repeat my baseline regression by replacing the sample to bottom 50 percent of the whole sample based on the 1980 state's percentage of the Voting-Age Population (VAP) that voted for the highest office in US elections. The sample includes firm-year observations from 1977–2019. I winsorize continuous variables at the 1st and 99th percentiles. Variable definitions are provided in Appendix B. Standard errors are adjusted for clustering at the state of location level. T values are reported in parentheses. *** denotes significance at the 1% level; ** denotes significance at the 5% level; * denotes significance at the 10% level.

Independent Variables	***Dependent Variable***			
	(1)	(2)	(3)	(4)
	LOW VEP	**HIGH** VEP	**LOW** VAP	**HIGH** VAP
	EP_1	EP_1	EP_1	EP_1
PSL	-0.274***	-0.014	-0.046**	-0.041
	(-3.027)	(-0.825)	(-2.272)	(-1.549)
ROA	2.057***	2.521***	2.191***	2.488***
	(11.658)	(24.523)	(13.985)	(19.022)
PPE	-0.172	-0.304***	-0.211	-0.257***
	(-1.530)	(-6.095)	(-1.726)	(-4.712)
Leverage	0.252***	0.332***	0.296***	0.262***
	(5.743)	(8.795)	(12.272)	(4.672)
Capex	2.587***	2.853***	2.640***	2.828***
	(25.427)	(38.426)	(19.518)	(33.819)
Firm age	-0.011	-0.027	-0.068**	0.008
	(-0.480)	(-0.964)	(-3.042)	(0.396)
SG&A	-0.871***	-0.972***	-0.875***	-1.007***
	(-19.183)	(-19.439)	(-26.797)	(-16.169)
Dsale	-0.007	0.045**	0.011	0.013
	(-0.585)	(2.154)	(0.568)	(0.984)
Tobin's Q	0.077***	0.054***	0.064***	0.066***
	(12.294)	(7.548)	(6.506)	(10.143)
Cash	-0.010	-0.068	-0.065	-0.015
	(-0.121)	(-1.300)	(-0.834)	(-0.265)
Constant	3.251***	3.296***	3.451***	3.162***
	(46.663)	(38.920)	(31.652)	(52.959)
Observations	42,365	52,050	47,345	47,026
R-squared	0.901	0.866	0.885	0.882
Company FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
State-Year FE	YES	YES	YES	YES

2.6.4 Adoption of pay secrecy laws and employee average salary

Drawing upon Coase's (1937) stakeholder theory, companies are expected to consider employee well-being to foster favorable employment benefits and working conditions. For example, explicit contractual claims, such as wages, hold legal binding and take precedence over the claims of bondholders and stockholders. This commitment to employee welfare not only cultivates employee loyalty but also enhances productivity levels. Moreover, modern management theory highlights the significance of employee welfare in motivating employee engagement, ultimately leading to improved performance and increased shareholder value. In this context, Levine (1992) and Wadhwani and Wall (1991) have found a positive correlation between higher wages and enhanced productivity.

However, the adoption of pay secrecy laws and increased pay transparency are believed to lead to a decline in employee wages. Kim (2015) reveals that the implementation of pay secrecy laws results in a 3% increase in total compensation for female workers and reduces the gender pay gap by over 5%, particularly among women with higher education qualifications. Duchini et al. (2020) also observe that pay transparency reduces gender pay disparities by compressing wages from the upper end. Specifically, pay transparency prompts a decrease in male salaries, while the change in women's occupational composition has yet to translate into significant salary increases. Their findings indicate that the decline in men's pay stems from nominal reductions at the higher end of the wage distribution and wage freezes in lower-paid occupations. Additionally, Mas (2017) discovers that the 2010 California mandate requiring municipal salary disclosure results in an average compensation decline of approximately 7%. In summary, it is believed that pay transparency leads to lower average employee salaries.

Consequently, it is believed that companies in states affected by pay secrecy laws tend to reduce employee salaries, which subsequently detrimentally impacts employee productivity. In other words, employee salaries can be viewed as a potential channel linking the adoption of pay secrecy laws and employee productivity. To investigate the effects of pay secrecy law adoption on average employee salary, I re-conduct my baseline regression by replacing the dependent variable, employee productivity, with average employee salary. To calculate the average pay of regular employees at the firm level, I adopt the approach utilized by Faleye et al. (2013), which involves dividing the total labor expenses reported in Compustat by the number of employees.

However, non-executive employee compensation data is not mandated to be publicly disclosed by companies. Consequently, my sample is constrained to those companies that voluntarily disclose employee compensation data in Compustat. As a result, a substantial portion of observations is omitted compared to my baseline regression sample. This discrepancy introduces potential concerns of self-selection. I address this issue by comparing my employee salary sample to my main sample in my baseline regression. ***Table 2.4*** presented below displays the industry distribution for both the restricted sample and the total sample. It is evident that while the firms included in the restricted sample exhibit higher revenue figures, employee productivity is relatively comparable to that of my total sample for each industry. Notably, the median value of employee productivity

(EP_1) in the restricted sample is 25.38, in contrast to 26.81 observed for the firms included in the baseline regression.

Table 2.4. Comparison between the sample firms of employee salary with the sample firms of the baseline regerssion

The table compares the distribution of my sample firms of employee salary with sample firms of my baseline regression over 1977–2019. Industry breakout is by one-digit SIC code, where 1 is mining and construction, 2 is consumer manufacturing, 3 is electrical and industrial manufacturing, 4 is transportation and utilities, 5 is trade, 6 is financial, 7 is commercial services, 8 is private price services and 9 is public administration.

SIC code	% of observations		Revenue MEAN		EP_1 MEDIAN	
	Sample	Total	Sample	Total	Sample	Total
1	4.1	7.7	2870.880	1017.857	4.647	4.761
2	14.2	18.3	3796.915	1505.618	3.313	3.279
3	14.2	31.2	3519.099	921.724	2.665	3.029
4	32.9	10.5	2219.323	2120.704	4.173	4.395
5	13	12.2	1519.751	1976.458	2.221	2.542
6	0	0	0	0	0	0
7	11	14.1	602.498	693.019	2.783	3.191
8	9.7	4.7	1409.588	682.374	2.806	2.690
9	0.7	1.3	5027.803	1389.103	2.7665	2.926
All	100	100	20965.861	10306.859	25.378	26.817

I present the OLS regression results in Column (1) of ***Table 2.5***, the coefficient on the PSL indicator is -0.09226, statistically significance at the 5% level and indicating a negative effect of pay secrecy law adoption on the firm's employee salary. Column (2) provides regression results after incorporating various control variables, reinforcing the robustness of my findings.

Moreover, in order to enhance the robustness of my findings, I re-conduct my baseline regression analysis using the restricted sample due to the unavailability of the non-executive employee compensation data. The empirical outcomes of this analysis are presented in Column (3) and (4) of ***Table 2.5***. The significant negative relationship between the adoption of pay secrecy laws and employee productivity still holds, after incorporating the same control variables as in my baseline regression. Although I acknowledge the limitations of my sample consisting only of firms that voluntarily disclosed employee compensation data, I believe that the employee compensation sample reasonably represents the firms included in my baseline regression. Nonetheless, I urge caution in interpreting my results, considering this caveat.

Table 2.5. Effects of pay secrecy laws on employee salary.

The Column (1) and Column (2) of this table reports coefficients from OLS regressions of a firm's average employee salary on an indicator for whether the firm's state of location has adopted pay secrecy laws. The sample includes firm-year observations from 1977–2019. The Column (3) and Column (4) of this table reports coefficients from OLS regressions of a firm's employee productivity on an indicator for whether the firm's state of location has adopted pay secrecy laws, for the restricted sample imposed by unavailability of employee productivity data from 1977–2019. I lag all independent variables by one year to mitigate the issue of reverse causality. The sample includes firm-year observations from 1991–2013. The sample includes firm-year observations from 1977–2019. I winsorize continuous variables at the 1st and 99th percentiles. Variable definitions are provided in Appendix B. Standard errors are adjusted for clustering at the state of location level. T values are reported in parentheses. *** denotes significance at the 1% level; ** denotes significance at the 5% level; * denotes significance at the 10% level.

	Dependent Variables			
Independent Variables	(1) Employee salary	(2) Employee salary	(3) EP_1	(4) EP_1
PSL	-6.428**	-7.464***	-0.128*	-0.134*
	(-2.178)	(-3.233)	(-1.752)	(-1.885)
ROA		0.717		1.298***
		-0.295		-4.951
PPE		0.573		-0.111
		-0.23		(-1.250)
Leverage		0.957		0.100
		-0.219		-0.875
Capex		0.950		2.378***
		-0.269		-13.081
Firm age		0.763		0.041
		-0.43		-0.623
SG&A		0.505		-0.859***
		-0.094		(-4.729)
Dsale		-2.828**		0.115
		(-2.321)		-1.68
Tobin's Q		0.046		0.061**
		-0.089		-2.214
Cash		26.821***		0.08
		-3.366		-0.35
Constant	40.090***	33.513***	3.379***	2.785***
	-153.4	-6.819	-543.103	-14.578
Observations	14,161	7,492	9,221	3,593
R-squared	0.879	0.899	0.942	0.966
Company FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
State-Year FE	YES	YES	YES	YES

2.7. Other robustness and diagnostic tests

2.7.1 Stacked difference-in-differences estimation

I investigate the impact of pay transparency on employee productivity through the way of introducing the adoption of pay secrecy laws to employ a difference-in-differences estimation. The pay secrecy laws are adopted staggeredly across different states in the United States. By utilizing the difference-in-differences estimation strategy, I account for the presence of staggered treatments, enabling a comparison of the pre- and post-effects of pay secrecy legislation on the states subject to the treatment (referred to as the treatment group) with those states that were not affected by such changes (referred to as the control group). This approach is necessary due to the occurrence of multiple exogenous shocks affecting different states and firms at various time points.

Through the implementation of this methodology, I effectively eliminate the potential for reverse causality between the adoption of pay secrecy laws and the level of employee productivity. In contrast, situations involving a single shock encounter a common identification challenge, as incidental noise coincides with the shock itself, which directly influences the dependent variable (Roberts and Whited, 2013).

Nonetheless, Cengiz et al. (2019) have drawn attention to potential econometric concerns associated with aggregating discrete DiD estimates using ordinary least squares (OLS), including the presence of heterogeneous treatment effects and potential negative weights assigned to specific treatments. To ensure a more accurate examination of the relationship, I additionally employ stacked difference-in-differences estimates as a robustness check. This approach allows me to analyze data derived from a staggered adoption design. The stacked DID method aims to transform the staggered adoption setting into a two-group, two-period design. In this transformed design, the difference-in-differences estimates the average effect of the treatment on the treated, taking into account the relative sizes of the group-specific datasets and the variance of treatment status within those datasets. To achieve this, separate datasets are stacked, each containing observations on treated and control units for each treatment group.

This approach uses a more stringent criteria for admissible clean control groups. Besides, by stacking and aligning events in event-time, this approach is equivalent to a setting where the events happen contemporaneously, which prevents the use of past treated units as effective comparison units. To ensure that the control group is pure, all firm-year observations that have been treated are dropped, guarding against bias due to heterogeneous treatment effects (Goodman-Bacon, 2019). Specifically, a new dataset is created for each treatment event (i.e., when a state adopted the pay secrecy law), containing all firm-year observations in a window [-5,5] that ranges from 5 years before the event to 5 years after the event. Finally, these group-specific datasets are stacked in event-time and outcomes are regressed on treatment status (the indicator variable Stacked PSL takes the value of one after the firm is treated in an event year (i.e., $\tau > 0$) in each group, and zero otherwise), fixed effects for firm by Cohort combinations and fixed effects for relative year by Cohort combinations. The standard errors are clustered by group by state.

These stacked regressions are of the form:

$$\boldsymbol{Employee\ Productivity}_{\boldsymbol{itd}} = \boldsymbol{\alpha} + \boldsymbol{\beta} \times (\boldsymbol{T}_{\boldsymbol{sd}} \times \boldsymbol{P}_{\boldsymbol{td}}) + \boldsymbol{\gamma} \times \boldsymbol{X}_{\boldsymbol{itd}} + \boldsymbol{\theta}_{\boldsymbol{sd}} + \boldsymbol{\gamma}_{\boldsymbol{td}} + \boldsymbol{\varepsilon}_{\boldsymbol{itd}} \qquad \textbf{(2)}$$

where $\boldsymbol{i}$ indexes firms; $\boldsymbol{t}$ indexes relative year to each pay secrecy law adoption; $\boldsymbol{d}$ indexes dataset group by each pay secrecy law adoption event; $\boldsymbol{Employee\ Productivity}_{\boldsymbol{itd}}$ is the dependent variable of interest. $\boldsymbol{T}_{\boldsymbol{sd}}$ is an indicator that company s is a treated unit in sub-experiment $\boldsymbol{d}$. $\boldsymbol{P}_{\boldsymbol{td}}$ is an indicator that period $\boldsymbol{t}$ is in the post period in sub-experiment $\boldsymbol{d}$. I utilize the same control variables as my baseline regression. $\boldsymbol{\theta}_{\boldsymbol{sd}}$ and $\boldsymbol{\gamma}_{\boldsymbol{td}}$ are Firm by Cohort and relative year by Cohort fixed effects respectively. $\boldsymbol{\varepsilon}_{\boldsymbol{itd}}$ is an error term. Specifically, I assign a firm’s location based on the location of its headquarters, which is typically also where major plants and operations are located (Henderson and Ono, 2008).

The coefficient on interaction term measures the impact of changes in state litigation regarding pay secrecy laws on a firm's employee productivity compared to rival companies in unaffected states. I present the estimation result in Column (1) of ***Table 2.6***. The difference-in-differences estimate is -0.09124, which is statistically significant at the 1% level, which confirms that my difference-in-differences estimates are not sensitive to heterogeneous treatment effects.

Table 2.6. Stacked difference-in-differences estimation and dynamic difference-in-differences estimation

Column (1) of this table reports results from stacked OLS difference-in-differences estimation of employee productivity on the indicator for the adoption of pay secrecy laws, by focusing on a window that contains the five years before and after the adoption of pay secrecy laws (and dropping states that ever-adopted pay secrecy laws). Column (2) and (3) report coefficients from OLS regressions of a firm's employee productivity on series of indicators for the timing of states passing pay secrecy laws, firm fixed effects (FE), state-of-incorporation-by-year FE, and standard industrial classification industry-by-year FE. The sample includes firm-year observations from 1977–2019. I winsorize continuous variables at the 1st and 99th percentiles. Variable definitions are provided in Appendix B. Standard errors are adjusted for clustering at the state of location level. T values are reported in parentheses. *** denotes significance at the 1% level; ** denotes significance at the 5% level; * denotes significance at the 10% level.

	Dependent Variables		
Independent Variables	(1) EP_1	(2) EP_1	(3) EP_1
Stacked PSL	-0.091***	-	-
	(-2.984)	-	-
PSL Passage^{-3}	-	0.007	-0.017
	-	(0.250)	(-0.324)
PSL Passage^{-2}	-	0.048	-0.020
	-	(1.074)	(-0.399)
PSL Passage^{-1}	-	-0.051	-0.081
	-	(-1.516)	(-1.583)
PSL Passage^{0}	-	-0.005	-0.069
	-	(-0.122)	(-1.166)
PSL Passage^{+1}	-	0.001	-0.003
	-	(0.030)	(-0.072)
PSL Passage^{+2}	-	-0.049*	-0.043
	-	(-1.802)	(-0.860)
PSL Passage^{+3+}	-	-0.095***	-0.089*
	-	(-3.038)	(-1.876)
PPE	-0.202***		-0.324***
	(-4.771)		(-9.808)
Leverage	0.198***		-0.109***
	(4.929)		(-3.136)
Capex	2.595***		2.810***
	(15.323)		(48.122)
Firm age	0.011		-0.006
	(0.764)		(-0.413)
SG&A	-0.921***		-0.938***
	(-12.774)		(-23.449)
Dsale	0.044**		0.051***
	(2.177)		(4.379)
Tobin's Q	0.072***		0.141***
	(19.640)		(25.928)
Constant	3.403***	3.267***	3.301***
	(81.891)	(1,940.696)	(68.766)
Observations	153,243	104,517	83,499
R-squared	0.901	0.852	0.875
Company FE	-	YES	YES
Industry-Year FE	-	YES	YES
State-Year FE	-	YES	YES
Company-Cohort FE	YES	-	-
Event-Year-Cohort FE	YES	-	-

2.7.2 Dynamic difference-in-differences estimation

The fundamental identification assumption underlying the difference-in-differences methodology employed in my baseline regression is that, in the absence of the law, companies of treatment group and control group would exhibit similar trends. Specifically, in my instance, I expect the change in employee productivity levels for firms headquartered in states that adopted pay secrecy laws to be equivalent to that of firms in states that have not adopted such laws. Consequently, in this section, I study the timing of changes in level of employee productivity relative to the timing of adoptions of the pay secrecy laws to examine whether my sample conforms to the aforementioned assumption. If reverse causality drives my results, I should observe a declining trend in level of employee productivity for firms located in states affected by pay secrecy laws prior to the implementation of said laws.

To test the pre-treatment trends in employee productivity for both the treated firms and control firms, following Klasa et al. (2018), Ali et al. (2019) and Gao et al. (2021), I re-estimate my baseline regression by replacing the PSL indicator with seven indicator variables: $PSL\ Passage^{-3}$, $PSL\ Passage^{-2}$, $PSL\ Passage^{-1}$, $PSL\ Passage^{0}$, $PSL\ Passage^{+1}$, $PSL\ Passage^{+2}$ and $PSL\ Passage^{+3+}$. The key variables of interest are $PSL\ Passage^{-3}$, $PSL\ Passage^{-2}$, $PSL\ Passage^{-1}$, $PSL\ Passage^{0}$, $PSL\ Passage^{+1}$, $PSL\ Passage^{+2}$ and $PSL\ Passage^{+3}$. Specifically, $PSL\ Passage^{-3}$, $PSL\ Passage^{-2}$, $PSL\ Passage^{-1}$, $PSL\ Passage^{0}$, $PSL\ Passage^{+1}$, $PSL\ Passage^{+2}$ and $PSL\ Passage^{+3+}$ are equal to one if the firm is headquartered in a state that will pass the pay secrecy laws in three, two years and one year, in that year, adopted the pay secrecy laws one year ago, adopted the pay secrecy laws two and three, and zero otherwise. At the end points, $PSL\ Passage^{+3+}$ equals one for all years that are three or more years after pay secrecy laws' adoption, and zero otherwise.

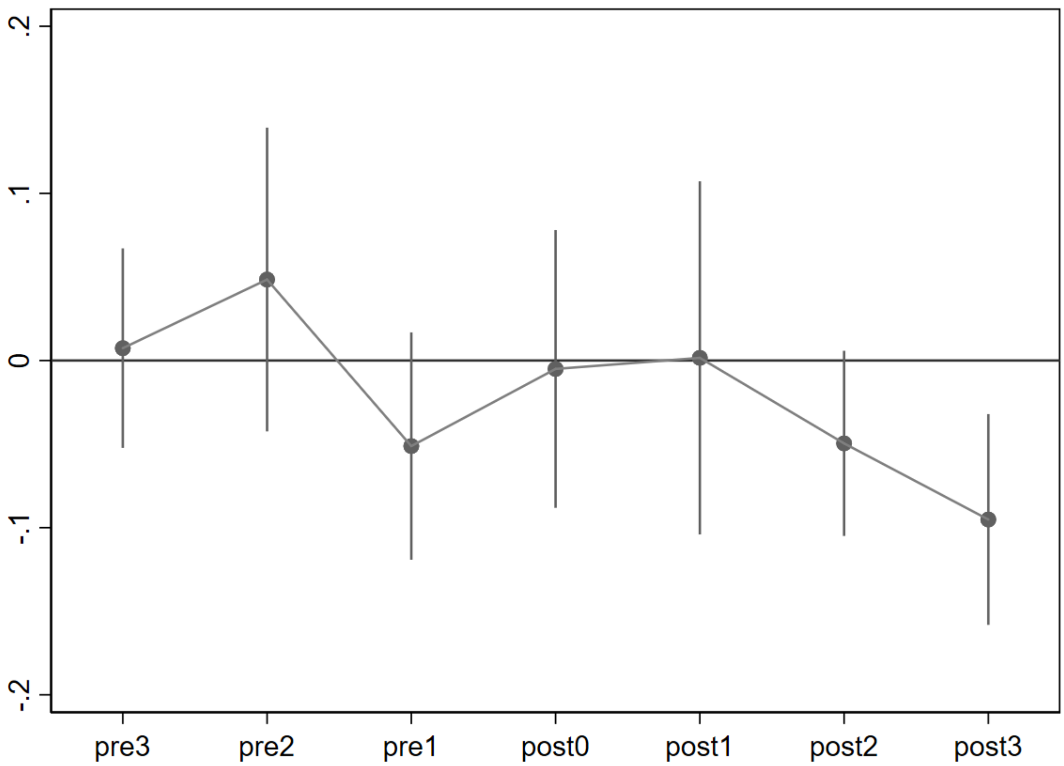
I present the dynamic difference-in-differences estimation result in Column (2) and (3) of ***Table 2.6***. Specifically, I place my focus on the coefficients on the indicators $PSL\ Passage^{-3}$, $PSL\ Passage^{-2}$, $PSL\ Passage^{-1}$, since their magnitude and significance indicate whether there exist disparities in employee productivity between treated firms and their control firms prior to the implementation of pay secrecy laws. My results reveal that the coefficients on the pre-event indicators do not exhibit statistical significance in the presented settings, satisfying the requirement that pre-event period coefficients should not yield significant results. This indicates that firms in states that adopted pay secrecy laws do not experience a decline in employee productivity relative to control firms until after the enactment of such laws. Consequently, I conclude that there are no discernible differences between the treatment group and the control group prior to the adoption of pay secrecy laws, thereby supporting the validity of the parallel trend assumption inherent in the difference-in-differences approach (Roberts and Whited, 2013).

In comparison to the pre-treatment years, I observe a decreasing effect of pay secrecy laws on employee productivity two years subsequent to their implementation. I show that the coefficients on the $PSL\ Passage^{0}$ and $PSL\ Passage^{+1}$ demonstrate small magnitudes and lack statistical significance for both settings. However, the coefficient for $CS\ Passage^{+2}$ displays a negative and statistically significant effect at the 10% level, while the coefficient for $CS\ Passage^{+3+}$ exhibits a negative and statistically significant effect at the 1% level. Taken together, these results align with the notion that the influence of pay secrecy laws on employee productivity may require an extended period to manifest and demonstrate a lasting impact following their adoption. Thus, my findings provide additional evidence that the negative effect of pay secrecy laws on employee productivity is not driven by reverse causality and lend support to a causal relationship.

Additionally, a graphical representation is included to visually depict the dynamic impact of adopting pay secrecy laws on employee productivity. ***Graph 2.1*** serves to illustrate that treatment and control companies demonstrate statistically similar trends leading up to the implementation of pay secrecy laws. Moreover, it reveals a decline in employee productivity one year after the introduction of such laws, with evidence of a sustained effect over time. These empirical findings alleviate concerns pertaining to reverse causality and substantiate a causal relationship.

Graph 2.1. Dynamic difference-in-differences regression

This graph reports results from OLS regressions of employee productivity on a series of indicators for the timing of states passing pay secrecy law, which reflects the dynamic effects of pay secrecy laws. The confidence interval is 95%. The sample spans 1977–2019. The key variables of interest are pre 3, pre 2, pre 1, post 0, post 1, post 2 and post 3, which are equal to one are equal to one if the firm is incorporated in a state that will pass the CS laws in three, two years and one year, in that year, adopted the CS laws one year ago, adopted the CS laws two and three or more years ago, and zero otherwise.



2.7.3 Propensity Score Matching (PSM) Analysis

The potential for bias arises when differences in outcomes between treated and untreated groups are influenced by a factor that predicts treatment rather than the treatment itself. In randomized experiments, randomization ensures unbiased estimation of treatment effects by balancing treatment groups on average across all covariates, as governed by the law of large numbers. However, in observational studies, treatment assignment is typically non-random, leading to the need for methods that mitigate treatment assignment bias and emulate randomization.

To address the self-selection bias stemming from firm-related characteristics that could impact my results, I employ Propensity Score Matching (PSM) analysis by creating a sample of units that received the treatment that is comparable on all observed covariates to a sample of units that did not receive the treatment. PSM is a statistical technique that aims to estimate the effect of a treatment or intervention by accounting for covariates that predict receiving the treatment. By reducing bias caused by confounding variables, PSM enhances the accuracy of estimating treatment effects when comparing outcomes between treated and control units (Rosenbaum and Rubin, 1983).

Specifically, in my study, I compare the employee productivity of firms headquartered in states that adopted pay secrecy laws to those headquartered in states that did not adopt such laws but are otherwise similar. The treatment group consists of firms in states that adopted pay secrecy laws, while the control group comprises firms in states that did not adopt such laws. For each year, I match treatment firms with control firms based on firm characteristics used as control variables in my baseline regression model. I estimate the probability of being assigned to the treatment or control group using a logit regression that includes all control variables, year, state of headquarters, and industry fixed effects, consistent with my baseline regression. Subsequently, I utilize the propensity scores derived from this logit estimation to perform matching within a caliper of 0.01, without replacement.

In Panel A of ***Table 2.7***, I present the firm characteristics of my treatment and control samples. I observe that the firm characteristics in both groups are similar for most of the control variables employed in the matching process. Panel B of ***Table 2.7*** presents the results obtained from estimation using the PSM. Specifically, the coefficient estimate for the variable PSL is -0.055, which is statistically significant at the 10% level. This finding emphasizes that my results are not driven by systematic differences between firms with high and low levels of pay transparency. Overall, the results obtained after accounting for sample selection bias through the application of the PSM method support my baseline findings.

Table 2.7. Propensity Score Matching (PSM) test

This table reports coefficients from OLS regressions of a firm's employee productivity on indicator for the passing pay secrecy laws, firm fixed effects (FE), state-of-incorporation-by-year FE, and standard industrial classification industry-by-year FE using propensity score matching (PSM) approach. Panel A shows the results of the comparison of the characteristics of the treatment and control firms. Panel B presents the results of the impact of pay secrecy laws' passage on employee productivity based on the matched sample. The sample includes firm-year observations from 1977–2019. I winsorize continuous variables at the 1st and 99th percentiles. Variable definitions are provided in Appendix B. Standard errors are adjusted for clustering at the state of location level. T values are reported in parentheses. *** denotes significance at the 1% level; ** denotes significance at the 5% level; * denotes significance at the 10% level.

Panel A: Descriptive statistics for the matched sample			
Dependent variable	Treatment Firms	Control Firms	t-test
EP_1	3.323	2391	6.50 ***
Control Variables	Treatment Firms	Control Firms	t-test
ROA	0.043	0. 048	-1.26
PPE	0. 514	0. 499	0.91
Leverage	0. 239	0. 221	1.83
Capex	0. 078	0. 079	-1.46
Firm age	2.448	2.441	0.74
SG&A	0. 348	0. 371	-1.44
Dsale	0. 164	0. 168	-1.06
Tobin's Q	1.518	1.646	-0.93
Cash	0. 140	0. 160	-1.14

Panel B: PSM Regression Analysis	
Dependent variable	EP_1
PSL	-0.035*
	(-2.209)
ROA	2.853***
	(18.215)
PPE	-0.182**
	(-2.622)
Leverage	0.349***
	(3.708)
Capex	2.490***
	(16.710)
Firm age	-0.061***
	(-3.676)
SG&A	-1.004***
	(-25.902)
Dsale	0.047
	(1.035)
Tobin's Q	0.052***
	(6.973)
Cash	-0.145**
	(-2.591)
Constant	3.491***
	(76.156)
Observations	17,941
R-squared	0.905
Company FE	YES
Industry-Year FE	YES
State-Year FE	YES

2.7.4 Placebo test

I further perform placebo experiments by simulating fictitious alterations in the pay secrecy laws preceding the actual legal changes in states affected by these modifications. By introducing these placebo PSL indicator variables, I re-estimate the difference-in-differences regression models. Specifically, I generate fictitious variations in the PSL that occur 3 and 5 years prior to the real changes in the PSL within each state undergoing law changes.

If I posit that the impact on employee productivity can be attributed to and causally linked with the adoption of pay secrecy laws, I would expect not to observe a significant relationship between employee productivity and the randomly assigned passage of pay secrecy laws in these placebo experiments. The OLS regression results, as depicted below in ***Table 2.8***, indicate that although the coefficients on the placebo PSL indicators exhibit negative values, they lack statistical significance. Moreover, the magnitude of these coefficients diminishes as I move further away from the actual law change, suggesting a decaying effect.

Table 2.8. Placebo test

This table report coefficients from OLS regressions of a firm's employee productivity on indicator of fictitious changes in the pay secrecy laws, firm fixed effects (FE), state-of-incorporation-by-year FE, and standard industrial classification industry-by-year FE. The sample includes firm-year observations from 1977–2019. I winsorize continuous variables at the 1st and 99th percentiles. Variable definitions are provided in Appendix B. Standard errors are adjusted for clustering at the state of location level. T values are reported in parentheses. *** denotes significance at the 1% level; ** denotes significance at the 5% level; * denotes significance at the 10% level.

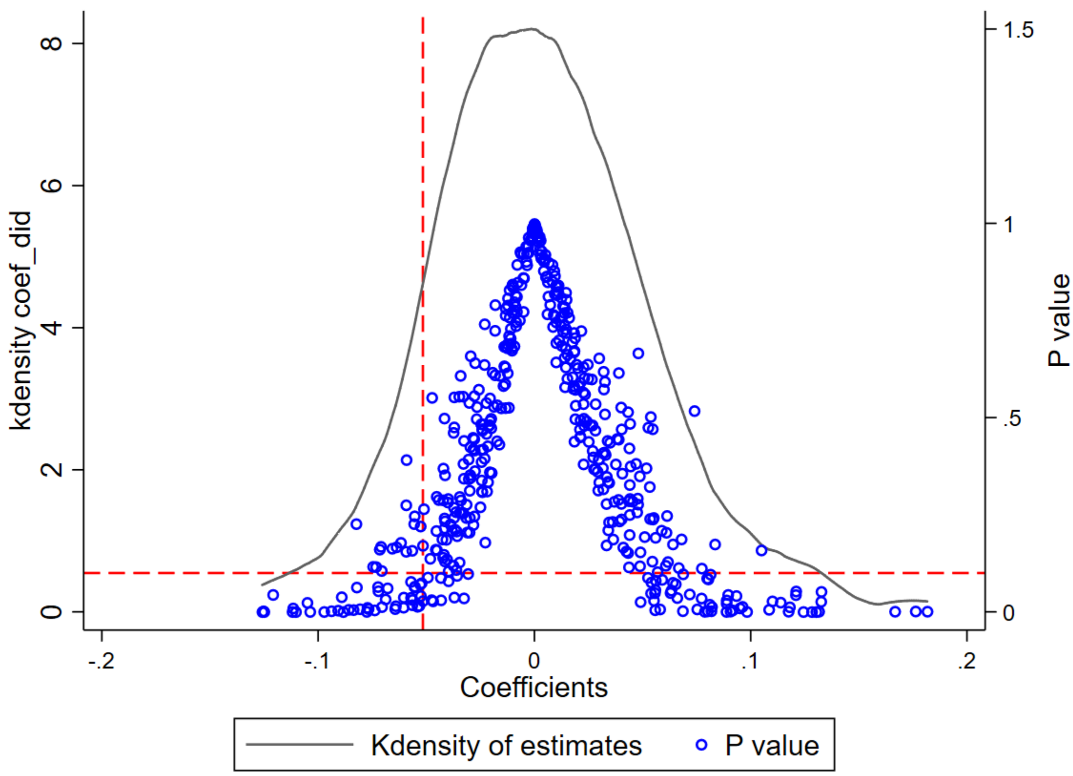
Independent Variables	***Dependent Variables***	
	(1) 5 Years Before the PSL adoption EP_1	(2) 3 Years Before the PSL adoption EP_1
Placebo PSL	-0.037	-0.043
	(-1.395)	(-1.396)
ROA	2.362***	2.364***
	(22.815)	(23.112)
PPE	-0.228***	-0.233***
	(-3.625)	(-3.650)
Leverage	0.270***	0.288***
	(10.065)	(11.287)
Capex	2.734***	2.742***
	(31.728)	(32.074)
Firm age	-0.022	-0.024
	(-1.152)	(-1.246)
SG&A	-0.925***	-0.939***
	(-26.108)	(-27.887)
Dsale	0.008	0.010
	(0.748)	(0.983)
Tobin's Q	0.065***	0.066***
	(11.267)	(11.119)
Cash	-0.002	-0.010
	(-0.049)	(-0.246)
Constant	3.213***	3.249***
	(51.733)	(54.283)
Observations	93,011	96,553
R-squared	0.869	0.870
Company FE	YES	YES
Industry-Year FE	YES	YES
State-Year FE	YES	YES

Additionally, it has been demonstrated by Bertrand et al. (2004) that difference-in-differences analyses conducted over extended time series can potentially result in an overestimation of t-statistics and significance levels when there is correlation among observations within each unit. To mitigate the possibility of spurious results, following the approach outlined by Bertrand et al. (2004) and Guo and Masulis (2015), I perform a placebo test. This test involves randomly assigning the adoption of pay secrecy laws to states, ensuring an equal probability of adoption for each state and thereby ensuring that any differences observed between and within states are not systematic.

Specifically, for each year in which one or multiple states adopt pay secrecy laws, I randomly designate an equal number of states as the pseudo-treatment group, while the remaining states serve as the control group. I then estimate baseline regressions based on these pseudo-treatment states, saving the coefficient estimates for pseudo adoption of pay secrecy laws. This procedure is repeated 1000 times. The results, as presented in ***Graph 2.2*** below, reveal that the coefficients obtained from the pseudo-regressions follow a normal distribution with a mean of 0. Furthermore, the corresponding P-values are predominantly greater than 0.1. Notably, the vertical dotted line in the figure represents the actual regression coefficient obtained from the main regression analysis presented in column (2) of ***Table 2.2***, which falls within the tail of the overall distribution of pseudo-regression coefficients. Taken together, these findings provide evidence that the relationship between the adoption of pay secrecy laws and employee productivity, as documented in my primary tests, is unlikely to be spurious.

Graph 2.2. Distribution of coefficients of placebo test

This table reports the distribution of coefficients from OLS regressions of employee productivity on the indicator for fictitious passage of pay secrecy law for 1000 times. The placebo test is conducted by randomly assigning states passing the pay secrecy laws, which ensures that each state has the same chance to adopt the pay secrecy laws and thus guarantees that any difference between and within states is not systematic. The horizontal axis represents the coefficients of the regression result, and the vertical axis represents the corresponding P values. The vertical dotted line in the figure is the real regression coefficient obtained in the main regression presented above, shown in column (2) of Table 2.2.



2.7.5 Other robustness tests

In this section, I elucidate three comprehensive robustness assessments conducted to validate the veracity of my principal discoveries. First, earlier research conducted by Gao et al. (2018) also investigates the impact of state-level staggered adoption of Inevitable Disclosure Doctrine (IDD) on employee productivity, the authors argue that the IDD may alter employees' incentives to work for the firm. On the one hand, the IDD might motivate employees to exert greater effort in order to retain and excel in their current positions, as it limits their options for alternative employment opportunities. On the other hand, it could potentially diminish their incentives to perform optimally, as it disrupts the labor market and hinders the fair valuation of employees' human capital.

To address this concern and minimize the possibility of omitted variable bias, I include additional controls for the adoption and rejection of the IDD in my analysis. Out of the total 50 states in the US, 21 states adopted the IDD during my sample period, while three states that had previously adopted the IDD subsequently rejected it. Following the methodology employed by Klasa et al. (2018), I create an indicator variable, IDD, to capture the presence of the IDD. For the 21 states where the courts implemented the IDD, this indicator is set to zero for all years prior to the adoption date and to one for the year of adoption and subsequent years. For the remaining 29 states that neither explicitly adopted nor repealed the IDD, I set the IDD indicator to zero.

By integrating the IDD indicator into my baseline regression analysis, as reported in columns (1) and (2) of ***Table 2.9***, the coefficients on PSL are still significantly negative at the 5% level and my findings indicate either a null effect or a negative effect of the IDD on employees' productivity. Thus, I assert the robustness of my results even when considering the existence of the IDD.

Secondly, in order to conduct a robustness test, I exclude companies headquartered in Louisiana, New Jersey, and Minnesota from my sample as these states implemented pay secrecy laws towards the end of my data collection period, as suggested by Gao et al. (2021). Subsequently, I re-estimated my baseline regression using the revised sample excluding the aforementioned states. The results of these analyses are presented in Column (3) and Column (4) of ***Table 2.9*** below. Notably, the coefficients associated with PSL remain significantly negative at the 5% significance level, providing further confirmation of the robustness of my findings.

Finally, taking cues from Mas (2017), who shed light on the effects of the 2010 California mandate requiring the disclosure of municipal salaries on compensation reductions and turnover among top administrators, I also exclude companies headquartered in California. Following this adjustment, I re-conducted the baseline regression using the updated sample without California-based companies. The corresponding results are displayed in Column (5) and Column (6) of ***Table 2.9***. Remarkably, the coefficients pertaining to PSL remain significantly negative, thus reinforcing the robustness of my findings. Collectively, these robustness tests instill greater confidence in the validity of my primary conclusions.

Table 2.9. Other robustness tests

The Column (1) and Column (2) of this table reports coefficients from OLS regressions of a firm's employee productivity on an indicator for whether the firm's state of location has adopted pay secrecy laws, adding IDD indicator as additional control. The Column (3) and Column (4) of this table reports coefficients from OLS regressions of a firm's employee productivity on an indicator for whether the firm's state of location has adopted pay secrecy laws, excluding firms headquartered in states adopted the laws around the end of my sample period. The Column (5) and Column (6) of this table reports coefficients from OLS regressions of a firm's employee productivity on an indicator for whether the firm's state of location has adopted pay secrecy laws, excluding firms headquartered in California. The sample includes firm-year observations from 1977–2019. I lag all independent variables by one year to mitigate the issue of reverse causality. I winsorize continuous variables at the 1st and 99th percentiles. Variable definitions are provided in Appendix B. Standard errors are adjusted for clustering at the state of location level. T values are reported in parentheses. *** denotes significance at the 1% level; ** denotes significance at the 5% level; * denotes significance at the 10% level.

	Dependent Variables					
Independent Variables	(1) EP_1	(2) EP_1	(3) EP_1	(4) EP_1	(5) EP_1	(6) EP_1
PSL	-0.037**	-0.037**	-0.039**	-0.037**	-0.051**	-0.046*
	(-2.391)	(-2.345)	(-2.288)	(-2.089)	(-2.074)	(-1.864)
IDD	-0.001	-0.016				
	(-0.047)	(-0.760)				
ROA		2.341***		2.306***		2.361***
		(23.458)		(22.741)		(18.828)
PPE		-0.237***		-0.222***		-0.215***
		(-3.659)		(-3.297)		(-3.117)
Leverage		0.292***		0.286***		0.289***
		(11.488)		(10.358)		(10.099)
Capex		2.746***		2.742***		2.696***
		(30.359)		(28.348)		(30.091)
Firm age		-0.027		-0.027		-0.012
		(-1.500)		(-1.380)		(-0.765)
SG&A		-0.947***		-0.948***		-0.969***
		(-28.677)		(-25.143)		(-24.832)
Dsale		0.012		0.013		0.004
		(1.056)		(1.084)		(0.582)
Tobin's Q		0.065***		0.067***		0.073***
		(10.629)		(10.218)		(15.496)
Cash		-0.004		-0.016		-0.001
		(-0.099)		(-0.349)		(-0.028)
Constant	3.342***	3.296***	3.357***	3.294***	3.333***	3.220***
	(338.579)	(51.980)	(1,169.296)	(53.913)	(2,435.679)	(67.445)
Observations	124,061	99,939	113,440	91,085	107,778	86,403
R-squared	0.829	0.872	113,440	91,085	107,778	86,403
Company FE	YES	YES	0.831	0.874	0.843	0.882
Industry-Year FE	YES	YES	YES	YES	YES	YES
State-Year FE	YES	YES	YES	YES	YES	YES

2.8. Conclusion

In this study, I aim to establish a causal relationship between pay transparency and employee productivity. In order to enhance accuracy and mitigate confounding variables, my investigation introduces an innovative proxy measure for employee productivity. To capture employee productivity more accurately, encompassing all staff within a company, this measure is derived by dividing EBITDA, excluding incomes and incorporating expenses that are unrelated to employees or predominantly influenced by managerial factors, by the total number of employees. my analysis utilizes a panel of U.S. public firms spanning from 1977 to 2019 and employs a difference-in-differences methodology, leveraging the staggered adoption of pay secrecy laws to address endogeneity concerns. This approach allows me to mitigate the challenges of isolating exogenous variation in the pay of the relevant peer group.

My novel findings reveal that increased pay transparency is associated with lower employee productivity, even after controlling for high-dimensional fixed effects. The findings remain robust and in alignment with existing scholarly works when alternative metrics of employee productivity are employed (Kale et al., 2016; Gao et al., 2018). This supports the notion that wage comparisons can lead to decreased job satisfaction. Given that the primary intention of these legislation was not to influence employee productivity, this effect is most likely an unintentional consequence, which further make my utilization of the pay secrecy laws desirable. Notably, I observe that the decreasing effect on productivity becomes evident two years after the enactment of pay secrecy laws, providing further evidence against reverse causality concerns.

I further conduct a series of diagnostic and robustness tests to eliminate alternative explanations. My analysis demonstrates that the pre-treatment trends in employee productivity are indistinguishable between the treatment and control groups. Additionally, I perform a placebo test by randomly assigning states to adopt pay secrecy laws, effectively ruling out chance-driven outcomes. To address potential self-selection bias arising from firm-related characteristics that could affect my results, I employ a Propensity Score Matching (PSM) test. For each year, I match treatment firms with control firms based on the firm characteristics used as control variables in my baseline regression model. The results, after accounting for sample selection bias using the PSM method, align with my baseline findings, underscoring that my results are not driven by systematic differences between firms with varying levels of pay transparency. Furthermore, I apply stacked difference-in-differences estimates as a robustness check, mitigating the influence of heterogeneous treatment effects and avoiding potential negative weights of specific treatments. I find consistent results, confirming that my difference-in-differences estimates are not sensitive to heterogeneous treatment effects. These additional analyses consistently support a causal interpretation of my main findings, indicating that pay secrecy laws have a negative impact on firm-level employee productivity. Moreover, I employ alternative measures of employee productivity, control for other state-level laws, and extend the sample period to enhance the robustness of my findings.

I find that the effects of pay secrecy laws are more pronounced in firms headquartered in states with lower social capital, further supporting my claim that the impact of pay secrecy laws on employee productivity is linked to the restriction of pay secrecy practices and rules aimed at narrowing the gender pay gap. Additionally, I observe that following the adoption of pay secrecy laws, average employee salaries decrease. This finding helps explain why employees in companies headquartered in states with pay secrecy laws exhibit reduced productivity.

My study sheds light on the need to investigate the real effects of widely implemented pay secrecy rules and practices, which aim to limit wage comparisons and decrease employee dissatisfaction within companies, but have also been implicated in perpetuating pay discrimination (Kim, 2013, 2015; Cullen and Perez-Truglia, 2018; Baker et al., 2019). Furthermore, my research holds important policy implications. While nine states have already adopted pay secrecy laws, the remaining states are still deliberating whether to follow suit. my study contributes to the existing body of research on the economic impact of pay secrecy laws and deepens my understanding of these laws. However, there remains ample opportunity to investigate the broader effects of pay secrecy laws on other outcomes and further advance my comprehension of pay transparency for future studies.

Appendix A: List of States Legislating Pay Secrecy Laws

Information is provided by the U. S. Department of Labor (Permanent Link: https: //hdl. handle. net/ 1813/78735)

State	Pass year	Details
Michigan	1982	Mich. Comp. Laws Section 408.483a Prohibited conduct. Sec. 13a. (1) An employer shall not do any of the following: (a) Require as a condition of employment nondisclosure by an employee of his or her wages. (b) Require an employee to sign a waiver or other document which purports to deny an employee the right to disclose his or her wages. (c) Discharge, formally discipline, or otherwise discriminate against for job advancement an employee who discloses his or her wages. This provision was added to Act 390 of 1978, Payment of Wages and Fringe Benefits, by Act 524 of 1982, effective March 30,1983.
California	1984	Labor Code, Section 232 "No employer may do any of the following: a. Require, as a condition of employment, that an employee refrain from disclosing the amount of his or her wages. b. Require an employee to sign a waiver or other document that purports to deny the employee the right to disclose the amount of his or her wages. c. Discharge, formally discipline, or otherwise discriminate against an employee who discloses the amount of his or her wages. "
Illinois	2003	ST CH 820 § 112/10 Sec. 10. Prohibited Acts. (b) It is unlawful for any employer to interfere with, restrain, or deny the exercise of or the attempt to exercise any right provided under this Act [Equal Pay Act of 2003]. It is unlawful for any employer to discharge or in any other manner discriminate against any individual for inquiring about, disclosing, comparing, or otherwise discussing the employee's wages or the wages of any other employee, or aiding or encouraging any person to exercise his or her rights under this Act.

Vermont	2005	Title 21 (Labor) , Chapter 5 (Employment Practices) , Sec. 495 (Unlawful Employment Practices) . Sec. 495(a) It shall be unlawful employment practice, except where a bona fide occupational qualification requires persons of a particular race, color, religion, national origin, sex, sexual orientation, gender identity, ancestry, place of birth, age, or physical or mental condition: (7) (B) (i) No employer may do any of the following: (I) Require, as a condition of employment, that an employee refrain from disclosing the amount of his or her wages or from inquiring about or discussing the wages of other employees. (II) Require an employee to sign a waiver or other document that purports to deny the employee the right to disclose the amount of his or her wages or to inquire about or discuss the wages of other employees. (ii) Unless otherwise required by law, an employer may prohibit a human resources manager from disclosing the wages of other employees. (8) Retaliation prohibited. An employer, employment agency, or labor organization shall not discharge or in any other manner discriminate against any employee because the employee: (D) has disclosed his or her wages or has inquired about or discussed the wages of other employees.
Maine	2009	Chapter 29, S. P. 33 - L. D. 84, An Act to Ensure Fair Pay, effective 9/12/09 Sec. 1.26 MRSA Sec. 628, first paragraph, as amended by PL 2001, c. 304, Sec. 2, is further amended to read: "An employer may not discriminate between employees in the same establishment on the basis of sex by paying wages to any employee in any occupation in this State at a rate less than the rate at which the employer pays any employee of the opposite sex for comparable work on jobs that have comparable requirements relating to skill, effort and responsibility. Differentials that are paid pursuant to established seniority systems or merit increase systems or difference in the shift or time of the day worked that do not discriminate on the basis of sex are not within this prohibition. An employer may not discharge or discriminate against any employee by reason of any action taken by such employee to invoke or assist in any manner the enforcement of this section. An employer may not prohibit an employee from disclosing the employee's own wages or from inquiring about another employee's wages if the purpose of the disclosure or inquiry is to enforce the rights granted by this section. Nothing in this section creates an obligation to disclose wages. "

Colorado	2009	Senate Bill 08-122, approved 4/17/08 Sec. 1.24-34-402(1) , Colorado Revised Statutes, is amended BY THE ADDITION OF A NEW PARAGRAPH to read: 24-34-402. Discriminatory or unfair employment practices. (1) It shall be a discriminatory or unfair employment practice: (i) unless otherwise permitted by federal law, for an employer to discharge, discipline, discriminate against, coerce, intimidate, threaten, or interfere with any employee or other person because the employee inquired about, disclosed, compared, or otherwise discussed the employee's wages; to require as a condition of employment nondisclosure by an employee of his or her wages; or to require an employee to sign a waiver or other document that purports to deny an employee the right to disclose his or her wage information. this paragraph (i) shall not apply to employers who are exempt from the provisions of the 'national labor relations act, ' 29 u. s. c. sec. 151 et seq.
Louisiana	2013	Chapter 6- A (Louisiana Equal Pay for Women Act) of Title 23 of the Louisiana Revised Statutes of 1950 §664. Prohibited acts D. It shall be unlawful for an employer to interfere with, restrain, or deny the exercise of, or attempt to exercise, any right provided under this Chapter. It shall be unlawful for any employer to discriminate, retaliate, or take any adverse employment action, including but not limited to termination or in any other manner discriminate against any employee for inquiring about, disclosing, comparing, or otherwise discussing the employee's wages or the wages of any other employee, or aiding or encouraging any other employee to exercise his or her rights under this Chapter. Note: This Act applies only to any department, office, division, agency, commission, board, committee or other organizational unit of the state.

New Jersey	2013	Title 10. Civil Rights Sec. 10:5-12. Unlawful employment practices, discrimination. 11. It shall be an unlawful employment practice, or, as the case may be, an unlawful discrimination: r. For any employer to take reprisals against any employee for requesting from any other employee or former employee of the employer information regarding the job title, occupational category, and rate of compensation, including benefits, of any employee or former employee of the employer, or the gender, race, ethnicity, military status, or national origin of any employee or former employee of the employer, regardless of whether the request was responded to, if the purpose of the request for the information was to assist in investigating the possibility of the occurrence of, or in taking of legal action regarding, potential discriminatory treatment concerning pay, compensation, bonuses, other compensation, or benefits. Nothing in this subsection shall be construed to require an employee to disclose such information about the employee herself to any other employee or former employee of the employer or to any authorized representative of the other employee or former employee.
Minnesota	2014	Ch. 239-H. F. No. 2536 Article 3. Labor Standards and Wages Sec. 2. [181.172] WAGE DISCLOSURE PROTECTION. (a) An employer shall not: (1) require nondisclosure by an employee of his or her wages as a condition of employment; (2) require an employee to sign a waiver or other document which purports to deny an employee the right to disclose the employee's wages; or (3) take any adverse employment action against an employee for disclosing the employee's own wages or discussing another employee's wages which have been disclosed voluntarily. (b) Nothing in this section shall be construed to: (1) create an obligation on any employer or employee to disclose wages; (2) permit an employee, without the written consent of the employer, to disclose proprietary information, trade secret information, or information that is otherwise subject to a legal privilege or protected by law; (3) diminish any existing rights under the National Labor Relations Act under United States Code, title 29; or (4) permit the employee to disclose wage information of other employees to a competitor of their employer. (c) An employer that provides an employee handbook to its

		employees must include in the handbook notice of employee rights and remedies under this section. (d) An employer may not retaliate against an employee for asserting rights or remedies under this section. (e) An employee may bring a civil action against an employer for a violation of paragraph (a) or (d) . If a court finds that an employer has violated paragraph (a) or (d) , the court may order reinstatement, back pay, restoration of lost service credit, if appropriate, and the expungement of any related adverse records of an employee who was the subject of the violation.

Appendix B: Variable definition

Variable	*Definition*
Measures of employee productivity	
EP_1	Income before extraordinary items divided by the number of employees, which removes any components likely to be unrelated to employee productivity and dominated by managers ((ebitda - ni + ib +capx-nopi)/emp).
EP_2	Income before extraordinary items divided by the number of employees (ib /emp).
EP_3	EBITDA per employee (ebitda /emp).
Measure of adoption of pay secrecy laws	
PSL	A dummy variable that equals one if a pay secrecy law has been passed by time in state and zero otherwise.
Measures of control variables	
Employee average salary	Total labor expenses reported in Compustat divided by the number of employees (emp)
Social capital	Percentage of voting eligible population that voted for the highest office in a given election year. The numerator is the number of people who voted for the "highest office" in a given election. The denominator is the voting-eligible population, defined as the number of people eligible to vote. (Source: www.electproject.org/home)
ROA	Income before extraordinary items normalized (ib) by lagged total assets (at).
PPE	Gross property, plant, & equipment (ppegt) normalized by total assets.
Leverage	Total debt (dltt+dlc) normalized by total assets (at).
Capex	Capital Expenditures (capx) normalized by total assets (at).
Firm age	Number of years since a firm's first appearance in the Compustat database.
SG&A	Selling, general, and administrative expenses (xsga) divided by lagged total assets (at).
Dsale	Annual sales (sale) growth rate from year t-1 to year t.
Tobin's Q	Market value of equity plus the book value of total assets minus the book value of equity minus balance sheet deferred taxes, normalized by the book value of total assets.
Cash	Cash and short-term investments (che) normalized by total assets (at).
IDD	A dummy variable that equals one if Inevitable Disclosure Doctrine has been adopted by time in state and zero otherwise.

Chapter 3. EXECUTIVE MOBILITY AND INSTITUTIONAL OWNERSHIP: EVIDENCE FROM THE INEVITABLE DISCLOUSURE DOCTORINE

Abstract

This research employs a difference-in-differences framework to examine the consequences of restricted executive mobility on institutional shareholding. It introduces the staggered recognition of the Inevitable Disclosure Doctrine (IDD), which imposes stricter limitations on managerial mobility in order to protect trade secrets. This study represents the first attempt to provide evidence that, on average, the recognition of IDD leads to a decline in equity holdings among institutional investors. This effect is primarily driven by reduced corporate governance, as indicated by agency costs. Notably, activist and long-term institutions, among various classifications of institutional investors, exhibit a significant sensitivity to the constraint on executive mobility, reinforcing their heightened monitoring incentives. Moreover, my research presents empirical evidence that the impact of IDD recognition on institutions is more pronounced in industries characterized by a knowledge-intensive focus, highlighting the influence of executive mobility on institutional shareholding due to trade secret protection. These findings underscore the motivations of institutional investors to target portfolio companies, aiming to mitigate monitoring costs and fulfill their fiduciary responsibilities. Finally, my study ensures the robustness of the results through a comprehensive range of diagnostic and robustness tests.

3.1. Introduction

The prevalence of pension assets has led to a significant increase in institutional shareholding (Sias and Starks, 1998). Given their substantial holdings in equities, institutional investors are more susceptible to declines in stock returns, necessitating a cautious approach to avoid negative price impact and exercise prudence (Del Guercio, 1996; Hawley and Williams, 2000; Parrino et al., 2003). As a result, they are motivated to allocate resources to enhance their ability to gather information, thereby making them more likely to possess informed insights about the firm's future prospects for monitoring purposes (Hartzell and Starks, 2003; Parrino et al., 2003).

In the realm of corporate governance, institutional investors actively engage in the oversight of companies in which they possess shares, employing a combination of "voice" and "exit" strategies to exercise their influence (Levit, 2013; Edmans, 2014). The concept of "voice" encompasses a range of direct intervention measures, including both formal mechanisms such as proxy voting to challenge management positions and the submission of shareholder proposals, as well as informal channels such as engaging in correspondence with the board and maintaining regular communication with portfolio firm management (Shleifer and Vishny, 1986; Maug, 1998; Harris and Raviv, 2010; Levit and Malenko, 2011). Through the execution of these actions, institutional investors articulate their dissatisfaction with management, be it through private or public means (Holderness and Sheehan, 1985; Barclay and Holderness, 1991; Bethel et al., 1998; Brav et al., 2008; Klein and Zur, 2009).

While the concept of "exit" entails the act of selling shares or the potential threat thereof

(Parrino et al., 2003; Admati and Pfleiderer, 2009; Edmans, 2009; Edmans and Manso, 2011). Companies with inadequate corporate governance practices display a lack of prudence, which goes against the fiduciary responsibility owed to institutional investors. Supporting this perspective, Parrino et al. (2003) observe that such companies often diminish or eliminate dividends and experience heightened stock price volatility. Consequently, institutional investors tend to manifest their discontent with corporate governance by divesting from underperforming companies. This trend is reflected in a notable decline in the holding periods of institutional investors, with the U.S. Census Bureau (2000) reporting a decrease to approximately two years, raising significant concerns.

Previous research has predominantly focused on investigating whether institutional investors engage in monitoring activities and their subsequent outcomes, with a particular emphasis on activist institutional investors. However, only a restricted body of literature has presented empirical evidence pertaining to the actions taken by institutional investors in response to dissatisfaction with corporate management. Furthermore, there exists a varied comprehension regarding the degree to which corporate governance quality significantly affects different types of institutional investors. Hence, the objective of this study is to examine the impact of restrictions on executive mobility, resulting from the staggered implementation of the Inevitable Disclosure Doctrine (IDD) across different states in the United States, on institutional shareholding.

Restricted executive mobility imposes higher costs on managers whose current positions are at risk, leading to heightened career concerns and enhanced incentives to engage in opportunistic activities that could improve their current employer's perception of their abilities (Kothari et al., 2009; Gao et al., 2018; Ali and Li, 2019). Additionally, limitations on managers' external employment options result in a decrease in the pool of potential replacement CEOs. Consequently, companies face challenges in identifying and recruiting more qualified CEOs to replace incumbents, thus necessitating the retention of the current CEO (Grande-Herrera, 2019). This disruption to the labor market discipline mechanism fosters the occurrence of executive opportunistic behaviors (Li et al., 2017; Kim et al., 2020; Ali and Li, 2019; Li et al., 2018; Gao et al., 2018; Islam et al., 2020; Na, 2020). Therefore, it is reasonable to posit that restricted executive mobility undermines corporate governance quality.

As demonstrated earlier, companies may seek enhanced monitoring through an increase in institutional shareholding to bolster their corporate governance, considering the monitoring role of institutional investors, following more restricted executive mobility (Chung and Zhang, 2011). Alternatively, institutional investors must balance their monitoring costs with the benefits derived from such activities and exercise prudence. Thus, corporate governance quality influenced by opportunistic activities serves as a crucial criterion in the targeting mechanism of institutional investors (McCahery et al., 2010).

The IDD is a legal doctrine through which an employee may be enjoined from joining a new job or forming a competing company if it can be demonstrated that the employee's new responsibilities will inevitably involve the disclosure, utilization, or reliance on trade secrets of the former employer, specifically for firms headquartered in the relevant state (Kahnke et al., 2013). Since the enforcement of the IDD is not contingent on the type of employee contract, location of future rival firms, or the presence of non-compete agreements, the enforcement of the IDD directly and effectively restricts potential outside employment options for knowledgeable employees. Since employees who possess valuable trade information are more likely to leave their current companies (Coff, 1997; Ganco et al., 2015; Kacperczyk, 2012; Kacperczyk and Balachandran, 2018) in order to bring their valuable expertise to competing firms or establish new ventures, the IDD limits the mobility of managers who typically have greater access to their firm's trade secrets (Ali et al., 2019). Consequently, this restriction leads to a certain degree of decline in corporate governance quality.

I thus utilise the staggered recognition of the IDD and rejections of the previously adopted IDD by U.S. state courts and a large panel of firm-years over a period of almost 40 years, to employ a difference-in-differences design as natural experiments in order to detect the relationship between executive mobility and institutional shareholding. Leveraging a difference-in-differences design, I capitalize on the presence of multiple shocks affecting different firms at various points in time. Specifically, I compare the before-after effects of IDD changes in states where IDD were implemented (the treatment group) with those in states where no such changes occurred (the control group) (Gormley and Matsa, 2016; Klasa et al., 2018; Ali et al., 2019). This methodology enables me to address potential biases associated with the timing of the laws (Bertrand and Mullainathan, 2003) and circumvent alternative explanations that could arise in settings with a single shock, where contemporaneous events may drive my findings (Roberts and Whited, 2013).

More specifically, the court's recognition or rejection of the IDD is independent of firm-specific characteristics and does not aim to restrict institutional shareholding. Unlike the enactment of state laws, which can be influenced by lobbying and political pressures that may lead to reverse causation, the recognition and rejection of the IDD is determined through court rulings on specific landmark cases that establish precedents for future legal proceedings. Since a court's ruling on a major IDD case primarily depends on the nature of the case and the characteristics of the justices involved, the court decisions regarding the IDD can be considered exogenous to the decision-making processes of firms and shareholders.

Even though, my analysis incorporates both location-state-by-year and industry-by-year fixed effects, providing greater confidence in the robustness of my findings. Specifically, given that many companies are headquartered in states different from the ones in which they are incorporated, I have the opportunity to include the location-state-by-year fixed effect to account for this factor. By incorporating these high-dimensional fixed effects, I can mitigate concerns related to unobserved sources of heterogeneity that may be associated with the industry, location, or observation year of the firms (Gormley and Matsa, 2016).

My findings indicate that firms headquartered in states that have recognized the IDD experience an average decrease in institutional shareholding of 2% compared to the mean shareholding during the sample period. This suggests that institutional investors tend to sell shares when they are dissatisfied with executive management. To address concerns of reverse causality, I incorporate a one-year lag for the IDD indicator in my baseline regression model, allowing sufficient time for affected companies to adjust their institutional shareholding. Importantly, I observe that the decreasing effect occurs two years after the passage of the IDD, further mitigating concerns of reverse causality.

In addition, I delve into a more detailed investigation of the specific categories of institutional investors that are significantly influenced by corporate governance quality. Following the categorization by Bushee (1998) based on expected investment horizon, I classify institutional investors as either "long-term" or "short-term" institutions. I also consider the fiduciary standard, distinguishing between banks, insurance companies, investment advisers (including mutual fund companies), and pensions and endowments, as outlined by Bushee (2001). my analysis further distinguishes activist investors among banks, insurance companies, investment advisers (including mutual fund companies), and pensions and endowments based on the fiduciary standard. Activist institutions, as distinguished from passive institutions, actively engage in buying or selling shares (exit) to influence managerial decisions (Appel et al., 2016). Active investors, according to Del Guercio and Hawkins (1999), tend to exhibit higher turnover rates as they actively participate in corporate governance and shape firm policies (e.g., Aghion et al., 2013; Brav et al., 2008). Conversely, passive institutions typically have a limited channel for engagement, primarily relying on "voice" mechanisms due to their close alignment with benchmark portfolio weights. Consistent with my hypothesis, my findings highlight that among the classifications of institutional investors, activist and long-term institutions exhibit significant influence.

Cengiz et al. (2019) have highlighted the potential econometric challenges that may arise when integrating discrete difference-in-differences (DiD) estimates using the ordinary least squares (OLS) method. To address these concerns and mitigate the influence of heterogeneous treatment effects and negative weights associated with specific treatments, I adopt a stacked difference-in-differences (DID) estimation approach. This methodology has been previously employed by Gormley and Matsa (2011), Deshpande and Li (2019), and Cengiz et al. (2019). The consistent finding supports the robustness and validity of the original DiD estimates.

I subsequently undertake a series of tests to validate the accuracy and robustness of my difference-in-differences (DiD) analysis. The fundamental assumption underlying the difference-in-differences methodology is that, in the absence of the law, treatment and control groups of firms would exhibit parallel trends. Specifically, in my case, I expect the change in institutional ownership levels of firms located in states that have recognized the IDD to be similar to that of companies located in states that have not recognized the IDD. I demonstrate that the pre-treatment trends in institutional shareholding align between these two groups of firms.

Additionally, I conduct a placebo test by randomly assigning states to recognize the IDD, ensuring equal probabilities of IDD recognition across all states and minimizing systematic differences. my results reveal that the actual coefficient estimate from the baseline regression lies well to the right of the distribution of coefficient estimates from 1000 simulations, providing strong evidence that my findings are not a result of chance.

To address potential biases arising from self-selection of firms, I employ a Propensity Score Matching (PSM) test. This test matches treatment firms with control firms based on firm characteristics used as control variables in my baseline regression for each year. The results of the PSM test corroborate my baseline findings, confirming that my conclusions are not driven by systematic differences between firms that have implemented the IDD and those that have not.

Finally, I conduct additional robustness tests to further ensure the reliability of my main results. I adopt an alternative list of states implementing the IDD, based on the research conducted by Qiu and Wang (2018), and find that my findings remain consistent, demonstrating that the IDD adoption exert a negative impact on institutional shareholding. I provide supplementary evidence supporting the notion that the effect of the IDD on institutional shareholding is tied to the protection of trade secrets, as I observe a stronger effect in companies headquartered in states with greater knowledge-focused investments. Furthermore, I explore the relationship between the implementation of the IDD and agency costs, shedding light on the mechanism through which institutional investors withdraw due to poor corporate governance. my results indicate that firms based in affected states exhibit lower-quality corporate governance after the implementation of the

IDD, aligning with my expectations.

This study contributes to the existing literature by providing empirical evidence that institutional investors take corporate governance into consideration when targeting companies to hold shares. Specifically, my findings demonstrate that institutional shareholding decreases when executive mobility is restricted due to a state's recognition of the Inevitable Disclosure Doctrine (IDD), resulting from opportunistic behaviors by managers who face increased job loss costs. Prior research has examined various effects related to executive mobility restrictions, such as executive bargaining power (Grande-Herrera, 2019), the sensitivity of CEO pay to systematic performance (Na, 2020), earnings management (Gao et al., 2018), agency costs (Islam et al., 2020), asymmetric withholding of bad news (Ali and Li, 2019), and tax avoidance (Li et al., 2018). Therefore, my study extends the literature by examining the outcomes associated with restricted executive mobility.

Furthermore, my research expands the understanding of the economic effects of the IDD. Previous studies have focused on its impact on employee mobility (Png and Samila, 2013), agency problem (Qiu and Wang, 2018), Venture Capital (Castellaneta et al., 2016), innovation (Contigiani et al., 2018), stock price (Liu and Ni, 2019), disclosure quality (Ali et al., 2019), mergers and acquisitions (Chen et al., 2018; Dey and White, 2019), CSR (Flammer and Kacperczyk, 2019), capital structure (Klasa et al., 2018). However, there is limited evidence regarding the consequence of the IDD on ownership structure, and my study is the first to investigate its unintended impact on institutional ownership.

Moreover, existing literature has explored how institutional investors engage in corporate governance through exit or voice strategies. Some investors choose to sell their shares when dissatisfied with management (Parrino et al., 2003; Admati and Pfleiderer, 2009; Edmans, 2009), while others voice their concerns privately or publicly (Holderness and Sheehan, 1985; Barclay and Holderness, 1991; Bethel et al., 1998; Brav et al., 2008; Klein and Zur, 2009). my study contributes to understanding the criteria institutional investors employ when targeting companies and provides evidence that activist and long-term institutional investors tend to sell their shares when dissatisfied with executive management. Additionally, my research complements studies that examine managers' efforts to attract specific types of investors (e.g., Bushee and Miller, 2012; Karolyi and Liao, 2015).

Lastly, this study has implications for policy. While approximately 20 out of the 50 US states have adopted the IDD (see the *data and methodology part* for the list of states that recognized the IDD), policymakers in many other states are still debating its adoption, partially due to a lack of understanding of its economic effects. By shedding light on the consequences of the IDD, my study offers insights that can inform policy discussions surrounding its implementation.

The subsequent sections of this paper follow a structured organization: Section 3.2 offers a comprehensive overview of the pertinent literature, providing a foundation for the study. Building upon this foundation, Section 3.3 delves into the development of hypothesis.

Section 3.4 succinctly outlines the primary methodology employed in the analysis, highlighting the approach taken to address the research questions. The detailed account of the data used and the process of variable construction is presented in Section 3.5. In Section 3.6, the paper unveils and thoroughly discusses the key findings derived from the analysis. Section 3.7 sheds light on further investigations conducted to enhance the robustness of the essay. Ultimately, Section 3.8 serves as the concluding section of the paper, summarizing the main outcomes, emphasizing their significance, and proposing potential avenues for future research.

3.2. Literature review

3.2.1 Executive mobility

3.2.1.1 Executive mobility and managerial opportunism

Restricted mobility of executives leads to a decrease in the size of the replacement executive pool, which is a crucial institution in the executive labor market (Hermalin and Weisbach, 2017). This pool comprises individuals with the necessary qualifications, institutional expertise, social networks, and availability, including current executives, members of internal and external senior management teams, staff members, and other specialists, who are potential future executives eligible for selection by the board of directors. Despite the thorough understanding of the company and its internal workings possessed by internally appointed executives, companies are increasingly opting to hire outsider CEOs (Zajac, 1990; Parrino, 1997; Farrell and Whidbee, 2003; Graham et al., 2018). These external candidates offer unique information, skills, and networks that prove especially valuable in businesses requiring a fresh perspective or structural changes. Supporting this perspective, Helmich (1974) and Helmich and Brown (1972) demonstrate that firms experience higher rates of growth and organizational change when selecting an outsider as the new CEO.

Consequently, when executive mobility is restricted, companies have fewer opportunities to identify and hire high-performing outsider CEOs as replacements but rather prefer to retain the current CEO (Grande-Herrera, 2019). Murphy and Z´abojn´ık (2007) show that in contexts with a relatively elastic supply of CEOs, boards tend to prioritize external managerial abilities. This heightened emphasis on external talent translates into increased compensation for external CEOs, as they are believed to bring added value to the firm. However, if this is the case, the disciplinary mechanism of the labor market may be compromised, leading to opportunistic behavior by executives (Li et al., 2017; Kim et al., 2020; Ali and Li, 2019; Li et al., 2018; Gao et al., 2018; Islam et al., 2020; Na, 2020).

Moreover, existing executives are less likely to receive job offers from other competing companies (Gao et al., 2015). For managers whose jobs are at risk, restrictions on outside employment opportunities increase the costs associated with job loss. Furthermore, these restrictions heighten career concerns among managers, thereby enhancing their incentive to take risks and engage in opportunistic activities that could positively influence their current employer's assessment of their abilities (Kothari et al., 2009; Gao et al., 2018; Ali and Li, 2019). In situations where uncertainty exists regarding managers' abilities, the labor market evaluates them based on corporate performance (e.g., Gibbons and Murphy, 1992; Hermalin and Weisbach, 1998).

3.2.2.2 Influence of restricted executive mobility

Previous studies have extensively examined the effects of restricted executive mobility. Grande-Herrera (2019) argues that limitations on managers' outside employment opportunities result in increased bargaining power for executives. They find that a decrease in the pool of potential replacement CEOs leads to longer CEO tenure, lower forced turnover, and higher compensation for incumbent CEOs. Furthermore, at the firm level, these restrictions lead to lower CEO-firm matches, reduced firm efficiency, lower performance, and higher over-investment. Na (2020) reveals that increased executive mobility positively affects the sensitivity of CEO pay to systematic performance, indicating that firms link CEO compensation to systematic performance in order to retain talent and ensure continued participation.

Gao et al. (2018) investigate the effects of mobility among key employees on earnings management. They find that reduced turnover likelihood among key employees with access to trade secrets significantly decreases the occurrence of upward earnings management. To illustrate it, companies provide employees with a package of claims, consisting of both explicit and implicit components. The explicit claim refers to a clear employment contract, while the implicit component represents a tacit promise of long-term working conditions, continued employment, and opportunities for career development (Cornell and Shapiro, 1987). Consequently, companies make long-term income-increasing accounting choices to safeguard financial security, thereby reducing the expected cost of hiring and retaining key employees by elevating the value of their implicit claims (Bowen et al., 1995; Burgstahler and Dichev, 1997; Matsumoto, 2002; Cheng and Warfield, 2005). Exogenous shocks that reduce turnover likelihood among employees increase the expected cost of hiring and retaining employees, subsequently decreasing managers' incentives to manipulate earnings upward.

Islam et al. (2020) explore the relationship between manager mobility restrictions and agency costs. They argue that strengthened executive mobility restrictions amplify executive career concerns and intensify conflicts in risk preferences between well-diversified shareholders and undiversified managers (Hölmstrom 1999). This refers to risk-related agency conflicts and leads to distortions in corporate financing decisions.

Ali and Li (2019) find that executive mobility positively influences the asymmetric withholding of bad news. They demonstrate that career concerns serve as a channel linking executive mobility to voluntary corporate disclosure, with both upside and downside career concerns having opposite effects. In general, managers tend to withhold more bad news than good news due to career concerns (e.g., Kothari et al., 2009). However, Ali and Li (2019) differentiate between managers' concerns about the possibility of termination (downside career concern) and their concerns about the possibility of promotion in the external labor market (upside career concern). They argue that for managers whose jobs are at risk, restrictions on outside employment opportunities exacerbate their incentive to withhold bad news. However, for managers seeking promotional opportunities in the external market, the restrictions on outside employment opportunities are likely to have an opposite effect on their incentives to withhold bad news. The incentives that these managers would otherwise have to hide bad news in the hope of landing a better job elsewhere are suppressed since external promotional opportunities are now restricted. Despite the differing effects of upside and downside career concerns, Ali and Li's (2019) findings indicate that, on average, firms increase the asymmetric withholding of bad news relative to good news when executive mobility is restricted. This aligns with the evidence provided by Glaeser (2018), which demonstrates the negative effects of restricted mobility on corporate transparency.

Similarly, Li et al. (2018) establish a link between restricted executive mobility and corporate tax avoidance, finding a positive relationship. Under agency conflicts, managers weigh the benefits and costs of tax avoidance to determine the optimal level of avoidance from their perspective. When the cost of job loss increases, managers tend to increase their incentives for tax avoidance, performance improvement, and influencing their current employer's assessment of their abilities (Gao et al., 2018). However, restricted executive mobility reduces opportunities for managers with better outside job prospects, subsequently decreasing their incentives for tax savings, performance improvement, and influencing the assessment of their abilities by external employers.

3.2.2.3 IDD (Inevitable Disclosure Doctrine) and executive mobility

The Inevitable Disclosure Doctrine (IDD) serves as a protective measure for safeguarding trade secrets, with its primary aim being to restrict employee mobility (Kahnke et al., 2013). More precisely, employers possess the authority to petition state courts for measures that limit an employee's ability to work for a competitor, including imposing restrictions on their responsibilities or even prohibiting their involvement in establishing a rival company. Notably, executives typically enjoy greater access to the firm's trade secrets (Ali et al., 2019). Furthermore, it is common for employees who acquire significant trade information to depart from their current organizations (Coff, 1997; Ganco et al., 2015; Kacperczyk, 2012; Kacperczyk and Balachandran, 2018), thereby bringing their valuable expertise to competing enterprises. Consequently, the IDD places more stringent constraints on the employment mobility of executives. Supporting this perspective, Castellaneta et al. (2017) provide evidence that the escalating litigation risk faced by managers who possess trade secrets and proprietary information can diminish both the availability and desirability of labor-market opportunities.

The IDD operates at the state level, originating from the Uniform Trade Secrets Act (UTSA), which defines trade secrets as any information that (i) derives independent actual or potential economic value, from not being generally known to and not being readily ascertainable by proper means by outside parties who can obtain economic value from its disclosure or use, and (ii) is the subject of efforts that are reasonable under the circumstances to maintain its secrecy. For example, trade secrets can encompass a wide range of information, such as customer details, pricing, cost information, future business plans, formulas, practices, processes, and designs.

Trade secrets play a vital role in various industries, offering businesses a competitive advantage over their rivals. Notably, a federal court ruling in 2011, where Kolon Industries Inc. was ordered to pay DuPont Co. $919.9 million for the misappropriation of 149 trade secrets related to DuPont's Kevlar business, highlights the significant value attached to these secrets. Hall et al. (2012) estimate an average value of $6.3 million per trade secret based on this ruling. Furthermore, a recent survey by Marsh and Liberty Underwriters found that trade secrets generate the most revenue among different types of intellectual property, surpassing trademarks and patents. Similar findings were reported in earlier studies by Cohen et al. (2000) and Arundel (2001).

The IDD is applicable if "threatened misappropriation" happens that does not immediately follow the general principles in trade secrets law, as codified in the UTSA. Misappropriation refers to the acquisition or disclosure of trade secrets through improper means or without consent from individuals bound by a duty to maintain secrecy. Under the IDD, a firm can pursue legal action based on the potential harm it may suffer. To secure an injunction, the firm must establish three key elements: (i) the employee had access to its trade secrets, (ii) the employee's responsibilities at the new employer would inevitably lead to the use or disclosure of those trade secrets, and (iii) such disclosure would result in irreparable economic harm to the firm. Importantly, the firm is not required to prove actual wrongdoing by the employee or divulge specific details of the trade secrets during the lawsuit.

It is important to note that legal disputes arising from employment contracts generally fall within the realm of employment law, and thus the jurisdiction for lawsuits seeking to protect a firm's trade secrets when employees switch employers is typically determined by the state where the former employee worked (Malsberger, 2004; Garmaise, 2011). The state of incorporation of the former or new employer, as well as the employee's state of residence, are not significant factors when applying the IDD. Consequently, the IDD offers protection for a firm's trade secrets even if the new employer operates in a state that has not adopted the IDD. This recognition of the IDD contributes to reducing the likelihood of turnover among crucial employees who possess trade secrets (Seaman 2015).

Employment contracts often include a nondisclosure agreement (NDA) and/or a covenant not to compete (CNC) as additional measures to protect trade secrets in situations where employees intend to switch jobs or establish competing companies. NDAs, however, have limitations as they require the detection and proof of violations before legal action can be initiated against a former employee. Moreover, by the time violations are established, the harm caused by the breach may have already transpired. CNCs are most effective when employees seek job transitions within the same state Garmaise (2011), as the restrictions on competing with the former employer are typically confined to specific geographic areas (Malsberger, 2004).

The IDD provides significant additional protection of a firm’s trade secrets even if the firm’s employees sign NDAs and/or CNCs. First, it does not entail specific geographic restrictions, and thus it is more far-reaching than CNCs. Second, it increases the enforceability of NDAs and CNCs. For instance, it allows courts to prohibit an individual’s employment at a rival firm if this would inevitably lead to a future violation of an NDA, i.e., before an actual violation is detected. The IDD is also a powerful means to establish a key element in any legal action to enforce a CNC, i.e., a significant likelihood of irreparable harm to the firm if the employee is allowed to work for the rival. Finally, the IDD allows courts to grant an injunction even if the employee did not sign an NDA or CNC with the former employer, i.e., solely on the basis that disclosure of the trade secrets is inevitable.

3.2.2 Institutional investors

3.2.2.1 Role of institutional investors

Institutional investors, due to their substantial equity positions, are primarily influenced by reductions in stock return, necessitating their avoidance of price impact as a prudent approach (Del Guercio, 1996; Hawley and Williams, 2000; Parrino et al., 2003). Supporting this perspective, institutional investors demonstrate a preference for stocks characterized by lower return volatility (Huang, 2009), as heightened stock return volatility can amplify perceived firm riskiness and subsequently raise the cost of capital (Froot et al., 1992). Similarly, other studies indicate that institutional investors favor stocks of companies with higher market liquidity (Badrinath et al., 1996; Falkenstein, 1996), greater average trading volumes (Falkenstein, 1996; Gompers and Metrick, 1998), stocks of companies that distribute cash dividends or engage in share repurchases (Grinstein and Michaely, 2005), stocks of larger companies (Gompers and Metrick, 2001), and stocks with low distress risk (Ye et al., 2019).

Consequently, institutional investors are motivated to allocate resources towards enhancing their information-gathering capabilities, thereby increasing the likelihood of being well-informed about the prospects of the firms they monitor (Hartzell and Starks, 2003; Parrino et al., 2003). Shareholders with significant equity positions exhibit a greater inclination to assume a monitoring role compared to atomistic shareholders, as the associated benefits are more likely to outweigh the costs incurred, leading to effective control of exit costs (Grossman and Hart, 1980; Shleifer and Vishny, 1986; Huddart, 1993).

Thus, institutional investors display a preference for stocks of companies demonstrating superior managerial performance (Parrino et al., 2003) and higher quality of corporate governance (Chung and Zhang, 2011). Bushee and Noe (2000) provide evidence that institutional investors favor stocks of companies with enhanced disclosure practices, as this reduces monitoring costs by lowering equity and debt expenses (Botosan, 1997; Sengupta, 1998; Botosan and Plumlee, 2000), narrowing bid-ask spreads (Welker, 1995; Healy et al., 1999; Leuz and Verrecchia, 2000), and enhancing stock price responsiveness to earnings (Price, 1998), consequently diminishing the magnitude of periodic surprises regarding firm performance and reducing stock price volatility (Lang and Lundholm, 1993; Healy et al., 1999).

In general, institutional investors actively engage in corporate governance of the firms in which they hold shares, utilizing a combination of "voice" and "exit" strategies (e.g., Edmans, 2014; Levit, 2013). "Voice" refers to direct intervention through formal channels, such as proxy voting to counter management positions in portfolio firms and filing shareholder proposals, as well as informal channels, including communication via letters to the board and frequent contact with portfolio firm management (Harris and Raviv, 2010; Levit and Malenko, 2011; Maug, 1998; Shleifer and Vishny, 1986). Through these actions, institutional investors express their dissatisfaction to management, whether privately or publicly (Holderness and Sheehan, 1985; Barclay and Holderness, 1991; Bethel et al., 1998; Brav et al., 2008; Klein and Zur, 2009).

On the other hand, "exit" refers to the actual or potential selling of shares (Parrino et al., 2003; Admati and Pfleiderer, 2009; Edmans, 2009; Edmans and Manso, 2011), indicating that when reviewing and finding unsatisfactory corporate governance, institutional investors tend to divest their stocks since such companies typically underperform. Moreover, institutional investors have legal fiduciary responsibilities to act prudently on behalf of their funds' beneficial owners (Monks, 1997; Chung and Zhang, 2011). Companies with inadequate corporate governance exhibit reduced prudence, thereby violating the fiduciary duty of institutional investors. Parrino et al. (2003) observe that such companies, characterized by poor management as indicated by forced CEO turnover, often reduce or eliminate dividends, leading to increased share price volatility, further supporting the explanation that some institutions divest due to prudence concerns.

3.2.2.2 Institutional activism

As previously demonstrated, the extent of institutional investors' engagement in corporate governance cannot be accurately determined solely by the appearance or percentage of their shareholdings in corporations. Instead, stronger indicators of their level of intervention or activism provide more reliable insights (Ryan and Schneider, 2002). Activist institutions place greater emphasis on their monitoring role and are more influenced by the quality of corporate governance in their pursuit of prudence. They utilize their influence to affect the processes or outcomes of specific portfolio firms or to bring about broader changes in processes or outcomes across multiple firms by symbolically targeting one or more portfolio firms.

A key distinguishing factor between activist and passive institutions lies in whether they actively buy or sell shares (exit) to influence managerial decisions (Appel et al., 2016). According to Del Guercio and Hawkins (1999), active investors tend to exhibit higher turnover rates. Their active buying or selling of shares serves as a mechanism for engaging in corporate governance and influencing firm policies (e.g., Aghion et al., 2013; Brav et al., 2008). They accumulate shares and make demands of managers or active fund managers, or alternatively divest their positions when managers underperform.

The selling of shares is commonly believed to be the most common action taken by institutions when dissatisfied with management. This aligns with the notion of the "Wall Street Rule," which suggests that institutional investors implicitly express praise or criticism of management through buying or selling, rarely engaging in more direct means of communication, not even a phone call. Dissent from the "Wall Street Rule" is virtually nonexistent. Furthermore, scholars argue that activism is the exception rather than the rule because institutions generally prioritize liquidity over monitoring (Coffee, 1991; Bhide, 1994). They contend that the concentrated ownership necessary for institutional investors to be actively involved in corporate governance carries a significant cost in terms of sacrificed liquidity. Bhide (1994) maintains that the liquidity of US markets hampers effective corporate governance since institutions can easily sell shares. Additionally, the reduction in trading costs over the past 25 years has increased the incentive for institutional investors to quickly divest from a stock rather than attempt to steer management in a new direction.

On the other hand, Maug (1998) argues that the relationship between liquidity and control is theoretically ambiguous. While it is easier to sell a large ownership position in a more liquid stock market, such markets also facilitate investors in acquiring large positions and consequently benefiting from their increased monitoring activities. Maug (1998) concludes that the impact of liquid stock markets on corporate governance is an empirical question, as these markets have two opposing effects. The question lies in determining which effect dominates. In summary, these two aspects are not contradictory; activist institutions can both "vote with their feet" by selling their shares and actively engage in corporate governance.

They aim to replicate the performance of a market index by holding a basket of representative securities in the index, proportionate to their weights. Index funds, which hold nearly all stocks in the market index rather than a representative sample, are the most visible type of passive funds (Appel et al., 2016). The investment objective of such institutions is to deliver returns comparable to those of a market index like the S&P 500 or a specific investment style, such as large-cap value, with low turnover, diversified portfolios, and minimal expenses.

However, opponents argue that there is insufficient strong evidence demonstrating that such activism has a significant impact on long-term stock or operating performance (Smith, 1996; Karpoff et al., 1996; Del Guercio and Hawkins, 1999). Moreover, limited evidence exists regarding the influence of institutional activism on specific corporate decisions (Huson, 1997; Carleton et al., 1998).

3.2.2.3 Activism of classified institutional investors.

To assess the extent of institutional activism, three key dimensions must be considered: portfolio firm characteristics, market characteristics, and the individual characteristics of institutional investors. Regarding portfolio firm characteristics, previous studies have examined factors such as firm size (Smith, 1996), stock price, media visibility, systematic risk level, transaction costs (Falkenstein, 1996), percentage of institutional ownership (Carleton et al., 1998; Del Guercio and Hawkins, 1999; Smith, 1996), and the likelihood of a forthcoming stock split (Mason and Shelor, 1998). Market characteristics also play a role in influencing institutional investors' decisions, including the availability of alternative equity options for funds (David et al., 1998), the potential for gaining an advantage over uninformed traders (Kahn and Winton, 1998), and expected returns from non-equity investment alternatives (McCarthy, 1999; O'Barr and Conley, 1992).

Regarding the individual characteristics of institutional investors, Ryan and Schneider (2002) synthesized prior research to create a composite model with twelve variables that illustrate different levels of activism among categories of institutions. Their findings indicate that: Larger funds possess greater resources and expertise for engaging in activism compared to smaller funds (Byrd et al., 1998); Funds with longer time horizons may offer portfolio firms the advantage of patient capital (Porter, 1992), and this patience is associated with increased influence and accountability, as reflected in activism (Black, 1992; Gibson, 1990; Millstein, 1991); Funds with mixed financial and nonfinancial performance expectations tend to engage in more activism than those solely focused on financial performance; Pressure-resistant funds tend to exhibit greater activism compared to pressure-sensitive funds; Increasing the ownership percentage and portfolio investment in the target firm's stock facilitates greater engagement in activism; A larger proportion of equity in the asset mix contributes to increased activism.; Institutional investors have legal fiduciary responsibilities to their funds' beneficial owners (Monks, 1997), and funds unaffected by ERISA regulation or conflicts arising from bankruptcy laws are more likely to engage in activism; Defined-benefit funds tend to be more activist compared to defined-contribution funds; Funds with a higher proportion of passive management tend to exhibit more activism than those with a lower proportion; The extent of external management of portfolios positively correlates with activist behavior; Retaining proxy voting rights enables funds to take more activism actions than delegating proxy voting.

Based on the aforementioned arguments, the following discussion focuses on the classification of institutional investors based on their level of activism. Firstly, due to varying liquidity needs, institutions differ in their investment time horizons. Therefore, Bushee (1998) categorizes institutional investors into "transient," "dedicated," and "quasi-indexers." "Transient" institutions are generally regarded as short-term-focused investors, while "dedicated" and "quasi-indexers" belong to the long-term category.

Short-term investors, such as mutual fund managers and bankers, prioritize the ability to redeem their shares at any time to meet liquidity requirements (Levinthal and Myatt, 1994; Monks and Minow, 1996). Porter (1992) highlights that short-term investors typically exhibit high portfolio turnover and hold highly diversified stocks. Such investors tend to rely on market forces rather than actively seeking means to enhance fund performance. Consequently, short-term institutional investors are less likely to gather information relevant to long-term value (Porter, 1992). In summary, based on the aforementioned reasons, "transient" institutions can be categorized as passive entities.

While long-term institutions are known for their commitment to providing stable, long-term shareholdings that focus on generating dividend income or capital appreciation over extended periods (Bushee, 1998). This patient capital approach, as emphasized by Porter (1992), grants portfolio firms the advantage of enhanced influence and accountability, which manifests in the form of activism (Black, 1992; Gibson, 1990; Millstein, 1991). Supporting this perspective, Harford et al. (2018) argue that being monitored by long-term institutional investors strengthens governance practices and curtails managerial misbehaviors such as earnings manipulation, financial fraud, accounting misconduct, and option backdating, while also reducing managerial entrenchment. These investors discourage certain investment and financing activities but encourage dividend payouts. Importantly, they also have a positive impact on both the quantity and quality of corporate innovation. Graham et al. (2005) contend that managers often admit to sacrificing long-term shareholder value in favor of short-term profits to meet shareholders' expectations. Therefore, oversight by investors with long investment horizons takes precedence over various other mechanisms in addressing managerial myopia (Drucker, 1986; Porter, 1992; Monks and Minow, 1995).

However, distinctions exist between "dedicated" and "quasi-indexers" institutions. Dedicated institutions, as outlined by Porter (1992) and Dobrzynski (1993), demonstrate a preference for substantial average investments in portfolio firms and exhibit extremely low turnover, reflecting a "relationship investing" role. Consequently, there is no doubt that "dedicated" institutions belong to the category of activist institutions. On the other hand, quasi-indexers institutions, while also favoring low turnover, tend to hold diversified portfolios, aligning with a passive, buy-and-hold investment strategy across a wide range of firms (Appel et al., 2016).

In an alternative perspective, Bushee (2001) offers a classification of institutional investors based on the fiduciary standard, which entails the prudent and responsible investment of funds on behalf of their clients. Four categories are distinguished based on legal form, illustrating the effects of fiduciary restrictions: banks, insurance companies, investment advisers (including mutual fund companies), and pensions and endowments.

In particular, banks serve as agents for both individuals and other institutions to oversee equities solely via their trust departments, through which they earn fee income by serving as a fiduciary, according to Fabozzi (1995) and Kidwell et al. (1993). The fiduciary obligations imposed on banks have led to their avoidance of investing in stocks that have been deemed imprudent by the courts (Badrinath et al., 1989; Del Guercio 1996). As a result, banks have the lowest percentage of institutional shareholdings. This is due to regulatory frameworks such as the Glass-Steagall Act, which prevented nationally and state-chartered Federal Reserve member banks from holding equity for their own accounts (Barth et al., 2000). Furthermore, banks tend to have a short investment horizon and are more sensitive to financial expectations and pressure. These factors, along with their relatively small fund size, suggest that their investment approach is generally passive, as Brancato (1997) has argued.

Pensions and endowments encompass private and public pension funds, as well as endowments of universities and foundations. While this group of institutions is subject to fairly strict fiduciary responsibilities, the standard has not been enforced as rigorously as in bank trusts (O'Barr and Conley, 1992; Del Guercio, 1996).

Pension funds are classified by the public and private sectors. Private plans are sub-categorized as either single-employer plans, which are administered by the corporate employers of beneficiaries, or multiemployer plans, which are typically managed by members' unions or TIAA-CREF. These funds are recognized as playing a prominent role in institutional ownership in the country, accounting for over 40% of total institutional ownership according to Securities Industry Association (2000) and have a legal obligation to provide support to participants during their retirement years (Kidwell et al., 1993).

Public pension funds rank among the largest institutional investors globally, including notable examples like CalPERS (Zorn, 1996), although smaller funds exist within this category. Generally, managers of public pension funds exhibit a long-term investment horizon due to the outflows to plan participants (Brown, 1998; Monks and Minow, 1996) and have lower asset turnover (Brancato, 1995). In addition to financial returns, these plans also prioritize social performance (Johnson and Greening, 1999; Romano, 1993). The public nature of these plans grants them independence from the private sector, reducing their sensitivity to external pressure (Blair, 1995; Brickley and Smith, 1988).

Public pension plans allocate almost 60% of their assets to equity investments (U.S. Census Bureau, 2000). While subject to diverse state legal environments, they lack federal-level regulation and are not governed by ERISA or bankruptcy conflicts (Martin, 1990; Woods, 1996). Considering that beneficiaries typically work in unionized civil service environments (O'Barr and Conley, 1992), these plans tend to be defined benefit plans (Andrews and Hurd, 1992), with managers leaning toward passive management of their portfolios (Institutional Investor, 1994; Sorensen et al., 1998). Although public pension plans generally retain their proxy voting rights, three out of the four largest funds are externally managed (Del Guercio and Hawkins, 1999).

Private pension funds, similar to their public counterparts, adopt a long-term investment approach due to their foreseeable and enduring obligations (Brown, 1998; Monks and Minow, 1996). This suggests a potential avenue for significant levels of activism.

However, certain characteristics of private pension funds tend to make them less inclined towards activism and more passive. For instance, their primary focus on financial performance fosters a lower propensity for activism. Nevertheless, private pension funds should still be considered among activist institutions, at the very least, in line with Useem et al. (1993). They may engage in slightly more activism compared to insurance companies or banks (Burr, 1995), as exemplified by the "golden rule" of non-interference with fellow corporations, as noted by Monks (Bird, 2001a).

Insurance firms, encompassing life or health and property or casualty companies, serve as risk bearers within the insurance process. The core activities of insurance companies involve managing private pension funds and utilizing equities as investment vehicles for their premiums. Life insurance companies, primarily through annuity contracts, compete with mutual funds in the pension management realm (Economist, 1999a; Schott, 1993). Unlike banks and pensions, these institutions face fewer restrictions arising from fiduciary responsibilities. Due to state regulations (Fabozzi, 1995) and the objective of aligning investments with the maturity of their liabilities for hedging purposes (Reardon, 1993), insurance companies tend to favor bonds and mortgages over stocks. The short-term horizon, focus on financial performance, sensitivity to external pressures, and limited equity exposure of insurance firms, along with rigorous regulation and active internal management, collectively contribute to a strong rationale for limited intervention, as proposed by Gillan and Starks (2000).

Simultaneously, investment companies and advisers establish mutual funds as a means to manage individual investments, which explains their elevated turnover rates. Among various types of institutions, investment advisers bear the least stringent fiduciary responsibilities (Del Guercio, 1996). Mutual funds rank second highest in terms of institutional shareholding in the United States and possess characteristics such as open-endedness and diversified portfolios. Crucially, external investment companies and advisers manage these diversified portfolios, catering to pensions and endowments, and transact shares in response to customer demand (Radcliffe, 1990). Only those funds investing in equity securities are classified as institutional investors. The high turnover rates of mutual funds and the relatively relaxed fiduciary standards (Del Guercio, 1996) contribute to their active nature, aligning with the perspectives of Davis and Thompson (1994) and Hirschman (1970), who argue that mutual fund managers typically express their discontent with underperforming firms through "exit" rather than "voice."

3.3. Hypothesis development

Following the recognition of IDD, companies headquartered in affected states are granted protection for their trade secrets by preventing employees from joining rival companies or establishing new ones (Kahnke et al., 2013). This restriction primarily impacts executives who typically possess a greater amount of business secrets (Ali et al., 2019). Consequently, executives are more significantly influenced by the IDD recognition, resulting in restricted executive mobility.

Restricted executive mobility has several implications. Firstly, it increases the cost of job loss for executives whose current positions are in jeopardy, thereby intensifying their career concerns. This elevated concern leads to an increase in their risk-taking incentives to engage in opportunistic activities that may positively influence their current employer's assessment of their abilities (Kothari et al., 2009; Gao et al., 2018; Ali and Li, 2019). Additionally, limitations on executives' outside employment opportunities contribute to a decrease in the pool of potential replacement CEOs. Consequently, companies have fewer options to find and hire a more competent CEO to replace the incumbent one, leaving them with no choice but to retain their current CEO, even if their performance is subpar (Grande-Herrera, 2019). As a result, the labor market discipline mechanism can be weakened following the recognition of IDD, creating an environment conducive to opportunistic activities among executives (Li et al., 2017; Kim et al., 2020; Ali and Li, 2019; Li et al., 2018; Gao et al., 2018; Islam et al., 2020; Na, 2020). In summary, restricted executive mobility following IDD recognition undermines corporate governance quality.

Institutional investors are widely regarded as a new monitoring mechanism that enhances corporate governance (Hartzell and Starks, 2003). Due to their substantial equity positions, institutional investors are more affected by decreased stock returns, which necessitates their avoidance of price impact to exercise prudence (Del Guercio, 1996; Hawley and Williams, 2000; Parrino et al., 2003). Therefore, institutional investors are incentivized to allocate resources to enhance their information-gathering capabilities, which enables them to be better informed about the prospects of the firm (Hartzell and Starks, 2003; Parrino et al., 2003). Consequently, companies may actively pursue increased institutional shareholding to benefit from enhanced monitoring following IDD recognition (Chung and Zhang, 2011).

H1(a): Following the recognition of IDD, institutional shareholding is increased for companies headquartered in affected states.

On the other hand, institutional investors engage in corporate governance of the companies they hold shares in through a combination of "voice" and "exit" approaches (e.g., Edmans, 2014; Levit, 2013). "Voice" refers to direct intervention, where institutional investors express their dissatisfaction with management, either privately or publicly (Holderness and Sheehan, 1985; Barclay and Holderness, 1991; Bethel et al., 1998; Brav et al., 2008; Klein and Zur, 2009). Alternatively, institutional investors can threaten or actually execute their exit strategy (Parrino et al., 2003; Admati and Pfleiderer, 2009; Edmans, 2009; Edmans and Manso, 2011). In practice, when reviewing and finding corporate governance unsatisfactory, institutional investors tend to sell their stocks, as poorly governed companies generally underperform. Moreover, companies with weak corporate governance exhibit reduced prudence, which violates institutional investors' fiduciary responsibility. Parrino et al. (2003) find that such companies often cut or eliminate dividends, and their share prices tend to be more volatile, aligning with the explanation that some institutions sell due to prudence concerns. Consequently, corporate governance quality serves as a crucial criterion for institutional investors' targeting mechanisms (McCahery et al., 2010).

H1(b): Following the recognition of IDD, institutional shareholding is decreased for companies headquartered in affected states.

Compared to short-term institutions, long-term institutions are characterized by their provision of long-time-horizon capital, which enables them to adopt a patient approach in their dealings with portfolio firms (Porter, 1992). This patient approach is accompanied by increased influence and accountability of these institutions, as demonstrated through activism (Black, 1992; Gibson, 1990; Millstein, 1991). Long-term institutional investors, therefore, have a strong incentive to allocate resources towards actively monitoring their portfolio firms (Parrino et al., 2003). Furthermore, their ability to spread the costs and benefits of ownership over an extended period grants long-term investors a comparative advantage in effectively overseeing managerial actions (Gaspar et al., 2005; Chen et al., 2007). Supporting this perspective, Harford et al. (2018) establish that the monitoring presence of long-term institutional investors contributes to decision-making processes that maximize shareholder value.

As important agents of corporate governance, long-term investors serve to curb managerial entrenchment and restrain managerial misconduct (Harford et al., 2018). In instances where they are dissatisfied with managerial performance, long-term investors employ a monitoring mechanism known as "exit" to exert influence on managerial behavior (Admati and Pfleiderer, 2009; Edmans, 2009; McCahery et al., 2016). Specifically, long-term investors retain their shareholdings when managers exhibit proper conduct but divest their shares if misconduct is observed. The practice of monitoring through "exit" is widespread in reality (e.g., Parrino et al., 2003).

H2: Following IDD recognition, companies in affected states witness their decreasing long-term institutional ownership, compared to short-term institutions.

Activist institutions wield their influence in two primary ways: either by directly affecting processes or outcomes within a specific portfolio firm or by instigating broader changes across multiple firms through the symbolic targeting of one or more portfolio firms. This signifies that activist institutions prioritize their monitoring role and are particularly swayed by the quality of corporate governance. In contrast to passive institutions, whose influence is typically limited to the use of "voice" due to their close alignment with benchmark weights, active investors are inclined to actively engage in share transactions (exit) to impact managerial decisions (Appel et al., 2016). Specifically, they accumulate shares and assert demands upon managers or active fund managers, or alternatively, divest their positions in response to poor managerial performance (Del Guercio and Hawkins, 1999; Brav et al., 2008; Aghion et al., 2013).

Among the various categories of institutional investors identified by Bushee (2001), investment advisors exhibit a high turnover rate and adhere to the least restrictive fiduciary standard (Del Guercio, 1996), indicating their active nature. This aligns with the findings of Davis and Thompson (1994) and Hirschman (1970), who suggest that mutual fund managers commonly express their dissatisfaction with underperforming firms through the exercise of "exit" rather than "voice."

H3: Following IDD adoption, companies in affected states witness their decreasing

ownership of investment advisors, among the institutional investor classifications based on the fiduciary standard.

The aforementioned discussion demonstrates that the recognition of IDD and the resulting restricted executive mobility can have several implications. Firstly, it leads to an increase in managers' career concerns, which subsequently increases their incentives to engage in risk-taking behaviors and opportunistic activities (Kothari et al., 2009; Gao et al., 2018; Ali and Li, 2019). Additionally, the impaired labor market discipline mechanism stemming from IDD recognition contributes to the prevalence of opportunistic activities among executives (Li et al., 2017; Kim et al., 2020; Ali and Li, 2019; Li et al., 2018; Gao et al., 2018; Islam et al., 2020; Na, 2020). Consequently, the agency costs within organizations are likely to increase in the wake of IDD recognition.

Furthermore, the impact on institutional ownership can be attributed to the traditional agency problem characterized by conflicting interests between managers and shareholders. Hence, I posit that the rise in agency costs serves as a plausible mechanism through which institutional ownership is influenced following IDD recognition by the firms in which they invest. Thus, I can infer that:

H4: Following the IDD adoption, there tend to be more agency costs in affected companies.

3.4. Methodology

To empirically establish the association between the recognition of the IDD and institutional ownership, I employ the difference-in-differences methodology as proposed by Bertrand and Mullainathan (2003), incorporating staggered treatments at the state level. Specifically, since multiple shocks affect different states and firms at different points, I can compare the before-after effect of the change in the IDD in affected states (the treatment group) to the before-after effect in states in which such a change was not affected (the control group), which enables me to deal with possible biases associated with the timing of the laws (Bertrand and Mullainathan, 2003). Furthermore, I mitigate the possibility of alternative explanations applicable to settings with a single shock, where contemporaneous events may influence my findings (Roberts and Whited, 2013). The identification strategy I adopt has also been employed in several recent studies, including Gormley and Matsa (2016), Klasa et al. (2018), Ali et al. (2019), Gao et al. (2021), among others. This strategy aids in drawing reliable conclusions regarding causality.

$$\begin{aligned}\boldsymbol{Institutional\,Shareholding}_{\,i,k,l,t} = \\ \boldsymbol{\alpha} + \boldsymbol{\beta} \times \boldsymbol{IDD}_{\boldsymbol{k,t-1}} + \boldsymbol{\gamma}' \times \boldsymbol{X}_{\boldsymbol{i,k,l,t-1}} + \boldsymbol{FirmFE} + \boldsymbol{IndustrybyYearFE} + \\ \boldsymbol{StatebyYearFE} + \boldsymbol{\varepsilon}_{\boldsymbol{i,k,l,t}} \qquad \textit{(3)}\end{aligned}$$

where $\boldsymbol{i}$ indexes firms; $\boldsymbol{k}$ indexes state of location; $\boldsymbol{l}$ indexes state of incorporation; $\boldsymbol{t}$ indexes years; $\boldsymbol{FirmFE}$, $\boldsymbol{IndustrybyYearFE}$ and $\boldsymbol{StatebyYearFE}$ are firm, four-digit-SIC industry-by-year and state-of-incorporation-by-year fixed effects respectively. $\boldsymbol{IO}_{\,i,k,l,t}$ is the dependent variable of interest. $\boldsymbol{IDD}_{\boldsymbol{k,t-1}}$ is the “treatment dummy”— a dummy variable that equals one if a IDD has been passed by time $\boldsymbol{t-1}$ in state $\boldsymbol{k}$ and zero otherwise. $\boldsymbol{X_{i,k,l,t-1}}$ denotes the set of time-varying control variables. $\boldsymbol{Error_{i,k,l,t}}$ is an error term. Specifically, I assign a firm's location based on the location of its headquarters, which is typically also where major plants and operations are located (Henderson and Ono, 2008). I lag all independent variables by one year to further mitigate the issue of reverse causality. The coefficient of interest in this model is $\boldsymbol{\beta}$. As explained by Imbens and Wooldridge (2009), the employed-firm fixed effects lead to $\boldsymbol{\beta}$ being estimated as the within-firm 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.

The inclusion of firm fixed effects allows for the control of unobserved, time-invariant variations across firms. Similarly, state-by-year fixed effects enable the control of unobserved, time-varying differences across states, while industry-by-year fixed effects address unobserved, time-varying differences across industries. I are able to control for state-level conditions that coincide with the IDD recognition since more than half of my firm samples are incorporated in a different state than where they are located. By considering these high-dimensional fixed effects, I can increase my confidence in the significant coefficient I observe, as it is less likely to be influenced by unobserved heterogeneous factors associated with the firm's industry, location, or observation year (Gormley and Matsa, 2014; 2016). As my treatment is defined at the state level, I apply clustering of standard errors by state.

Furthermore, it is important to note that the court's decision regarding the IDD is unrelated to firm-specific characteristics and is not intended to influence institutional shareholding. Unlike the enactment of state laws, which may be influenced by lobbying and political pressures, leading to potential reverse causation, the adoption and rejection of the IDD are based on court rulings in specific significant cases, which then serve as precedents for future cases. The court's ruling on a major IDD case is primarily influenced by the nature of the case and the characteristics of the justices, making state court rulings regarding the IDD arguably exogenous to firms' and shareholders' decision-making processes.

However, concerns have been raised about the potential econometric issues that arise from aggregating discrete difference-in-differences (DiD) estimates using ordinary least squares (OLS) estimation (Cengiz et al., 2019). To address these concerns and mitigate the biases inherent in such comparisons, I adopt the stacked difference-in-differences estimates as a robustness check, following the approaches of Gormley and Matsa (2011), Deshpande and Li (2019), and Cengiz et al. (2019), as discussed in the following sections.

3.5. Data sources and variable construction

3.5.1 Data sources and sample selection

To gather the necessary variables for my analysis, I undertake a data compilation process by merging several datasets. Specifically, I combine CRSP, Standard and Poor's Compustat, and Thomson-Reuters 13f data from the period spanning 1989 to 2018. The CRSP and Compustat datasets provide financial and stock-related information for the firms under study. Meanwhile, the Thomson-Reuters 13f dataset offers ownership details, encompassing various entities such as mutual funds, hedge funds, insurance companies, banks, trusts, and pension funds. I exclude data prior to 1989 due to the presence of continuous or missing data in CRSP, Compustat, and Thomson-Reuters 13f.

I obtain the details of IDD adoption dates from Klasa et al. (2018). To ensure an adequate time span for institutional investors to adjust their shareholding levels, I choose 1989 and 2018 as my starting and ending points, respectively. These years are set more than five years before Massachusetts adopted the IDD and after the last state on the adoption list, North Carolina, changed its IDD-related status.

My analysis excludes utility and financial companies (SIC codes 4900–4999 and SIC codes 6000–6999) due to differing regulatory oversight compared to other sectors. I narrow down the CRSP's share codes to values 10 and 11 to include only common shares. Additionally, I limit my sample to firms headquartered in the United States. I exclude observations with missing variables and institutional ownership data that show negative values. Furthermore, I employ winsorization at the 1% level for all continuous variables to mitigate extreme values. The final sample consists of 90,383 firm-year observations, representing 8,813 unique firms. Among these, 3,952 companies are headquartered in states that have adopted the IDD, while 4,861 companies are headquartered in states that have not adopted the IDD.

3.5.2 Definitions of variables

3.5.2.1 Inevitable Disclosure Doctrine

The application of the IDD occurs at the state level, dictated by the jurisdiction of state courts. Specifically, the applicability of the IDD typically hinges on the states where employees are based, rather than the state of incorporation. However, due to data limitations, I are only able to access information regarding the state where a firm's headquarters is located, assuming that employees with greater access to trade secrets tend to work at the headquarters (Klasa et al., 2018).

I obtain the specific dates of IDD adoption from Klasa et al. (2018). Appendix A provides a comprehensive overview of the states that have adopted these provisions, along with details of precedent-setting court cases and their respective adoption dates. New York was the first U.S. state to adopt the IDD in 1919. Subsequently, three states adopted the IDD in the 1960s, one in the 1970s, four in the 1980s, nine in the 1990s, and three in the 2000s. In total, 21 states adopted the IDD, with 12 of them doing so during my sample period. Over my sample period, three states that had previously adopted the IDD subsequently rejected it. Consistent with Klasa et al. (2018), for the 21 states where courts adopted the IDD, I construct an indicator variable, IDD, which takes a value of zero in all years prior to the adoption date and a value of one in the year of adoption and subsequent years. For the remaining 29 states that did not explicitly adopt the IDD, the IDD indicator is set to zero.

3.5.2.2 Institutional ownership

The dependent variable in my regression analysis is the institutional ownership percentage, which I obtain from Thomson-Reuters 13f. While some seminar papers, such as Chen et al. (2002), construct institutional breadth and other metrics at the mutual fund ownership level using the Thomson-Reuters Mutual Fund database, recent studies have shifted their focus to Thomson-Reuters 13f data, which provides a broader perspective on aggregate ownership positions held by institutional investors. According to the introduction of Thomson-Reuters 13f, it encompasses ownership not only by mutual funds but also by hedge funds, insurance companies, banks, trusts, pension funds, and other entities. I measure institutional ownership by calculating the ratio of the Institutional Ownership Level to the total number of shares outstanding, following the approach of Chung and Zhang (2011). The Institutional Ownership Level is computed by summing up all shares for each security on a quarterly basis.

I further classify institutional investors according to their investment horizon, in line with the framework proposed by Bushee (1998). Institutional investors are divided into long-term ownership and short-term ownership. Specifically, long-term ownership includes "Dedicated" and "Quasi-indexers" institutions, while "Transient" institutions are considered short-term investors.

Consistent with Bushee (2001), institutional investors are classified based on the fiduciary standard. I categorize institutional investors into four groups according to their legal form, namely banks, insurance companies, investment advisers (including mutual fund companies), and pensions and endowments. Prior research has shown significant differences in institutional preferences for current earnings and certain firm characteristics (such as size and growth potential) based on this classification (Del Guercio 1996; Lang and McNichols 1997; Abarbanell et al., 2001). Banks represent individuals and other institutions in managing equities through their trust departments, and their strict fiduciary requirements lead them to avoid stocks defined by courts as imprudent (Badrinath et al., 1989; Del Guercio, 1996). Pensions and endowments encompass private pensions, public pensions, and endowments of universities and foundations. While fiduciary responsibilities also apply to this group of institutions, they have not been enforced as strictly as with bank trusts (O'Barr and Conley, 1992; Del Guercio, 1996). Insurance companies primarily engage in managing private pension funds and hold equities as an investment vehicle for their premiums. Compared to banks and pensions, these institutions face fewer restrictions in terms of fiduciary responsibilities. Investment advisers, on the other hand, establish mutual funds to manage individual investments. They also serve as external fund managers in pensions and endowments. Among all types of institutions, investment advisers have historically faced the least restrictive fiduciary responsibilities (Del Guercio, 1996).

3.5.2.3 Agency costs

Following the approach of Angetul (2000), I employ the expense ratio as a measure of agency costs, which is calculated as operating expenses divided by revenues. This ratio captures the effectiveness of a firm's management in controlling operating costs, including excessive perquisite consumption and other direct agency costs. Consequently, an increase in agency costs is typically associated with an increase in the expense ratio.

Additionally, I utilize the asset utilization ratio, calculated as annual sales divided by total assets, to assess the efficiency with which a firm's management deploys its assets. In contrast to the expense ratio, the sales-to-asset ratio is inversely related to agency costs. When agency costs increase, this ratio tends to decrease. Such costs arise from various factors, including the manager's poor investment decisions, insufficient effort exerted by the manager leading to lower revenues, and the consumption of executive perquisites, which results in the acquisition of unproductive assets such as extravagant office spaces, furnishings, automobiles, and resort properties.

3.5.2.4 Control variables

To empirically demonstrate the impact of IDD on institutional investors' shareholdings, it is essential to account for a range of firm-level characteristics that may influence institutional ownership. This aims to eliminate the possibility of alternative explanations where non-interest variables affect my dependent variable.

Prior studies have indicated that companies with stronger governance structures tend to exhibit higher stock market liquidity (Chung et al., 2010). Additionally, mutual funds tend to favor stocks characterized by greater market liquidity (Falkenstein, 1996; Huang, 2009). Thus, corporate governance and liquidity are likely to influence the investment decisions of institutional investors. Furthermore, Badrinath et al. (1996) revealed that institutional investors show a preference for mature companies with safety net attributes such as low yield volatility and low financial leverage. Gompers and Metrick (2001) discovered that institutional investors favor large corporate stocks, while Grinstein and Michaelly (2005) demonstrated a preference for companies with lower dividend payments. Gompers et al. (2003) also established a positive relationship between higher shareholders' equity and stock returns, suggesting better performance.

In addition to IDD, non-compete agreements can also affect institutional shareholding. Bird and Knopf (2015) found that employees tend to have greater mobility in the absence of stringent non-compete agreements. Therefore, I introduce the NCA variable as a control variable, following Garmaise's (2011) Noncompetition Agreement Enforceability Index (see Appendix B). This inclusion helps to mitigate potential explanations that changes in institutional ownership are driven by NCAs. Moreover, as agency costs are considered a plausible mechanism linking executive mobility and institutional shareholding, I incorporate agency costs as an additional control variable in the main regression.

Thus, based on the discussion above, I control following listed variables: NCA (Noncompetition Agreement Enforceability Index following Garmaise (2011)), Agency costs (Operating expenses (xopr) divided by revenues (ni)), firm size (Natural logarithm of total assets (at)), cash holding (Cash and short-term investments (che) normalized by total assets (at)), ROA (Income before extraordinary items normalized (ib) by lagged total assets (at)), Spread (Average effective bid-ask spread), Stock return (Annual stock return), leverage (Total debt (dltt+dlc) normalized by total assets (at)), Capex (Capital Expenditures (capx) normalized by total assets (at)), Stock volatility (Standard deviation of quote-midpoint daily returns), MTB (Ratio of market value of equity (csho*prcc_f) to book value of equity (at-dltt-dlc)), firm age (Number of years since a firm’s first appearance in the Compustat database), SG&A (Selling, general, and administrative expenses (xsga) divided by lagged total assets (at)), Turnover (Average ratio of monthly trading volume to the number of shares outstanding), ROE (Net income (ni) divided by total shareholders' equity value (csho*prcc_f)).

3.5.3 Descriptive statistics

TABLE 3.1 provides summary statistics for key dependent, independent, and control variables employed in my analysis. my primary focus lies on total institutional shareholding, which serves as the main dependent variable. Specifically, the average and median percentages of shareholding by institutional investors are 46% and 45%, respectively. The dataset comprises 3,952 companies headquartered in states that have implemented the IDD policy, while 4,861 companies are located in states that have not adopted the IDD. Notably, 37% of the firm-year observations in my sample pertain to companies headquartered in IDD-adopting states.

Table 3.1. Summary statistics

The sample consists of firm-year observations during the 1989−2018 period, obtained from the CRSP-Compustat merged database and Thomson Routers 13-f. Samples focus on public firms listed on CRSP/ Compustat merged database excluding utilities and financials (SIC codes 4900–4999 and SIC codes 6000–6999) due to different regulatory oversight from others and restrict the CRSP's share codes to values 10 and 11 to gain common shares. I also filter on firms headquartered in the U.S The observations with missing variables and minus institutional ownership data are excluded. All continuous variables are winsorized at the 1st and 99th percentiles.

Variables	n	Mean	S.D.	Min	0.250	Mdn	0.750	Max
IDD	95296	0.370	0.480	0	0	0	1	1
Institutional shareholding	95296	0.460	0.290	0	0.190	0.450	0.720	0.980
Long term	95296	0.320	0.220	0	0.120	0.300	0.500	0.790
Short term	95296	0.120	0.110	0	0.030	0.100	0.190	0.450
Bank	95296	0.060	0.060	0	0.010	0.050	0.100	0.250
Insurance	95296	0.020	0.030	0	0	0.010	0.030	0.150
Investment advisor	95296	0.330	0.220	0	0.130	0.300	0.510	0.790
Pension	95296	0.020	0.020	0	0	0.010	0.030	0.120
Expense ratio	93186	1.670	4.340	0.390	0.830	0.900	0.980	37.83
Asset utilization	95133	1.090	0.790	0	0.540	0.960	1.460	4.100
NCA	95296	3.380	2.400	0	0	3	5	9
Firm size	95230	5.570	2.020	1.440	4.090	5.410	6.920	10.72
Cash holding	95213	0.220	0.250	0	0.030	0.110	0.320	0.950
Leverage	94829	0.220	0.220	0	0.010	0.170	0.340	1
MTB	94757	2.140	2.420	0.010	0.790	1.370	2.490	15.28
ROE	95058	-0.100	0.460	-3.270	-0.060	0.030	0.060	0.290
Stock volatility	94476	0.200	0.160	0.040	0.110	0.160	0.230	1.120
Turnover	91190	14.82	14.77	0.690	5.050	10.26	19.14	84.54

3.6. Main results

3.6.1 The effect of IDD recognition on institutional shareholding

Table 3.2 presents the difference-in-differences estimates, as detailed in the Methodology section, examining the impact of state court recognition of IDD on institutional shareholding in affected states. The analysis encompasses a comprehensive sample of 90,383 observations spanning the period from 1989 to 2018. The first column of the table includes only the IDD indicator as the independent variable, along with firm, industry by year fixed effects, and headquarter-state by year fixed effects. In the second column, I introduce the NCA indicator, and in the third column, I control for agency costs represented by the expense ratio. Finally, the fourth column incorporates additional control variables mentioned earlier. The coefficient of interest, denoted as β, represents the average treatment effect of state recognition of IDD on institutional ownership.

Across all four columns, the coefficients consistently display negative and statistically significant values at the 5%, 5%, 5%, and 1% levels, respectively. This indicates that the recognition of IDD exerts a negative and statistically significant influence on the institutional shareholding of firms in affected states. Economically, in the context of the fourth column, which includes all control variables, my findings suggest that following IDD recognition, affected firms experience a 2% decrease relative to the mean of institutional shareholding during the sample period.

My findings of decreased institutional ownership following IDD recognition challenge the notion that companies actively seek to enhance their corporate governance by attracting increased institutional shareholding (Chung and Zhang, 2011). While institutional investors do take on a monitoring role in response to weakened corporate governance (Hartzell and Starks, 2003), as demonstrated above, these investors engage in corporate governance and monitor their portfolio companies through a combination of "voice" and "exit" strategies (e.g., Edmans, 2014; Levit, 2013).The results indicate that when institutional investors review and express dissatisfaction with corporate governance, they tend to sell their stocks as a prudent response. Notably, corporate governance quality serves as a key criterion guiding the targeting decisions of institutional investors (McCahery et al., 2010). In summary, the limitations on managerial mobility resulting from IDD recognition lead institutional investors to disengage, relative to their counterparts whose trade protection remains unaffected, instead of voicing their concerns to management.

Table 3.2. Effect of IDD on institutional shareholding

This table reports results from OLS regressions of institutional ownership on the indicator for the recognition of IDD. The sample spans the 1989-2018 period. *Institutional shareholding* serves as the dependent variable in my regression, *IDD* is an indicator variable equal to one if the firm is headquartered in a state whose courts recognize the IDD, and zero otherwise. Besides, I also introduce firm fixed effects (FE), state-of-incorporation-by-year FE, and standard industrial classification industry-by-year FE. Continuous variables are winsorized at their 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at the state level (*t* -statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Dependent variable			
	(1)	(2)	(3)	(4)
Independent Variables	Institutional shareholding	Institutional shareholding	Institutional shareholding	Institutional shareholding
IDD	-0.008**	-0.009**	-0.009**	-0.008***
	(-2.109)	(-2.197)	(-2.424)	(-3.289)
NCA		0.003**	0.003***	0.003***
		(2.648)	(2.709)	(4.514)
Expense ratio			-0.001**	-0.000
			(-2.269)	(-0.069)
Firm size				0.084***
				(31.445)
Cash holding				0.034***
				(4.158)
Leverage				-0.096***
				(-9.658)
MTB				0.008***
				(15.563)
ROE				0.025***
				(11.434)
Stock volatility				-0.117***
				(-12.40023)
Turnover				0.001***
				(14.407)
Constant	0.466***	0.453***	0.45845***	-0.018
	(317.398)	(99.844)	(104.488)	(-1.161)
Observations	92,713	90,383	88,388	83,218
R-squared	0.775	0.814	0.815	0.851
Company FE	YES	YES	YES	YES
State-by-Year FE	YES	YES	YES	YES
Industry-by-Year FE	YES	YES	YES	YES

3.6.2 The effect of IDD recognition on classified institutional shareholding

I have reached a general conclusion that restricted executive mobility resulting from the adoption of IDD, leading to worse corporate governance, adversely affects institutional shareholding. This negative impact can be attributed to the monitoring role and fiduciary responsibility of institutional investors. To further demonstrate that worse corporate governance and increased agency costs are the channels between executive mobility and institutional shareholding, I detect the effects of IDD recognition on institutional investors more specifically. In other words, which categories of institutional investors are influenced significantly among all the institutional investor classifications.

In order to classify institutional investors based on their expected investment horizon and fiduciary standards, I adopt the categorizations proposed by Bushee (1998) into 'long-term' and 'short-term' institutions, and by Bushee (2001) into banks, insurance companies, investment advisers (including mutual fund companies), and pensions and endowments, respectively. The sample used in my analysis spans the period from 1989 to 2018 and encompasses 65,038 observations.

Long-term institutions, in contrast to short-term institutions, tend to provide capital with longer investment horizons, offering the advantage of patient capital (Porter, 1992). This patience often translates into increased influence and accountability, evident in shareholder activism (Black, 1992; Gibson, 1990; Millstein, 1991). Consequently, long-term institutional investors have a stronger incentive to allocate resources towards monitoring their portfolio firms (Parrino et al., 2003). Importantly, by spreading the costs of ownership over an extended period, long-term investors possess a comparative advantage in effectively monitoring managers (Gaspar et al., 2005; Chen et al., 2007). Supporting this perspective, Harford et al. (2018) find that being monitored by long-term institutional investors results in decision-making processes that maximize shareholder value.

Given their potential as corporate governance agents (Harford et al., 2018), long-term investors play a restraining role in curbing managerial entrenchment and misconduct. When dissatisfied with managerial performance, these investors utilize the "exit" option to influence managerial behavior (McCahery et al., 2016; Admati and Pfleiderer, 2009; Edmans, 2009). In other words, they retain their shares if managers demonstrate proper conduct and sell their shares if managerial misconduct is observed. Monitoring through "exit" is a prevalent practice in the industry (e.g., Parrino et al., 2003).

When it comes to fiduciary responsibility, I investigate the distinctions between activist and passive institutional investors. Activist institutions employ their power to influence the processes or outcomes of specific portfolio firms, or to catalyze broader changes across multiple firms by targeting one or more portfolio firms. These institutions prioritize their monitoring role and are more influenced by the quality of corporate governance. Unlike passive institutions, whose influence is typically limited to "voice" due to their closely aligned portfolio weights with the benchmark, activist investors actively buy or sell shares (exit) to shape managerial decisions (Appel et al., 2016). They accumulate shares and exert demands on managers or active fund managers, or alternatively, divest their positions when managers underperform (Del Guercio and Hawkins, 1999; Aghion et al., 2013; Brav et al., 2008).

Among the institutional investor categories classified by Bushee (2001), investment advisers are characterized by a high turnover rate and a less restrictive fiduciary standard (Del Guercio, 1996), which reflects their active nature. This finding aligns with Davis and Thompson (1994) and Hirschman (1970), who argue that mutual fund managers often express their dissatisfaction with underperforming firms through "exit" rather than "voice."

As observed in ***Table 3.3***, the coefficients for long-term institutions and investment advisers are significantly negative, whereas the coefficients for other institutional investor categories are not statistically significant. This reveals that, following IDD adoption, firms in affected states experience a decline in long-term and activist institutional ownership, which provides robust evidence supporting my argument that long-term institutional investors fulfill their significant monitoring role due to their longer holding periods, while activist institutional investors actively trade shares to influence managerial decisions. This further corroborates my previous finding that institutional investors tend to divest their holdings due to their monitoring role and fiduciary responsibilities following IDD adoption, and that corporate governance and agency costs serve as the channels linking executive mobility and institutional shareholding.

Table 3.3. Effects of IDD on classified institutional shareholding.

This table reports results from OLS regressions of series of classified institutional ownership on the indicator for the recognition of the IDD. The sample spans the 1989-2018 period. The dependent variables are classified institutional ownership. On the basis of the previously calculated institutional ownership, *institutional shareholding* is further classified following BUSHEE (1998) based on investment horizon as well as following Bushee (2001) based on the fiduciary standard based on which to invest funds prudently on the authority of their clients. *IDD* is an indicator variable equal to one if the firm is headquartered in a state whose courts recognize the IDD, and zero otherwise. Besides, I also introduce firm fixed effects (FE), state-of-incorporation-by-year FE, and standard industrial classification industry-by-year FE. Continuous variables, except state-level variables, are winsorized at their 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at the state level (*t* -statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Dependent variable					
	(1)	(2)	(3)	(4)	(5)	(6)
Independent Variables	Long term	Short term	Bank	Insurance	Investment advisor	Pension
IDD	-0.007**	-0.002	-0.001	-0.001	-0.009***	-0.000
	(-2.551)	(-1.515)	(-0.738)	(-1.517)	(-3.670)	(-0.656)
NCA	0.001*	0.001***	-0.000	0.000	0.003***	0.000
	(1.847)	(3.022)	(-1.130)	(0.539)	(4.090)	(0.475)
Expense ratio	-0.000	0.000	0.000	0.000	-0.000	0.000
	(-0.697)	(0.644)	(0.008)	(0.071)	(-0.438)	(0.913)
Firm size	0.067***	0.011***	0.016***	0.005***	0.048***	0.005***
	(26.442)	(4.620)	(24.487)	(12.824)	(11.265)	(16.419)
Cash holding	0.014*	0.029***	0.008***	0.001	0.030***	0.001
	(1.843)	(7.771)	(3.679)	(1.142)	(3.830)	(0.611)
Leverage	-0.086***	-0.017***	-0.020***	-0.006***	-0.066***	-0.006***
	(-9.629)	(-4.714)	(-9.214)	(-4.269)	(-8.142)	(-5.207)
MTB	0.002**	0.000	0.001**	0.000	0.001	-0.000
	(2.275)	(0.498)	(2.263)	(0.473)	(1.493)	(-0.184)
ROE	0.021***	0.002	0.002***	0.000	0.019***	0.000*
	(7.720)	(1.151)	(4.150)	(1.388)	(5.515)	(1.787)
Stock volatility	-0.067***	-0.035***	-0.021***	-0.006***	-0.072***	-0.003***
	(-7.375)	(-3.797)	(-7.095)	(-3.621)	(-5.615)	(-3.513)
Turnover	0.000***	0.001***	0.000***	0.000***	0.001***	0.000***
	(2.895)	(17.460)	(12.361)	(4.989)	(11.446)	(2.778)
Constant	-0.006	0.041**	-0.025***	-0.009***	0.081***	-0.013***
	(-0.322)	(2.568)	(-4.859)	(-2.836)	(2.864)	(-5.263)
Observations	64,141	64,141	64,141	64,141	64,141	64,141
R-squared	0.843	0.727	0.788	0.656	0.835	0.689
Company FE	YES	YES	YES	YES	YES	YES
State-by-Year FE	YES	YES	YES	YES	YES	YES
Industry-by-Year FE	YES	YES	YES	YES	YES	YES

3.6.3 The effect of IDD recognition on agency problem

Given that the adoption of the IDD has been associated with a general decrease in institutional shareholding, attributable to the monitoring responsibilities and fiduciary obligations of institutional investors, I endeavor to delve further into the factors underlying the leaving of institutional investors from companies headquartered in affected states subsequent to the IDD recognition. my investigation centers on the concept of agency cost and its implications following IDD recognition. To assess agency costs, I employ the expense ratio, as proposed by Angetul (2000), which entails dividing operating expenses by revenues to gauge the efficacy of the firm's management in controlling operating costs. Furthermore, to bolster the reliability of my findings, I introduce the asset utilization ratio, also based on Angetul's (2000) framework, involving the division of annual sales by total assets to evaluate the effectiveness of the firm's management in deploying its assets. Consequently, an increase in agency costs is typically associated with a decrease in this measure. Unlike the expense ratio, agency costs display an inverse relationship with the sales-to-asset ratio. These costs arise due to managers: i) making suboptimal investment decisions, ii) exerting insufficient effort, leading to diminished revenues, and iii) indulging in executive perquisites, resulting in the acquisition of unproductive assets such as overly extravagant office space, furnishings, vehicles, and resort properties.

The analyzed dataset encompasses observations from the years 1989 to 2018, yielding a sample size of 90,216. The findings, presented in ***Table 3.4***, incorporate the control variables utilized in my baseline regression, along with an additional control for institutional shareholding. The results reveal a significantly positive coefficient for the expense ratio and a significantly positive coefficient for the asset utilization ratio, aligning with my prediction that agency costs escalate. This implies that, subsequent to the adoption of IDD, managers in affected states demonstrate inadequate control over operating costs and suboptimal asset deployment. Hence, I can surmise that the increased agency costs may serve as a potential mechanism influencing the behavior of institutional ownership following IDD adoption by the firms in which they invest.

Table 3.4. Effects of IDD on agency costs

This table reports results from OLS regressions of agency costs on the indicator for the recognition of the IDD. The sample spans the 1989-2018 period. I utilise expense ratio and asset utilization ratio to measure agency costs. IDD is an indicator variable equal to one if the firm is headquartered in a state whose courts recognize the IDD, and zero otherwise. Besides, I also introduce firm fixed effects (FE), state-of-incorporation-by-year FE, and standard industrial classification industry-by-year FE. Continuous variables, except state-level variables, are winsorized at their 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at the state level (*t* -statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Dependent variable					
Independent Variables	(1) Expense ratio	(2) Expense ratio	(3) Expense ratio	(4) Asset utilization	(5) Asset utilization	(6) Asset utilization
IDD	0.083*	0.080*	0.070*	-0.028*	-0.029*	-0.033*
	(1.826)	(1.766)	(1.685)	(-1.692)	(-1.802)	(-1.925)
Institutional		-0.361***	-0.103		-0.218***	0.021
shareholding		(-2.919)	(-0.714)		(-12.031)	(1.153)
NCA	-0.052***	-0.051***	-0.054***	0.002	0.002	0.000
	(-5.535)	(-5.540)	(-5.700)	(1.231)	(1.314)	(0.157)
Firm size			-0.136***			-0.195***
			(-3.247)			(-19.672)
Cash holding			2.744***			-0.721***
			(12.908)			(-28.889)
Leverage			-0.178			-0.126***
			(-0.877)			(-4.053)
MTB			0.024*			0.007***
			(1.764)			(6.100)
ROE			-0.445***			0.001
			(-5.829)			(0.192)
Stock volatility			0.148			-0.117***
			(1.192)			(-5.537)
Turnover			0.001			0.001***
			(0.410)			(4.177)
Constant	1.794***	1.963***	1.974***	1.106***	1.207***	2.376***
	(49.640)	(25.766)	(9.250)	(230.511)	(133.963)	(42.609)
Observations	90,179	90,179	85,023	91,983	91,983	86,760
R-squared	0.619	0.620	0.626	0.841	0.843	0.865
Company FE	YES	YES	YES	YES	YES	YES
State-by-Year FE	YES	YES	YES	YES	YES	YES
Industry-by-Year FE	YES	YES	YES	YES	YES	YES

3.7. Robustness and diagnostic tests

3.7.1 Role of institutional shareholding

The IDD is primarily aimed at safeguarding trade secrets. If the decreased institutional shareholding following the recognition of IDD is attributable to trade secret protection rather than being spuriously driven by unobserved heterogeneity, the effects of the IDD on institutional ownership are stronger in more knowledge-oriented industries. This argument is predicated on the understanding that the success of knowledge-oriented firms hinges greatly on the generation of new knowledge and organizational expertise. And the adoption of IDD enables them to maintain their competitive advantage and generate greater value (Qiu and Wang, 2018). Thus, protecting proprietary knowledge from unauthorized disclosure to competitors holds greater relevance for firms that are highly knowledge-oriented.

I gauge the degree of knowledge orientation in industries by assessing their selling, general, and administrative (SG&A) expenses. SG&A expenses encompass costs associated with employee training, information technology (IT) investment, consulting, advertising and marketing, research and development (R&D), as well as investments in information systems and distribution channels. These expenses are geared towards enhancing a firm's proprietary knowledge base. Numerous studies, such as Lev and Radhakrishnan (2005), Lev et al. (2009), Banker et al. (2011), Eisfeldt and Papanikolaou (2013), Zhang (2014), and Li et al. (1999), have established that capitalized SG&A expenses serve as a reliable proxy for a firm's organizational capital, which encompasses its accumulation of proprietary knowledge, including operational processes and know-how that confer a competitive edge, making them difficult for competitors to replicate (Prescott and Visscher, 1980). Therefore, I utilize SG&A expenses to measure the level of knowledge orientation in industries and firms. Specifically, I calculate the median percentage of SG&A spending relative to total sales for firms within an industry classified by the first two digits of the SIC code.

I examine heterogeneous treatment effects of state-level IDD recognition on institutional shareholding conditional on the 1990 industries' median percentage of SG&A spending of total sales (three years before the first state (TX) recognized the IDD during my sample period). Specifically, I repeat my baseline regression by replacing the sample with the top 50 per cent and bottom 50 per cent of the whole sample.

This section provides additional insights into the economic mechanism underlying my findings and emphasizes the strategic role of institutional investors in industries characterized by a greater emphasis on knowledge. Furthermore, these tests offer further evidence supporting the causal nature of my main results. In other words, if an omitted variable were to be responsible for driving the baseline results, it would also need to explain the cross-sectional results reported in this section. ***Table 3.5*** reveals a significantly negative coefficient estimating the impact of IDD recognition when affected firms operate in more knowledge-oriented industries. my findings demonstrate that IDD recognition leads to a larger reduction in institutional ownership for firms operating in knowledge-intensive industries. Consequently, I can conclude that IDD diminishes institutional ownership, and its effects are more pronounced in knowledge-oriented industries, thus corroborating my initial hypothesis.

Table 3.5. Cross-sectional variation in the effect of the IDD on institutional shareholding

This table reports results from OLS regressions of heterogeneous treatment effects of state-level IDD recognition on institutional shareholding conditional on knowledge-oriented level of industries. The two columns show results of regressions using the two subsamples, divided by the 1990 industries' median percentage of SG&A spending of total sales (three years before the first state (TX) recognized the IDD during my sample period). The sample spans the 1989-2018 period. *IDD* is an indicator variable equal to one if the firm is headquartered in a state whose courts recognize the IDD, and zero otherwise. Besides, I also introduce firm fixed effects (FE), state-of-incorporation-by-year FE, and standard industrial classification industry-by-year FE. Continuous variables, except state-level variables, are winsorized at their 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at the state level (*t* -statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Dependent variable	
Independent Variables	(1) More knowledge-oriented industries in1990 Institutional shareholding	(2) Less knowledge-oriented industries in1990 Institutional shareholding
IDD	-0.011***	-0.003
	(-2.961)	(-0.559)
NCA	0.002**	0.005**
	(2.258)	(2.255)
Expense ratio	0.000	-0.001
	(0.110)	(-0.728)
Firm size	0.087***	0.076***
	(21.734)	(15.046)
Cash holding	0.035***	0.039*
	(4.278)	(1.874)
Leverage	-0.086***	-0.124***
	(-7.116)	(-7.503)
MTB	0.008***	0.008***
	(13.774)	(6.082)
ROE	0.021***	0.030***
	(8.048)	(7.864)
Stock volatility	-0.119***	-0.111***
	(-9.150)	(-5.288)
Turnover	0.001***	0.001***
	(12.102)	(6.971)
Constant	-0.028	0.023
	(-1.351)	(0.756)
Observations	60,921	22,057
R-squared	0.855	0.850
Company FE	YES	YES
Industry-Year FE	YES	YES
State-Year FE	YES	YES

3.7.2 Stacked difference-in-differences estimation

Cengiz et al. (2019) have drawn attention to potential econometric challenges that arise when aggregating discrete Difference-in-Differences (DiD) estimates using Ordinary Least Squares (OLS). These challenges include heterogeneous treatment effects and the possibility of negative weights associated with specific treatments. To address these concerns, I employ stacked Difference-in-Differences (DID) estimates as a robustness check in my analysis of data obtained from a staggered adoption design. The stacked DID method aims to transform the staggered adoption scenario into a two-group, two-period design, wherein the difference in differences enables the identification of the average treatment effect on the treated. This identification is weighted by the relative sizes of the group-specific datasets and the variance of treatment status within those datasets. To achieve this, separate datasets containing observations on treated and control units for each treatment group are stacked.

This methodology employs a more rigorous set of criteria to establish clean control groups, aiming to enhance the validity of the analysis. Additionally, the approach utilizes event-time stacking and alignment, effectively simulating a scenario where events occur simultaneously, thereby precluding the use of previously treated units as suitable comparison units. By excluding all firm-year observations that have undergone treatment, the aim is to ensure a pure control group and mitigate potential biases arising from heterogeneous treatment effects (Goodman-Bacon, 2019). More specifically, a distinct dataset is constructed for each treatment event, encompassing all firm-year observations within a window of [-5, 5], spanning five years prior to and five years following the event.

Subsequently, these group-specific datasets are stacked in event-time, and outcomes are subjected to regression analysis, with the treatment status represented by an indicator variable (Stacked IDD) taking the value of one when the firm is treated in an event year (i.e., $\tau > 0$) within each group, and zero otherwise. The analysis includes fixed effects for all firm- Cohort combinations, and fixed effects for all relative-year-Cohort combinations. To account for potential clustering, the standard errors are clustered by group and state. These stacked regressions are of the form:

$$\boldsymbol{IO}_{\boldsymbol{itd}} = \boldsymbol{\alpha} + \boldsymbol{\beta} \times (\boldsymbol{T}_{\boldsymbol{sd}} \times \boldsymbol{P}_{\boldsymbol{td}}) + \boldsymbol{\gamma} \times \boldsymbol{X}_{\boldsymbol{itd}} + \boldsymbol{\theta}_{\boldsymbol{sd}} + \boldsymbol{\gamma}_{\boldsymbol{td}} + \boldsymbol{\varepsilon}_{\boldsymbol{itd}} \quad \textbf{(4)}$$

where $\boldsymbol{i}$ indexes firms; $\boldsymbol{t}$ indexes relative year to IDD recognition; $\boldsymbol{d}$ indexes dataset group by each IDD adoption event; $\boldsymbol{T}_{\boldsymbol{sd}}$ is an indicator that company s is a treated unit in sub-experiment ***d.*** $\boldsymbol{P}_{\boldsymbol{td}}$ is an indicator that period ***t*** is in the post period in sub-experiment $\boldsymbol{d}$. $\boldsymbol{\theta}_{\boldsymbol{sd}}$ and $\boldsymbol{\gamma}_{\boldsymbol{td}}$ are firm by Cohort and relative-year by Cohort fixed effects respectively. $\boldsymbol{IO}_{\boldsymbol{itd}}$ is the dependent variable of interest.

I present the estimation results in ***Table 3.6***. The difference-in-differences estimate is -0.00820, which is statistically significant at the 10% level. This confirms that my difference-in-differences estimates are not sensitive to heterogeneous treatment effects.

Table 3.6. Stacked difference-in-differences estimation and alternative IDD adoption list

Column (1) of this table reports results from stacked OLS difference-in-differences estimation of institutional ownership on the indicator for the recognition of the IDD, by focusing on a window that contains the five years before and after the adoption of IDD (and dropping states that ever-adopted IDD). Column (2) (3) (4) and (5) report results from OLS regressions of institutional ownership on the indicator for the recognition of the IDD using the list of IDD cases following Qiu and Wang (2018) to construct IDD indicator instead of the IDD case list following Klasa et al. (2018). The sample spans the 1989-2018 period. Institutional shareholding serves as the dependent variable in my regression. Besides, I also introduce firm fixed effects (FE), state-of-incorporation-by-year FE, and standard industrial classification industry-by-year FE. Continuous variables, except state-level variables, are winsorized at their 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at the state level (t -statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable				
Independent Variables	(1) Institutional shareholding	(2) Institutional shareholding	(3) Institutional shareholding	(4) Institutional shareholding	(5) Institutional shareholding
Stacked IDD	-0.008*				
	(-1.794)				
Alternative IDD		-0.007*	-0.008**	-0.009**	-0.009***
		(-1.816)	(-2.149)	(-2.354)	(-3.966)
NCA	0.006***		0.003**	0.003***	0.003***
	(3.327)		(2.660)	(2.727)	(4.586)
Expense ratio	-0.001			-0.000**	-0.000
	(-1.159)			(-2.275)	(-0.073)
Firm size	0.077***				0.084***
	(18.494)				(31.491)
Cash holding	0.029***				0.034***
	(2.705)				(4.159)
Leverage	-0.095***				-0.096***
	(-7.739)				(-9.659)
MTB	0.010***				0.008***
	(14.142)				(15.560)
ROE	0.023***				0.025***
	(7.458)				(11.438)
Stock volatility	-0.107***				-0.117***
	(-9.692)				(-12.410)
Turnover	0.001***				0.001***
	(9.698)				(14.445)
Constant	-0.036*	0.465***	0.454***	0.458***	-0.018
	(-1.696)	(309.457)	(101.251)	(105.905)	(-1.135)
Observations	118,244	92,713	90,383	88,388	83,218
R-squared	0.847	0.775	0.814	0.815	0.852
Company FE	-	YES	YES	YES	YES
State-by-Year FE	-	YES	YES	YES	YES
Industry-by-Year FE	-	YES	YES	YES	YES
Company-Cohort FE	YES	-	-	-	-
Event-Year- Cohort FE	YES	-	-	-	-

3.7.3 Robustness test using an alternative IDD case list

In this study, I investigate the effects of the adoption of Inevitable Disclosure Doctrine (IDD), which serves to safeguard trade secrets by placing limitations on employees' disclosure of proprietary information. To identify states that have implemented the IDD, I utilize an IDD indicator based on a case list provided by Klasa et al. (2018). However, an alternative IDD case list is presented by Qiu and Wang (2018), which differs from the Klasa et al. (2018) list in terms of the timing and the states where IDD recognition occurred. To ensure the robustness of my findings, I conduct additional analyses by reconstructing the IDD indicator using the Qiu and Wang (2018) list. Subsequently, I rerun my baseline regression to ensure the consistency of my results. The dataset employed in my analysis comprises 90,383 observations spanning the period from 1989 to 2018.

I reconstruct the IDD indicator following Qiu and Wang (2018). In their study, Qiu and Wang (2018) documented 24 court-ruling events that acknowledged the IDD, as well as 10 instances where it was rejected in the US from 1960 to 2014. In order to account for the time required by institutional investors to adjust their shareholding levels in response to the exogenous shock caused by the adoption or rejection of the IDD, I assign a value of 1 to the IDD dummy starting from the second year after the state court adopted the IDD. Prior to the adoption, the IDD dummy is assigned a value of 0 for all preceding years. Similarly, when the previously adopted IDD is subsequently rejected, the IDD dummy is reset to 0 from 1, starting from the second year after the state court rejected the IDD.

For the state of California, I introduce a modified approach to reflect its unique IDD adoption and subsequent Supreme Court overrule. Specifically, the IDD indicator is set to 0.5 after the adoption of the IDD and subsequent Supreme Court overrule, and it is further reduced to 0 after the later IDD rejection.

In the case of North Carolina, I employ a similar approach to account for its state court's initial partial adoption and subsequent full adoption of the IDD. The IDD indicator is set to 0.5 when the state court first attempted but did not fully adopt the IDD, and it is increased to 1 after the court later fully adopts the doctrine. Subsequently, when the court rejects the IDD, the IDD indicator is reverted from 1 to 0.

For states that maintain a consistent IDD status throughout my study period, the IDD dummy is uniformly set to 0. Additionally, it is important to note that I exclude states that solely rejected the IDD without any prior adoption, as my analysis focuses specifically on changes in the IDD status of state courts.

The consistency of the results presented in Column (2), (3), (4), and (5) of ***Table 3.6*** reinforces the findings observed in my baseline regression analysis. These results indicate a significant association between the recognition of the IDD and the institutional shareholding of the affected companies, even after controlling for the same set of variables as in my baseline regression model.

3.7.4 Dynamic difference-in-differences estimation

My study employs the difference-in-differences (DID) methodology in my baseline regression analysis. This approach relies on the crucial identification assumption that in the absence of the IDD, the treatment and control groups would display similar trends. Specifically, I anticipate that the change in institutional ownership among firms headquartered in states that adopted the IDD would mirror the trend observed for firms headquartered in states that have not adopted the IDD. Deviation from this parallel trend assumption would indicate the presence of reverse causality, suggesting that factors other than the IDD were responsible for the observed results. Such evidence might manifest as a declining trend in institutional ownership for affected firms prior to the implementation of the IDD.

In this section, my objective is to evaluate the validity of the parallel trend assumption in my study sample. I achieve this by scrutinizing the temporal relationship between changes in institutional ownership and the adoption of the IDD. Following the previous literature, like Klasa et al. (2018), I re-estimate Equation (1) after replacing IDD with seven indicator variables ($\boldsymbol{IDD\ Adoption^{-3}}$,$\boldsymbol{IDD\ Adoption^{-2}}$, $\boldsymbol{IDD\ Adoption^{-1}}$, $\boldsymbol{IDD\ Adoption^{0}}$, $\boldsymbol{IDD\ Adoption^{+1}}$, $\boldsymbol{IDD\ Adoption^{+2}}$, and $\boldsymbol{IDD\ Adoption^{+3+}}$) as the following:

where $\boldsymbol{i}$ indexes firms; $\boldsymbol{k}$ indexes state of location; $\boldsymbol{l}$ indexes state of incorporation; $\boldsymbol{t}$ indexes years; $\boldsymbol{FirmFE}$, $\boldsymbol{IndustrybyYearFE}$ and $\boldsymbol{StatebyYearFE}$ are firm, four-digit-SIC industry-by-year and state-of-incorporation-by-year fixed effects respectively. $\boldsymbol{IO}_{\boldsymbol{i,k,l,t}}$ is the dependent variable of interest. Each $\boldsymbol{IDD\ Adoption}$ variable indicates the time relative to the adoption year. The key variables of interest, $\boldsymbol{IDD\ Adoption^{-3}}$, $\boldsymbol{IDD\ Adoption^{-2}}$, $\boldsymbol{IDD\ Adoption^{-1}}$, $\boldsymbol{IDD\ Adoption^{0}}$, $\boldsymbol{IDD\ Adoption^{+1}}$, $\boldsymbol{IDD\ Adoption^{+2}}$, and $\boldsymbol{IDD\ Adoption^{+3+}}$, are equal to one if the firm is headquartered in a state that will adopt the IDD in three years, will adopt the IDD in two years, will adopt the IDD in one year, adopts the IDD in the current year, adopted the IDD one year ago, adopted the IDD two years ago, or adopted the IDD three or more years ago, respectively, and zero otherwise. I focus on the coefficients on the indicators $\boldsymbol{IDD\ Adoption^{-3}}$, $\boldsymbol{IDD\ Adoption^{-2}}$ and $\boldsymbol{IDD\ Adoption^{-1}}$, because their magnitude and significance indicate whether there are differences in intuitional shareholding between treated firms and their control firms prior to the adoption of IDD.

The results are shown in ***Table 3.7***. To explain the results, taking the first column (dynamic regression without control variables) as an example, I find that the coefficients on $IDD\ Adoption^{-3}$, $IDD\ Adoption^{-2}$ and $IDD\ Adoption^{-1}$are not significantly different from zero, suggesting that the parallel trends assumption of my difference-in-differences approach is not violated. In other words, the pretreatment trends of treated and control firms are statistically indistinguishable. The results also indicate that the coefficients on $\boldsymbol{IDD\ Adoption^{0}}$ and $IDD\ Adoption^{+1}$ are small in magnitude and not statistically significant, while the coefficients on $\boldsymbol{IDD\ Adoption^{+2}}$, and $\boldsymbol{IDD\ Adoption^{+3+}}$ statistically significant, which means the effect of IDD emerges two years after the its adoption, but not before, and is consistent with the intuition that it may take a longer time for IDD to impose an effect on institutional shareholding. This finding further mitigates concerns of reverse causality and also supports a causal effect.

Table 3.7. Dynamic difference-in-differences estimation

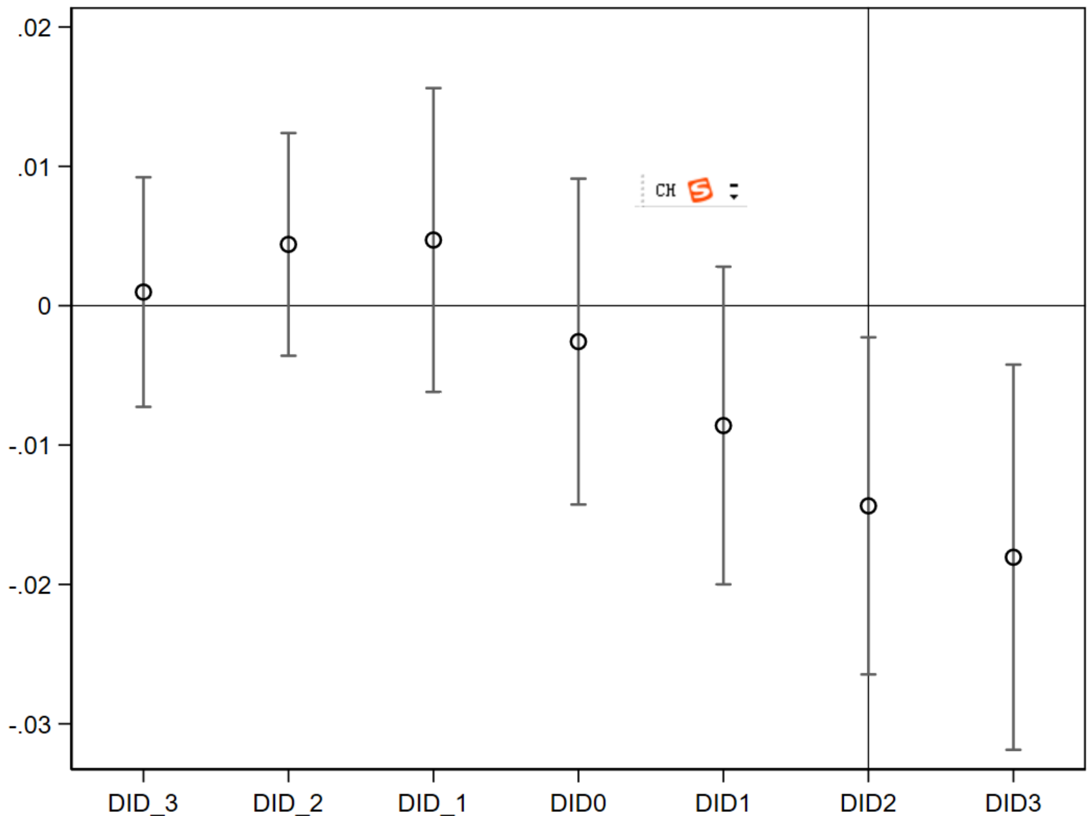
This table reports results from OLS regressions of institutional ownership on the indicator for the timing of state courts' adoptions of the IDD. The sample spans the 1989-2018 period. The key variables of interest are $IDD\ Adoption^{-3}$, $IDD\ Adoption^{-2}$, $IDD\ Adoption^{-1}$, $IDD\ Adoption^{0}$, $IDD\ Adoption^{+1}$, $IDD\ Adoption^{+2}$, and $IDD\ Adoption^{+3}$, which are equal to one if the firm is headquartered in a state that will adopt the IDD in three years, will adopt the IDD in two years, will adopt the IDD in one year, adopts the IDD in the current year, adopted the IDD one year ago, adopted the IDD two years ago, or adopted the IDD three or more years ago, respectively, and zero otherwise. Besides, I also introduce firm fixed effects (FE), state-of-incorporation-by-year FE, and standard industrial classification industry-by-year FE. Continuous variables, except state-level variables, are winsorized at their 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at the state level (t -statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Dependent variable		
Independent Variables	(1) Institutional shareholding	(2) Institutional shareholding	(3) Institutional shareholding
IDD Adoption^{-3}	0.001	0.001	0.001
	(0.198)	(0.370)	(0.261)
IDD Adoption^{-2}	0.004	0.005	0.003
	(0.920)	(1.025)	(0.753)
IDD Adoption^{-1}	0.004	0.003	0.002
	(0.723)	(0.552)	(0.445)
IDD Adoption^{0}	-0.002	-0.003	-0.006
	(-0.368)	(-0.573)	(-1.115)
IDD Adoption^{+1}	-0.008	-0.010	-0.011*
	(-1.262)	(-1.542)	(-1.894)
IDD Adoption^{+2}	-0.014*	-0.014**	-0.016**
	(-1.986)	(-2.008)	(-2.362)
$\text{IDD Adoption}^{+3+}$	-0.018**	-0.018**	-0.011
	(-2.184)	(-2.214)	(-1.544)
NCA	NO	YES	YES
Firm Controls	NO	NO	YES
Constant	0.400***	0.393***	-0.563***
	(97.040)	(64.500)	(-15.861)
Observations	101,784	101,784	101,784
R-squared	0.822	0.822	0.853
Company FE	YES	YES	YES
State-by-Year FE	YES	YES	YES
Industry-by-Year FE	YES	YES	YES

I additionally provide the dynamic difference-in-differences (DID) regression results in graphical form, displayed below. By focusing on the first column of the graph (representing the dynamic regression without control variables), I am able to depict the direct and dynamic effects of IDD adoption. The graphs visually demonstrate that, prior to the adoption of the IDD, treatment and control companies exhibit parallel trends that are statistically indistinguishable. Moreover, the findings reveal that it takes approximately two years for the IDD to manifest its influence on institutional shareholding. Taken together, these empirical results alleviate concerns regarding reverse causality and provide supporting evidence for a causal effect of the IDD.

Graph 3.1. Dynamic difference-in-differences regression

This graph reports results from OLS regressions of institutional ownership on the indicator for the timing of state courts' adoptions of the IDD without control variables, which reflects the dynamic effects of IDD. The confidence interval is 95%. The sample spans the 1989-2018 period. The key variables of interest are DID_3, DID_2, DID_1, DID0, DID1, DID2, and DID3, which are equal to one if the firm is headquartered in a state that will adopt the IDD in three years, will adopt the IDD in two years, will adopt the IDD in one year, adopts the IDD in the current year, adopted the IDD one year ago, adopted the IDD two years ago, or adopted the IDD three or more years ago, respectively, and zero otherwise. Besides, I also introduce firm fixed effects (FE), state-of-location-by-year FE, and four-digit standard industrial classification industry-by-year FE. Standard errors are corrected for heteroskedasticity and clustering at the state level (*t* -statistics are in parentheses).



3.7.5 Propensity Score Matching (PSM) Analysis

The potential for bias arises when differences in treatment outcomes between treated and untreated groups are influenced by a factor that predicts treatment rather than the treatment itself. Randomized experiments effectively address this issue by ensuring unbiased estimation of treatment effects. Through randomization, treatment groups are expected to be balanced on average for each covariate, as dictated by the law of large numbers. However, in observational studies, treatment assignments are typically non-random. To mitigate the bias introduced by non-random treatment assignment, matching techniques are employed to emulate randomization and create comparable samples of units that either received or did not receive the treatment.

In my pursuit of a more precise investigation into the relationship between the recognition of the IDD and institutional ownership, I utilize Propensity Score Matching (PSM). PSM is a statistical matching technique used to estimate the effect of a treatment, policy, or intervention by accounting for covariates that predict treatment receipt. By employing PSM, I aim to reduce bias caused by confounding variables that may exist when comparing outcomes among units that received the treatment versus those that did not (Rosenbaum and Rubin, 1983).

Specifically, I compare the institutional shareholding of companies headquartered in states that have recognized the IDD with those headquartered in states that have not recognized the IDD but are otherwise comparable. I designate firms headquartered in IDD-recognized states as the treatment group, while firms in non-IDD-recognized states serve as the control group. For each year, matching techniques are employed to pair treatment firms with control firms based on firm characteristics used as control variables in my baseline regression. The probability of being assigned to the treatment or control group is estimated using a logit regression that includes all control variables, year, state of headquarter, and industry fixed effects. Subsequently, propensity scores derived from this logit estimation are utilized for matching within a caliper of 0.01 without replacement. The outcomes presented in ***Table 3.8*** demonstrate consistency with my baseline regression results. Specifically, I observe that the firm characteristics of the treatment and control groups are similar for all control variables used in the matching process. Furthermore, the coefficient estimate on the IDD indicator is -0.00658 and statistically significant at the 5% level, indicating a negative treatment effect of IDD adoption on institutional ownership. Overall, my findings, which account for potential sample selection bias through the implementation of the propensity score matching (PSM) method, reinforce the results obtained from my baseline analysis.

Table 3.8. Propensity Score Matching (PSM) test

This table reports results from OLS regressions of institutional ownership on the indicator for the recognition of the IDD using Propensity Score Matching (PSM). The sample spans the 1989-2018 period. *Institutional Ownership* serves as the dependent variable in my regression. *IDD* is an indicator variable equal to one if the firm is headquartered in a state whose courts recognize the IDD, and zero otherwise. Besides, I also introduce firm fixed effects (FE), state-of-incorporation-by-year FE, and standard industrial classification industry-by-year FE. Continuous variables, except state-level variables, are winsorized at their 1st and 99th percentiles. Standard errors are corrected for heteroskedasticity and clustering at the state level (*t* -statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Variables	Mean		t-test			V(T)/V(C)
	Treated	Control	%bias	t	p>|t|	
Institutional shareholding	0.494	0.494	-0.2	-0.15	0.879	1.04
NCA	2.219	3.323	-53.5	-36.73	0	0.91*
agencycosts1	1.822	1.647	3.8	2.63	0.009	1.24*
size	5.729	5.760	-1.5	-1.06	0.289	1.12*
cash	0.208	0.182	11.9	8.24	0	1.19*
leverage	0.227	0.236	-4.1	-2.83	0.005	1.01
MTB	2.144	1.997	6.2	4.3	0	1.15*
ROE	-0.095	-0.090	-1.2	-0.8	0.424	1.10*
volatility	0.187	0.192	-2.9	-1.97	0.049	0.97
turnover	16.488	14.73	11.6	8.13	0	1.36*

Independent Variables	***Dependent variable***
	Institutional shareholding
IDD	-0.006**
	(-2.414)
NCA	0.002***
	-3.915
Expense ratio	-0.000
	(-0.217)
Firm size	0.082***
	-38.494
Cash holding	0.031***
	-3.955
Leverage	-0.097***
	(-10.146)
MTB	0.008***
	-16.494
ROE	0.026***
	-11.504
Stock volatility	-0.119***
	(-13.451)
Turnover	0.001***
	-18.239
Constant	-0.005
	(-0.377)
Observations	83220
R-squared	0.85
Company FE	YES
Industry-Year FE	YES
State-Year FE	YES

3.7.6 Placebo test

Bertrand et al. (2004) have demonstrated that when conducting difference-in-differences (DID) analyses using long time series data, there is a risk of overestimated t-statistics and significance levels due to correlation within unit observations. Given that the recognition of the IDD may be a coincidental event influencing the shareholding ratio of institutional investors, and that unobserved factors could confound the actual influential factors, the baseline regression results can be questionable. Consequently, it is crucial to examine whether changes in institutional shareholding can be attributed solely to IDD adoption or if they are influenced by other unobserved factors.

Therefore, to assess the causality between IDD recognition and changes in institutional shareholding, I conduct placebo tests as part of my analysis. These tests involve creating fictitious changes in IDD recognition that precede the actual changes in states where the recognition occurred. Subsequently, I re-estimate the baseline regression using these placebo IDD indicator variables within the difference-in-differences framework. Specifically, I generate fictitious IDD changes that occur 3 to 5 years prior to the actual IDD changes within each recognition-change state.

If the observed impact on institutional shareholding indeed stems from the recognition of IDD and represents a causal relationship, I would anticipate observing a lack of negative and statistically significant relationship between institutional shareholding and the randomly assigned recognition of IDD in these placebo tests. The ordinary least squares (OLS) regression results presented below indicate that while the coefficients on the placebo IDD indicators are negative, they lack statistical significance. Furthermore, the magnitude of these coefficients diminishes as I move further away from the actual change instances.

Table 3.9. Placebo test

This table reports coefficients from OLS regressions of institutional ownership on the indicator for fictitious recognition of IDD, firm fixed effects, state-of-incorporation-by-year fixed effects, and standard industrial classification industry-by-year fixed effects. I winsorize continuous variables at the 1 and 99 percent levels. I provide variable definitions in the Appendix. *** denotes significance at the 1% level; ** denotes significance at the 5% level; * denotes significance at the 10% level.

	Dependent variable			
	(1)	(2)	(3)	(4)
Independent Variables	Institutional shareholding	Institutional shareholding	Institutional shareholding	Institutional shareholding
IDD_2	-0.004			
	(-0.939)			
IDD_3		-0.002		
		(-0.459)		
IDD_4			-0.00180	
			(-0.33633)	
IDD_5				0.000
				(0.033)
NCA	0.003***	0.002***	0.002***	0.002***
	(3.728)	(3.362)	(3.326)	(3.383)
Expense ratio	0.000	0.000	0.000	0.000
	(0.386)	(0.425)	(0.347)	(0.174)
Firm size	0.086***	0.086***	0.086***	0.086***
	(30.770)	(31.430)	(30.969)	(27.794)
Cash holding	0.034***	0.034***	0.034***	0.033***
	(4.217)	(4.215)	(4.309)	(4.111)
Leverage	-0.101***	-0.102***	-0.103***	-0.105***
	(-10.582)	(-10.343)	(-10.594)	(-10.800)
MTB	0.009***	0.009***	0.009***	0.009***
	(17.873)	(18.558)	(18.886)	(19.165)
ROE	0.024***	0.023***	0.022***	0.022***
	(10.852)	(10.429)	(10.026)	(9.344)
Stock volatility	-0.112***	-0.112***	-0.113***	-0.114***
	(-11.790)	(-12.188)	(-12.590)	(-12.621)
Turnover	0.001***	0.001***	0.001***	0.001***
	(13.384)	(13.378)	(13.375)	(13.568)
Constant	-0.028*	-0.029*	-0.030**	-0.033**
	(-1.717)	(-1.895)	(-2.032)	(-2.088)
Observations	80,705	78,597	76,486	74,432
R-squared	0.862	0.861	0.861	0.860
Company FE	YES	YES	YES	YES
State-by-Year FE	YES	YES	YES	YES
Industry-by-Year FE	YES	YES	YES	YES

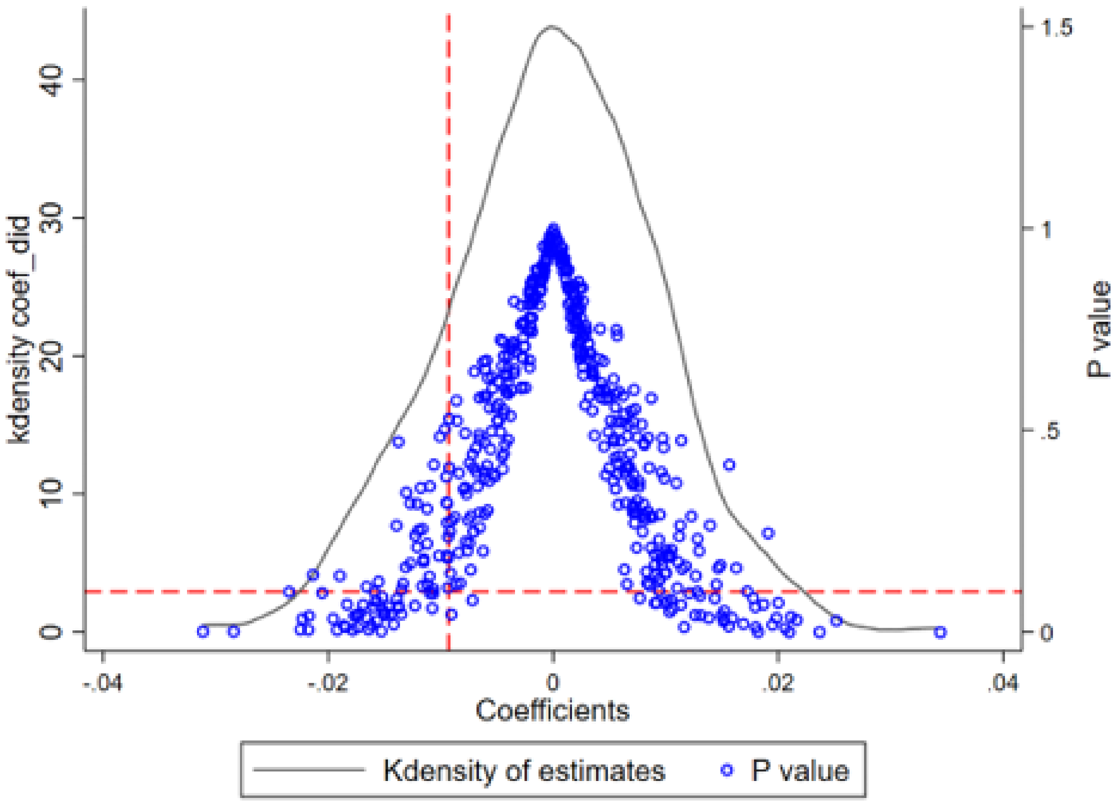
Furthermore, in order to mitigate the possibility of my findings being solely due to chance, I adopt a placebo test approach following the methodologies of Bertrand et al. (2004) and Guo and Masulis (2015). This test involves randomly assigning states to recognize the IDD, ensuring an equal probability for each state to adopt the IDD. By doing so, I ensure that any differences observed between and within states are not driven by systematic factors.

Specifically, for each year in which one or multiple states recognize the IDD, I randomly assign an equal number of states as the pseudo-treatment group, while the remaining states serve as the control group. I then estimate the baseline regressions using these pseudo-treatment states. The coefficient estimates of IDD adoption are saved, and this procedure is repeated 1000 times. The results, as illustrated in ***Graph 3.2***, reveal that the pseudo-regression coefficients conform to a normal distribution with a mean of 0. Moreover, the corresponding P-values are predominantly greater than 0.1.

Additionally, the vertical dotted line in the figure represents the actual regression coefficient obtained in the main regression analysis, specifically -0.00891, as shown in Column (2) of ***Table 3.2***. Notably, this value falls within the tail of the overall distribution of pseudo-regression coefficients. In summary, these results indicate that the association between IDD recognition and institutional shareholding, as documented in my main tests, is unlikely to be a spurious finding.

Graph 3.2. Distribution of coefficients of placebo test

This table reports the distribution of coefficients from OLS regressions of institutional ownership on the indicator for fictitious recognition of IDD for 1000 times. The placebo test is conducted by randomly assigning states recognizing the IDD, which ensures that each state has the same chance to recognize the IDD and thus guarantees that any difference between and within states is not systematic. The horizontal axis represents the coefficients of the regression result, and the vertical axis represents the corresponding P values. The vertical dotted line in the figure is the real regression coefficient obtained in my main regression presented above, that is -0.00891 shown in column (2) of Table 3.2.



3.8. Conclusion

My research demonstrates that restricted executive mobility reduces corporate institutional ownership through the utilization of a difference-in-differences framework, incorporating the staggered recognition of IDD, spanning the period from 1989 to 2018. The recognition of IDD results in a plausibly exogenous decrease in executive mobility by enhancing the firm's ability to prevent employees knowledgeable of its trade secrets from joining competitors or establishing new companies. I establish a causal channel between executive mobility and institutional ownership, positing that managers engage in opportunistic behavior to address career concerns resulting from mobility restrictions.

My findings reveal a significant decrease in institutional shareholding, particularly among long-term and activist investors, following the adoption of IDD. These results remain robust even after accounting for high-dimensional fixed effects. Notably, the decline in institutional ownership manifests two years after IDD recognition, mitigating concerns of reverse causality. Moreover, the effects are particularly pronounced in industries that prioritize knowledge-intensive activities. To bolster the credibility of my empirical results, I perform various diagnostic and robustness tests, including placebo tests, PSM tests, controlling for Non-Compete Agreements (NCA), using alternative IDD adoption dates, and controlling for the rejection of IDD. All of these tests consistently support my main regression results, corroborating the argument that institutional investors divest their shares when dissatisfied with management quality.

This paper prompts reflection among executives on strategies to uphold strong corporate governance practices while preserving institutional shareholding. Additionally, I highlight the need for further examination of distinct categories of institutional investors. Importantly, my study carries implications for policy discussions. Although approximately 20 out of 50 US states have adopted IDD (refer to the data and methodology section for the list of recognized states), many states are still engaged in deliberations regarding compliance, partly due to limited understanding of IDD's economic effects.

Appendix

Appendix A Precedent-setting legal cases adopting or rejecting the Inevitable Disclosure Doctrine. The table lists the precedent-setting legal cases in which state courts adopted the Inevitable Disclosure Doctrine (IDD) or rejected it after adopting it. The states omitted from the table did not consider or considered but rejected the IDD. The text of all court decisions is available from Google Scholar.

Appendix A

State	Precedent-setting case(s)	Date	Decision
AR	*Southwestern Energy Co. v. Eickenhorst*, 955 F. Supp. 1078 (W.D. Ark. 1997)	3/18/1997	Adopt
CT	*Branson Ultrasonics Corp. v. Stratman*, 921 F. Supp. 909 (D. Conn. 1996)	2/28/1996	Adopt
DE	*E.I. duPont de Nemours & Co. v. American Potash & Chem. Corp.*, 200 A.2d 428 (Del. Ch. 1964)	5/5/1964	Adopt
FL	*Fountain v. Hudson Cush-N-Foam Corp.*, 122 So. 2d 232 (Fla. Dist. Ct. App. 1960)	7/11/1960	Adopt
	Del Monte Fresh Produce Co. v. Dole Food Co. Inc., 148 F. Supp. 2d 1326 (S.D. Fla. 2001)	5/21/2001	Reject
GA	*Essex Group Inc. v. Southwire Co.*, 501 S.E.2d 501 (Ga. 1998)	6/29/1998	Adopt
IL	*Teradyne Inc. v. Clear Communications Corp.*, 707 F. Supp. 353 (N.D. 111. 1989)	2/9/1989	Adopt
IN	*Ackerman v. Kimball Int'l Inc.*, 652 N.E.2d 507 (Ind. 1995)	7/12/1995	Adopt
IA	*Uncle B's Bakery v. O'Rourke*, 920 F. Supp. 1405 (N.D. Iowa 1996)	4/1/1996	Adopt
KS	*Bradbury Co. v. Teissier-duCros*, 413 F. Supp. 2d 1203 (D. Kans. 2006)	2/2/2006	Adopt
MA	*Bard v. Intoccia*, 1994 U.S. Dist. LEXIS 15,368 (D. Mass. 1994)	10/13/1994	Adopt
MI	*Allis-Chalmers Manuf. Co. v. Continental Aviation & Eng. Corp.*, 255 F. Supp. 645 (E.D. Mich. 1966)	2/17/1966	Adopt
	CMI Int'l, Inc. v. Intermet Int'l Corp., 649 N.W.2d 808 (Mich. Ct. App. 2002)	4/30/2002	Reject
MN	*Surgidev Corp. v. Eye Technology Inc.*, 648 F. Supp. 661 (D. Minn. 1986)	10/10/1986	Adopt
MO	*H&R Block Eastern Tax Servs. Inc. v. Enchura*, 122 F. Supp. 2d 1067 (W.D. Mo. 2000)	11/2/2000	Adopt
NJ	*Nat'l Starch & Chem. Corp. v. Parker Chem. Corp.*, 530 A.2d 31 (N.J. Super. Ct. 1987)	4/27/1987	Adopt
NY	*Eastman Kodak Co. v. Powers Film Prod.*, 189 A.D. 556 (N.Y.A.D. 1919)	12/5/1919	Adopt
NC	*Travenol Laboratories Inc. v. Turner*, 228 S.E.2d 478 (N.C. Ct. App. 1976)	6/17/1976	Adopt
OH	*Procter & Gamble Co. v. Stoneham*, 747 N.E.2d 268 (Ohio Ct. App. 2000)	9/29/2000	Adopt
PA	*Air Products & Chemical Inc. v. Johnson*, 442 A.2d 1114 (Pa. Super. Ct. 1982)	2/19/1982	Adopt
TX	*Rugen v. Interactive Business Systems Inc.*, 864 S.W.2d 548 (Tex. App. 1993)	5/28/1993	Adopt
	Cardinal Health Staffing Network Inc. v. Bowen, 106 S.W.3d 230 (Tex. App. 2003)	4/3/2003	Reject
UT	*Novell Inc. v. Timpanogos Research Group Inc.*, 46 U.S.P.Q.2d 1197 (Utah D.C. 1998)	1/30/1998	Adopt
WA	*Solutec Corp. Inc. v. Agnew*, 88 Wash. App. 1067 (Wash. Ct. App. 1997)	12/30/1997	Adopt

Appendix B

Variable	Definition
Dependent Variables	
Institutional shareholding	Ratio of shares outstanding held by institutional investors to the total number of shares outstanding
Long term	Long-term institutional shareholding for each company
Short term	Short-term institutional shareholding for each company
Bank	Institutional shareholding held by banks for each company
Insurance	Institutional shareholding held by insurance companies for each company
Investment advisor	Institutional shareholding held by investment advisers (including mutual fund companies) for each company
Pension	Institutional shareholding held by pensions and endowments for each company
Expense ratio	Operating expenses (xopr) divided by revenues (ni)
Asset utilization	Annual sales (sale) divided by total assets (at)
Independent Variable	
IDD	IDD is an indicator variable equal to one if the firm is headquartered in a state whose courts recognize the IDD, and zero otherwise.
Control Variables	
NCA	Noncompetition Agreement Enforceability Index following Garmaise (2011)
Firm size	Natural logarithm of total assets (at)
Cash holding	Cash and short-term investments (che) normalized by total assets (at)
Leverage	Total debt (dltt+dlc) normalized by total assets (at)
MTB	Ratio of market value of equity (csho*prcc_f) to book value of equity (at-dltt-dlc)
ROE	Net income (ni) divided by total shareholders' equity value (csho*prcc_f)
Stock volatility	Standard deviation of quote-midpoint daily returns
Turnover	Average ratio of monthly trading volume to the number of shares outstanding

Chapter 4. TAKEOVER THREAT AND DEFAULT RISK: A CASUAL REEVALUAITON

Abstract

In my investigation, I re-evaluate the causal relationship between takeover threats and corporate default risk. In addition to conventional proxies for takeover threat, I also introduce the implementation of Second-Generation State-level Antitakeover Laws, employing a difference-in-differences approach to address associated endogeneity concerns. my research is the first to demonstrate that firms incorporated in influenced states experience a reduction in default probability following the enactment of Control Share Acquisition laws. Furthermore, I observe an increase in agency costs of equity, suggesting that weakened external monitoring mechanisms may prompt managers to exercise discretion in reducing debt usage. Therefore, I consider agency costs of equity can be the possible channel between takeover threats and default risk. This effect is more pronounced in companies with a high institutional shareholding, substantiating the notion that the decline in default risk for affected firms results from deteriorated corporate governance and heightened agency conflicts. Lastly, I report that the diminished default risk following the adoption of CS laws undermines shareholder interests and that managerial tendencies towards underinvestment following the adoption of CS laws indicate a preference for a "enjoy a quiet life" among executives. My findings remain robust following a series of validity assessments, further strengthening the academic contribution of my study.

4.1. Introduction

Default, one of the most disruptive occurrences in a company's lifespan, can lead to the limitation of production caused by supply chain breakdowns and workforce turnover, in addition to regulatory and enforcement costs and reduced customer loyalty (Brogaard et al., 2017). Notably, the occurrence of default is pervasive. The Securities and Exchange Commission reveals that during the period spanning from 1996 to 2008, 10% to 20% of of non-financial companies reported violation of a financial covenant in a credit agreement (Nini et al., 2012). Therefore, the possibility of default is of interest to various stakeholders such as debtholders, customers, suppliers, policymakers, and present and future investors, making it imperative to predict default (Traczynski, 2017; Sha et al., 2020).

According to Merton's (1974) model, the concept of corporate equity can be understood as resembling a call option tied to the underlying value of a firm's assets, whereby the strike price corresponds to the face value of the company's debt. This means that default occurs when the value of the firm's assets diminishes below its debt face value. Default risk represents the potential hazard undertaken by a lender, signifying the possibility that a borrower will fail to fulfill the obligatory payments associated with a debt. Notably, default risk may be not solely determined by variations in debt levels. Under circumstances where debt remains constant, the volatility or precariousness surrounding the overall cash flows of the firm also presumably contributes to the likelihood of default.

Extensive research has focused on identifying factors that determine a firm's default risk

(Giesecke et al., 2011; Hsu et al., 2015; Bennett et al., 2015; Brogaard et al., 2017). However, limited and inconclusive research has been conducted on the relationship between susceptibility to takeover and default risk. To address this gap, I causally re-investigate the relationship between takeover threats and default risk by introducing the adoption of state-level anti-takeover laws to utilize the difference-in-differences methodology.

Different theories have emerged regarding the consequences of anti-takeover provisions. The "Managerial Entrenchment Hypothesis" suggests a detrimental impact on stockholders' interests (Manne, 1965; Walkling and Long, 1984; Williamson, 1975), while the "Shareholder Interests Hypothesis" proposes that increased anti-takeover protection can enable managerial activities that primarily benefit shareholders (Grossman and Hart, 1980; Knoeber, 1986; Scherer, 1988).

The "Managerial Entrenchment Hypothesis" acknowledges an active takeover market as an external mechanism effectively disciplining managers (Fama and Jensen, 1983; Jensen and Ruback, 1983; Scharfstein, 1988; Lel and Miller, 2015). In cases of poor managerial performance, companies become more susceptible to takeovers, resulting in managerial replacements (Manne, 1965). Nevertheless, anti-takeover protections are believed to weaken this disciplinary mechanism by safeguarding managers from replacement, providing long-term contracts that alleviate career concerns, and granting additional voting power (Easterbrook and Fischel, 1981; Kesner and Dalton, 1985). Consequently, these provisions foster managerial entrenchment and give rise to agency costs of equity.

Motivated by self-interest, managers may engage in value-depleting endeavors, such as "empire building" (Jensen and Meckling, 1976; Scharfstein, 1988; Stein, 1988; Norton, 1998). Expanding on this viewpoint, weakened disciplinary mechanisms, facilitated by reduced likelihood of takeovers, amplifies default risk by exacerbating agency conflicts with external stakeholders (Driss et al., 2021). This, in turn, leads to reduced cash flows available for debt payments, ultimately elevating the risk of default (Balachandran et al., 2022).

In contrast, Garvey and Hanka (1999) provide empirical evidence indicating a diminished reliance on debt among firms subject to second-generation state-level anti-takeover legislation. This observed tendency can be attributed to a reduced probability of hostile takeover threats, which traditionally incentivize managers to increase their utilization of debt (Zwiebel, 1996; Novaes and Zingales, 1995). The diminished likelihood of managerial termination resulting from the implementation of anti-takeover laws mitigates managerial career concerns (Grossman and Hart, 1980; Knoeber, 1986; Scherer, 1988; Stein, 1988), thereby granting managers the latitude to exercise discretion in capital structure decisions that may not necessarily optimize shareholder wealth (Jung et al., 1996). Consequently, managers are inclined to curtail debt issuance beyond the preferences of shareholders, as the presence of debt imposes constraints on their strategic actions (Grossman and Hart, 1982; Stulz, 1990).

Moreover, the financial leverage ratio can function as a prominent predictor for forecasting default risk (Traczynski, 2017; Cathcart et al., 2019). As previously indicated, default transpires when the value of a company's assets declines below its debt's face value (Merton, 1974), thus substantiating the concept that default risk can be evaluated through this ratio. Furthermore, the trade-off theory of capital structure, as initially posited by Kraus and Litzenberger (1973), illuminates that augmented leverage amplifies the ex-ante expenses linked to financial distress. To summarize, enhanced anti-takeover safeguards possess the capacity to diminish default risk, thereby conferring advantages upon debtholders.

While the "Shareholder Interests Hypothesis" postulates that anti-takeover protection may stimulate managerial activities that prioritize shareholders. It is crucial to acknowledge that, in addition to agency costs of equity, anti-takeover provisions can also generate agency costs of debt. Managers often prioritize the interests of shareholders over those of debtholders in cases where their interests diverge, leading to debt-related agency costs as debtholders curtail their capital allocation (Kim and Sorensen, 1986). This dynamic can hinder the procurement of additional debt capital, particularly for financially distressed firms, ultimately heightening the risk of default.

For instance, managers may opt to allocate resources towards long-term investments instead of focusing on defensive strategies against takeovers or short-term profitability management (Pugh et al., 1992), which is commonly known as overinvestment. However, it is crucial to acknowledge that default risk is not solely influenced by variations in the level of debt. Even when the debt level remains constant, the riskiness of overall cash flows is believed to impact default risk. Consequently, investments in long-term projects that escalate risk and erode value can elevate default risk, as there is no guarantee of generating long-term value from these endeavors, despite their benefits for shareholders.

In this study, I investigate the above two conflicting perspectives by employing a difference-in-differences estimation strategy, introducing the staggered adoption of control share acquisition laws across US states. This approach allows me to compare changes in companies incorporated in states that adopt these laws with those in companies incorporated elsewhere (Gormley and Matsa, 2016; Klasa et al., 2018; Ali et al., 2019). The distinctive aspect of the staggered adoption of anti-takeover laws enables me to address potential biases related to the timing of the laws, as discussed by Bertrand and Mullainathan (2003). These laws primarily make it more challenging for hostile takeovers of target firms, without impacting friendly mergers or acquirers incorporated in the adopting states.

My analysis focuses on control share acquisition laws among second-generation anti-takeover laws, which aim to protect companies from proxy takeover threats. Previous studies have used various proxies to measure takeover threats. Cain et al. (2017) developed a takeover index that combines court decisions, state anti-takeover laws, macroeconomic conditions, and firm characteristics to estimate the likelihood of hostile takeovers. However, the index has been criticized for potential endogeneity issues since it includes endogenous firm characteristics and it may not generalize to acquisitions not classified as hostile (Karpoff and Wittry, 2018). In addition, the index represents a mixture effect of all the above-mentioned anti-takeover protections, which ignores which one provides the most powerful anti-takeover protection.

While second-generation anti-takeover laws are generally considered powerful (Karpoff and Wittry, 2018). Previous studies have focused on the effectiveness of business combination (BC) laws, based on Bertrand and Mullainathan's (2003) argument that only BC laws provide meaningful takeover protection. For example, Gormley and Matsa (2016) investigated managerial preferences in light of BC laws, as these laws have been extensively studied and their empirical setting aligns with Bertrand and Mullainathan (2003).

In my study, I aim to investigate the effects of anti-takeover protection, specifically focusing on the strength of such protection. Karpoff and Wittry (2018) challenge the notion that BC laws are the most stringent and argue that it is unclear which anti-takeover law offers the best protection against unsolicited takeovers. BC laws impose no restrictions on a bidder's ability to acquire shares but require a waiting period of two to five years for certain bidder-firm transactions, such as mergers or large asset sales, which can increase the bidder's expenses (Subramanian et al., 2010a). However, from a theoretical perspective, business combination regulations do not appear to provide stronger takeover protection.

The authors also highlight the stringency of control share acquisition regulations, as they have never been triggered. These laws suspend a bidder's voting rights until a majority of other shareholders decide to restore them, while the voting rights of current management remain unaffected. To successfully complete an acquisition, a bidder must secure supermajority support from disinterested shares, which increases the risk of failure and likely discourages many unsolicited bids in the first place.

Factors such as the political economy or the business cycle are unlikely to throw my analysis off. Studies like Romano (1987) and Bertrand and Mullainathan (2003) document that the anti-takeover laws' passage typically was not derived from the pressure of a large coalition of economic players in the state and illustrate that an omitted economic variable is unlikely to explain measured effects. Nevertheless, I control for related factors by including both location state-by-year and industry-by-year fixed effects in my analysis. Specifically, many companies are incorporated in a state other than the one in which they are situated, which provides me with the opportunity to include the location-state-by-year fixed effect. Totally, these high-dimensional fixed effects make me more confident that my finding of a significant coefficient is not attributable to unobserved sources of heterogeneous variation related to the firm's industry, location, or year of observation (Gormley and Matsa, 2014; 2016). Besides, I also control lobbying firms for specific state antitakeover laws identified by Karpoff and Malatesta (1989) and Gartman (2000) to address endogeneity issues.

Based on the discussion above, my results show that, on average, firms incorporated in states that have adopted the CS laws reduce their default risk by 18.2% relative to the mean of default risk during the sample period. The finding supports my prediction that managers reduce the use of debt or default risk at their discretion facing worse external monitoring mechanisms (Fama and Jensen, 1983; Jensen and Ruback, 1983; Lel and Miller, 2015; and Scharfstein, 1988; Garvey and Hanka, 1999). In my baseline regression setting, I lag the CS laws' adoption indicator by one year to allow enough time for affected companies to change their default risk, which mitigates concerns of reverse causality. More importantly, I find that such decreasing effect occurs one year after the passage of pay secrecy laws, which further mitigates concerns of reverse causality.

To further illustrate the underlying preferences of managers to decrease default risk, I also investigate the influence of anti-takeover protection on agency costs of equity and the effect of influenced default risk following the implement of CS laws on shareholder interests. I find that agency costs of equity increased and the decreased default risk hurts shareholders, which contradicts the findings of Balachandran et al. (2022). Besides, I find that, following the adoption of CS laws, managers tend to under-invest. This observation can be elucidated by emerging insights into agency conflicts, revealing that the increased adoption of anti-takeover provisions prompts managers to "Enjoy a quiet life", which ultimately undermines the interests of shareholders but provides advantages to debtholders (Bertrand and Mullainathan, 2003; Klock et al., 2005; Chava et al., 2009; Qiu and Yu, 2009; Gormley and Matsa, 2016). I also find that the effects of CS laws' adoption on default risk are more pronounced in companies with more institutional shareholding, especially long-term institutional shareholding. A series of robustness examinations are also conducted, including using alternative proxies of default risk and takeover threat level, parallel trend assumption checking, PSM, placebo test, stacked difference-in-difference estimation and so on.

My study contributes to the existing literature in several ways. Firstly, I expand the research on the determinants of corporate default risk. Previous studies have examined various factors that explain firm default risk, such as stock liquidity (Brogaard et al., 2017; Nadarajah et al., 2020), innovation performance (Hsu et al., 2015), and incentive structure (Bennett et al., 2015). I go further by investigating the effects of anti-takeover protection on default risk. While Balachandran et al. (2022) have already explored this relationship, my study differs in several aspects. Balachandran et al. (2022) argue that anti-takeover protection, as an exogenous shock to corporate governance, increases the likelihood of default derived from managerial opportunistic activities, which harms shareholder interests. In contrast, my research arrives at the opposite conclusion that decreased takeover threat is associated with a lower probability of default. Interestingly, I find that the reduced default risk following the adoption of control share (CS) laws negatively affects shareholder benefits, contradicting Balachandran et al. (2022). I illustrate that although corporate governance weakens after the implementation of anti-takeover laws, default risk does not necessarily increase. I provide an alternative perspective that increased usage of anti-takeover provisions leads managers to “enjoy a quiet life”, which harms shareholders but benefits debtholders (Bertrand and Mullainathan, 2003; Klock et al., 2005; Chava et al., 2009; Qiu and Yu, 2009; Gormley and Matsa, 2016). To support my hypothesis, I find evidence that managers tend to under-invest following the adoption of CS laws. Hence, my study contributes to the literature on the influence of managers' risk exposure on their business decisions, highlighting that traditional agency conflicts related to "private benefits" may not be the norm for managers to undertake activities that deviate from shareholders' interests.

I also address the endogeneity concerns. Balachandran et al. (2022) introduce a takeover index by Cain et al. (2017) to measure the threat of hostile takeovers, but it has been criticized for potential endogeneity issues since it includes endogenous firm characteristics and it may not generalize to acquisitions not classified as hostile (Karpoff and Wittry, 2018). Balachandran et al. (2022) attempted to mitigate endogeneity concerns to use a difference-in-differences (DiD) model by introducing the court rulings in Delaware in 1995, but their sample period was relatively short. In contrast, I employ a robust DiD methodology following Bertrand and Mullainathan (2003), considering staggered treatments at the state level through the introduction of CS laws. I also incorporate high-dimensional fixed effects based on the approach of Gormley and Matsa (2016) and utilize a larger sample covering the period from 1975 to 2007. Furthermore, I examine all takeover laws included in the hostile takeover index developed by Cain et al. (2017) to address concerns about potential omitted laws that may affect my findings.

My study is also related to the research conducted by Gormley and Matsa (2016), which investigates managers' incentive to play it safe and risk-taking decisions after being insulated by state-level takeover protection. They find that managers, with increased protection against hostile takeovers, can reduce their exposure to firm-specific idiosyncratic risk, like stock volatility and risk of distress, by pursuing diversifying acquisitions. They argue that managers tend to minimize unfavorable outcomes that could personally harm them, potentially at the expense of shareholder benefits (Sundheim, 2013). However, their study does not specifically focus on distress risk. In contrast, my research examines the influence of state-level anti-takeover protection (CS laws adoption) on managerial debt-taking discretion (e.g., Donaldson, 1969; Jensen, 1993; Jung et al., 1996) and, subsequently, default risk with a negative relationship. Additionally, their research primarily focuses on merger and acquisition activities with a relatively limited sample, whereas my study includes a broader sample encompassing all US-incorporated firms from 1975 to 2007.

Finally, I extend the investigation into the effects of CS laws, which has received limited attention thus far. I demonstrate that, in addition to business combination (BC) laws, CS laws also contribute to variations in corporate governance due to their robust anti-takeover protection. Moreover, I find that CS laws have a negative impact on default risk.

The structure of the study is outlined as follows: Section 4.2 presents an in-depth analysis of the pertinent literature to provide contextual background. Section 4.3 formulates hypotheses and delineates the associated predictions. Section 4.4 elucidates the data and variable construction methodologies employed in this study. Furthermore, Section 4.5 introduces the research design adopted to investigate the research questions. Section 4.6 thoroughly assesses the relationship between takeover threats and default risk. Section 4.7 delves into the examination of the impact of anti-takeover protection on agency costs of equity and the outcomes of shareholders. Section 4.8 presents the outcomes of robustness and diagnostic tests undertaken to validate the findings. Finally, Section 4.9 concludes the paper, summarizing the main findings and providing insights for future research directions.

4.2. Literature review

My literature review consists of two primary sections. The first section focuses on default risk and comprises two key components. Firstly, I delve into capital structure theory and discuss empirical evidence that supports the notion of leverage ratios as predictors of default risk. Secondly, I conduct a thorough review of related literature, exploring the effects of default risk on shareholder benefits. The second section of my review centers around theories pertaining to managerial decision-making following the implementation of takeover protections. I examine these theories in conjunction with supporting empirical evidence. Finally, I construct a set of hypotheses to establish the causal effects of state-level takeover protection on corporate default risk and subsequent shareholder benefits.

4.2.1 Capital structure and default risk

Default occurs when a company's cash flows are insufficient to meet its debt servicing costs and principal obligations. To support the view, Beaver (1966) initially confirms the strong predictive power of the cash flow to debt ratio at least five years before failure. This analysis cites Walter's (1957) "cash-flow" or "liquid-asset flow" model, which likens companies to reservoirs where liquid assets flow in and out, predicting solvency based on the depletion of this reservoir and failure to meet obligations. Altman (1968) supports this argument and introduces the Z-score, a weighted average of five accounting ratios, including operating efficiency, asset turnover, leverage ratio, asset liquidity, and earning power, as a bankruptcy prediction tool.

The trade-off theory of capital structure, proposed by Kraus and Litzenberger (1973), highlights the trade-off between the ex-ante costs of financial distress and the tax shields of debt (Modigliani and Miller, 1963). This theory emphasizes the pivotal role of leverage ratios in predicting default risk. Subsequent studies, such as Leland and Toft (1996), explore agency problems between debtholders and shareholders, particularly regarding the optimal leverage ratio. They find that short-term debt offers limited tax benefits compared to long-term debt, and it fosters greater incentive compatibility between creditors and shareholders while reducing "asset substitution" agency costs. Thus, when determining the ideal debt maturity, the trade-off between tax advantages and bankruptcy and agency costs must be considered.

Graham (2000) investigates the tax benefits derived from debt issuance using firm-specific benefit functions, estimating the capitalized tax benefit of debt to be 9.7% of a company's value. The study suggests that ordinary firms can quadruple tax benefits through debt issues until the marginal tax benefit starts diminishing. Additionally, it highlights how large, liquid, and profitable enterprises with lower expected distress costs exhibit conservative debt utilization.

Graham and Harvey (2001) argue that exogenous and unobservable shocks to the firm’s fundamental risk influence both leverage and corporate bond ratings. To address the potential endogeneity issue in Graham's (2000) research that leverage may lead to an overestimation of its effect on default risk, Molina (2005) examines the impact of leverage ratios on credit ratings. Neglecting the endogeneity of leverage, Molina (2005) finds that the effect of leverage on default risk is three times larger than it is. And a stronger effect means that leverage has a greater influence on the ex-ante costs of financial instability, which could outweigh current estimates of debt's tax advantages. Or in other words, such a larger estimate translates an increase in a firm's leverage into an increase in the firm's ex-ante costs of financial distress to avoid the underestimation of the impact of a leverage increase on ratings.

Almeida and Philippon (2007) explore the risk-adjusted probability of default derived from corporate bond spreads to calculate the present value of distress costs based on the assumption that the present value of distress costs is determined by risk premia, demonstrating that the marginal distress costs can be as significant as the tax advantages of debt calculated by Graham (2000). This helps explain why firms tend to be less leveraged than expected, given the substantial tax benefits associated with debt (Lemmon and Zender,2001; Minton and Wruck, 2001; Faulkender and Petersen, 2006).

Based on the discussion above, there is good reason to believe that companies have a target debt-equity ratio (Graham and Harvey, 2001; Brounen et al., 2006). Nevertheless, recognizing the significant role played by leverage ratios in measuring default risk required extensive detection and effort. Based on and extending the Black-Scholes model which presents a complete general equilibrium theory of option pricing only involving "observable" variables for direct empirical tests, Merton (1974) pioneers the introduction of dynamic capital structures to provide a significant theory capturing interest rates when there is a high likelihood of default, which greatly influenced subsequent research (Kealhofer and Kurbat, 2001; Crosbie and Bohn, 2003; Vassalou and Xing, 2004; Duffie et al., 2007). Merton's model treats a company's equity as a call option on the underlying value of its assets, with a strike price equal to the face value of the firm's debt. This means that default occurs when asset value falls below debt face value. The model includes the calculation of a distance-to-default (DD) metric, which is used to calculate the likelihood of a firm's assets being worth less than its debt face value.

Empirical studies confirm the mean-reverting nature of leverage ratios (Fama and French, 2002; Leary and Roberts, 2005; Flannery and Rangan, 2006; Lemmon et al., 2008; Harford et al., 2009; Huang and Ritter, 2009), consistent with the trade-off theory. Collin-Dufresne and Goldstein (2001) introduce a structural model of default with stochastic interest rates to capture this mean reversion, finding larger credit spreads for low-leverage businesses and reduced sensitivity to changes in company value.

Dangl and Zechner (2004) and Hui et al. (2007) extend Merton's (1974) classical structural analysis by incorporating dynamic capital structures. To enhance the benefits of dynamic capital structures, Bharath and Shumway (2008) propose the expected default frequency (EDF) measure or the naïve default probability measure, simplifying Merton's (1974) distance-to-default model. The same inputs as Merton’s distance-to-default model are utilized and the same functional form is kept in EDF, but the iterative solution procedure is forgone. Bharath and Shumway (2008) note that the predictive accuracy of the Merton model primarily arises from its functional structure rather than the true default probability. Campbell et al. (2008) employ discrete duration models or multi-period logit models, reaching a similar conclusion. They build upon the work of Shumway (2001) and Chava and Jarrow (2004). Duffie et al. (2007) utilize vector autoregression and a doubly-stochastic framework to model variable dynamics. And Löffler and Maurer (2011) forecast future leverage ratios adding leverage dynamics into the prediction of default and further improve statistical default prediction models by considering information about mean-reverting leverage ratios.

Traczynski (2017) employs Bayesian model to address model uncertainty and identifies the leverage ratio and stock market return volatility as robust default predictors, in both the total sample and individual industry groupings. As calculating market return volatility is challenging for unlisted firms, corporate financial leverage becomes a leading indicator (Merton, 1974; Collin -Dufresne and Goldstein, 2001; Vassalou and Xing, 2004; Bharath and Shumway, 2008). Cathcart et al. (2019) further explore the relationship between leverage ratios and default risk, finding that financial leverage has a greater influence on the probability of default for small and medium-sized enterprises (SMEs) compared to larger corporations. This discrepancy arises due to SMEs' higher exposure to short-term debt and the resulting increased refinancing risk.

4.2.2 Agency costs of debt and agency costs of equity

Shareholders and managers possess privileged access to internal company information, whereas creditors, who provide loans, possess significantly less internal information compared to stockholders. This means that conflicts arise between shareholders and creditors in leveraged companies, which leads to the agency costs of debt (Jensen and Meckling, 1976). Agency costs of debt occur when the interests of shareholders and creditors diverge, resulting in managers acting in favor of shareholders rather than debtholders. These costs arise when debtholders restrict the use of their capital due to concerns that management will prioritize shareholders over debtors (Kim and Sorensen, 1986), thereby increasing the difficulty of obtaining additional capital through debt, especially for financially distressed companies.

To illustrate the motivations of managers that disadvantage debtholders but benefit shareholders, several factors come into play. Firstly, when a company faces financial distress, particularly when on the verge of bankruptcy, shareholders often undertake substantial risks. Managers tend to take even greater risks to enhance expected returns for shareholders and safeguard their own job security. However, this increased risk-taking reduces expected returns for creditors and exposes them to heightened risk. Secondly, in cases where a company with a high likelihood of bankruptcy has an opportunity to significantly enhance its value by raising new equity capital to invest in new projects, thereby avoiding bankruptcy, managers tend to underinvest. This behavior stems from the fact that creditors would have to share a portion, if not the majority, of the income generated by the new investment project, thereby prioritizing shareholders over bondholders. Thirdly, a company experiencing financial distress, lacking prospects of recovery or losing confidence from shareholders, may attempt to maximize borrowing to transfer more benefits to shareholders before eventually filing for bankruptcy. Jensen and

Meckling (1976) demonstrate that the associated costs include the loss of opportunity wealth due to the influence of debt on corporate investment decisions, monitoring and bonding expenses incurred by bondholders and owner-managers, as well as bankruptcy and reorganization costs.

Myers (1977) further advances the understanding of agency costs of debt. This research is grounded in the concept that growth opportunities can be viewed as call options, with their value contingent upon discretionary future investments by the company. Consequently, the author argues that the issuance of relatively risky debt, which leads to suboptimal investment decision-making or even compels both the company and its creditors to bear the consequences of avoiding such suboptimal strategies, diminishes the present market value of a firm that holds real options. This study predicts a negative association between corporate borrowing and the share of market value attributed to real options.

Importantly, Nini (2012) provides evidence that creditors play a significant role in corporate governance to safeguard their own interests when a company defaults or experiences financial distress outside of bankruptcy. Creditors obtain substantial contractual rights upon covenant violations, allowing them to demand immediate or accelerated repayment of the loan amount. Debtholders typically choose to initiate credit agreement renegotiations, seeking waivers for the violations. Such amended credit agreements can lead to changes in the loan terms, such as reduced funding, shorter maturity, and higher interest rates, as well as increased contractual monitoring by lenders, such as collateral requirements and more stringent financial management and capital expenditure restrictions for violating companies. Additionally, it is worth noting that although accounting manipulation cannot prevent default, managers are likely to make income-increasing accounting decisions in hopes of improving their bargaining position during covenant renegotiations (Sweeney, 1994; DeFond and Jiambalvo, 1994; Beneish and Press, 1993, 1995a, b). Creditors also exert influence on business management behind the scenes by providing advice and urging management and the board to seek covenant waivers. Even though lender liability laws protect equity holders from direct interference, creditors can influence decision-making in the firm (Daniels and Triantis, 1995).

In some cases, covenant-violating firms may make concessions, such as replacing the CEO with a turnaround specialist, as seen in the example of Krispy Kreme Doughnut Corporation (Baird and Rasmussen, 2006). In line with the view that creditors utilize covenant violations to exert noncontractual control over business corporate governance, Nini et al. (2012) find that CEO turnover rates increase significantly after a company's debt default, particularly through forced resignations. Similar findings regarding CEO terminations after covenant violations are reported by Ozelge and Saunders (2009), who note a stronger link when loans are a significant source of funding. Balsam et al. (2018) find that debt default events result in an average decrease of 8.5% in CEO compensation. This decrease is particularly pronounced in risk-taking option grants and is amplified with the influence of creditors.

Regarding the agency costs of equity, there are differing views on the impact of debt on shareholder interests. On one hand, debt can have positive effects by disciplining managers and curbing excessive expenditures. Jensen (1986) argues that the disciplining effect of debt motivates managers in leveraged companies to work harder and avoid investing in projects with negative net present value when managers gain free cash flow more than what is required to support all projects. Debt issuance binds managers to future cash flow commitments, reducing excess cash flow available for discretionary spending. Substantial debt issuance for stock buybacks also provides incentives for managers to overcome resistance to downsizing, which is often necessary to allocate free cash flow efficiently. The risk of defaulting on debt payments acts as a powerful driver for businesses to improve their efficiency. Moreover, there are tax advantages associated with using debt or cash for stock repurchases.

On the other hand, the use of debt can have negative effects on shareholder benefits. Higher debt levels increase equity risk for managers and exacerbate "costly effort" and "playing it safe" agency conflicts of equity as well (Parrino et al., 2005), which hurt shareholders but meet debtholders' desire (Bertrand and Mullainathan, 2003; Klock et al., 2005; Chava et al., 2009; Qiu and Yu, 2009; Gormley and Matsa, 2016). According to the trade-off theory of capital structure proposed by Kraus and Litzenberger (1973), firms face a trade-off in choosing their capital structure, considering the costs of financial distress and the benefits of debt tax shields (Modigliani and Miller, 1963). Higher levels of debt can increase the costs associated with financial distress.

4.2.3 Takeover protection and shareholder outcomes

4.2.3.1 Monitoring role of takeover market

It is widely accepted that the takeover market plays a crucial role in improving market allocation. This is achieved through enhanced business management effectiveness, increased capital mobility, and better protection for non-controlling shareholders (Manne, 1965). Hostile takeovers can lead to the replacement of poorly performing managerial teams. Consequently, an active takeover market is considered an important external mechanism for disciplining managers (evidenced by Fama and Jensen, 1983; Jensen and Ruback, 1983; Lel and Miller, 2015; Scharfstein, 1988).

However, broadly defined anti-takeover protections aimed at preventing hostile takeovers, are often seen as influencing external shareholder governance. These protections grant managers additional voting power and shield them from market discipline (Easterbrook and Fischel, 1981; Kesner and Dalton, 1985). As a result, it becomes more challenging to remove management that engages in actions detrimental to shareholder interests, thus weakening shareholder control and leading to myopia by managers (Jensen and Meckling, 1976; Scharfstein, 1988; Stein, 1988; Norton, 1998). When the possibility of a takeover is eliminated, managers have greater freedom to act based on their own preferences, which may not align with the interests of shareholders.

4.2.3.2 Managerial Entrenchment Hypothesis

There are competing viewpoints regarding the impact of anti-takeover protection on shareholder benefits (Turk, 1992). The first perspective is the "Managerial Entrenchment

Hypothesis," which suggests that anti-takeover provisions are detrimental to the interests of shareholders (Manne, 1965; Williamson, 1975; Walkling and Long, 1984). As discussed earlier, the takeover market serves as an external mechanism for disciplining managers (Fama and Jensen, 1983; Jensen and Ruback, 1983; Scharfstein, 1988; Lel and Miller, 2015). This implies that anti-takeover protections, which shield managers from discipline and replacement (Easterbrook and Fischel, 1981; Kesner and Dalton, 1985), can have a negative impact on shareholder value by leading to managerial myopia derived from various agency conflicts (Jensen and Meckling, 1976; Scharfstein, 1988; Stein, 1988; Norton, 1998).

To support this perspective, Gompers et al. (2003) find that a higher Anti-Takeover Provisions (ATPs) index, composed of twenty-four ATPs, is associated with a lower corporate value. Bebchuk et al. (2009) further analyze this relationship and suggest that six specific ATPs in the index, including a classified board, a poison pill, and a supermajority amendment, are particularly effective. Cuñat et al. (2020) argue that removing takeover defenses at the firm level leads to a higher premium in takeover situations, indicating that managers place significant value on the protection provided by ATPs. Balachandran et al. (2021) also propose that increased anti-takeover protection raises default risk, which is often seen as stemming from managerial opportunistic behaviors.

4.2.3.3 Evidence from state-level anti-takeover laws supporting Managerial Entrenchment Hypothesis

Numerous studies have examined the implications of adopting anti-takeover laws on firm-level outcomes, shedding light on managers' preference for entrenchment. Shleifer and Summers (1988) demonstrate that when takeover threats are high, incumbent managers have less power relative to shareholders, implying that anti-takeover protection enhances managers' authority. Sroufe and Gelband (1990) assert that the Delaware business combination legislation, as exemplified by Justice Schwartz's decision, has significantly shifted the power balance between management and acquirers, granting management the ability to impede hostile takeovers through the board of directors, thereby weakening corporate governance. Supporting this perspective, Huang and Zhao (2009) find that CEO turnover and its sensitivity to performance increase following the decline of takeover threats resulting from the second generation of antitakeover legislation. Lel and Miller (2015) report that CEO turnover becomes more responsive to poor corporate performance after the adoption of legislation that reduces barriers to acquisitions, corroborating Huang and Zhao's findings (2009).

Furthermore, the passage of anti-takeover laws significantly impacts corporate performance. Initial releases of antitakeover legislation in a state indicate that affected companies experience unfavorable reactions in their stock prices (Karpoff and Malatesta, 1989). Giroud and Mueller (2010) discover that non-competitive industries witness a substantial decline in operational performance following the enactment of business combination laws, while competitive industries show no significant effect, suggesting that competition mitigates managerial slack. Huang and Peyer (2012) observe a decrease in firm value around the passage of business combination laws and argue for a substitute relationship between takeover defenses and a non-independent board during both positive and negative shocks. Chan et al. (2015) find that firms subject to business combination laws experience lower stock returns as their research and development (R&D) expenditures increase, implying that effective governance can prevent potential overinvestment in R&D spending, thereby enhancing the return on investment for firms engaged in such activities.

Bertrand and Mullainathan (1999) pioneer the use of anti-takeover law adoption as a proxy for weakened corporate governance quality. They argue that CEOs with weaker governance are more likely to extract personal benefits, leading to an increase in average managerial wages in affected companies. Furthermore, Bertrand and Mullainathan (2003) contend that the impact of business combination laws on corporate governance is more pronounced among various antitakeover legislations. Their study deepens my understanding of the effects of antitakeover laws by examining managerial preferences, suggesting that managers may pursue objectives that do not align with shareholder interests when they are not closely monitored. Previous literature suggests that pursuing private benefits can motivate managers to engage in value-destroying activities, such as "Empire Building" (Baumol, 1959; Marris, 1964; Williamson, 1964). Bertrand and Mullainathan (2003) leverage the unique aspects of corporate law and employ a difference-in-differences model to address potential biases related to the timing of legislation. Their findings indicate that active "Empire Building" may not be the norm, and managers may instead prefer to "enjoy the quiet life". The adoption of anti-takeover laws reduces managerial career concerns by making it more challenging to remove underperforming managers and potentially providing them with long-term contracts (Grossman and Hart, 1980; Knoeber, 1986; Scherer, 1988; Stein, 1988). In such circumstances, managers have an incentive to enjoy the quiet life and exert less effort, which contradicts shareholders' desires (Holmström, 1979; Grossman and Hart, 1983; Bertrand and Mullainathan, 2003).

Gormley and Matsa (2016) provide empirical evidence supporting the existence of risk-related agency conflicts by utilizing the passage of anti-takeover laws as an external shock to corporate governance. The authors argue that managerial risk preference is a major driver of management choices, company policies, and compensation structures that maximize shareholder value. Unlike costly efforts or private benefits, risk-related agency conflicts arise from the notion that risk-averse managers are incentivized to forgo risk-increasing but value-enhancing investments in favor of risk-reducing, suboptimal, or potentially value-destroying investments for reasons such as undiversified personal portfolios and career concerns (Jensen and Meckling, 1976; Amihud and Lev, 1981; Smith and Stulz, 1985; Holmstrom, 1999) , which reduces the frequency of adverse company results that are personally costly to the management (Sundheim, 2013). To support the argument, this study reveals that following the passage of business combination laws, managers engage in value-destroying activities, such as pursuing diversifying acquisitions targeting firms likely to reduce risk, have negative announcement returns, and be concentrated among firms with managers who gain the most from reducing risk, in order to lower their companies' stock volatility and risk of distress. This suggests that in the absence of external monitoring mechanisms associated with the threat of takeovers, managers tend to “play it safe”.

Garvey and Hanka (1999) further find that companies affected by the adoption of second-generation state-level anti-takeover laws reduce their use of debt since managers possess discretion over capital structure decisions which may not always align with maximizing shareholder wealth (Jung et al., 1996). This finding is supported by the arguments put forth by Zwiebel (1996) and Novaes and Zingales (1995) that managers employ debt due to its effects in reducing the threat of hostile takeovers, rather than for the benefit of shareholders. To make it more clear, Grossman and Hart (1982) and Stulz (1990) document that managers are likely to decrease debt issuance beyond what shareholders desire because debt constrains their actions. And Garvey and Hanka (1999) argue that the adoption of anti-takeover laws decreases the effectiveness of external monitoring mechanisms by raising the cost of takeovers, thus altering managerial incentives to conduct activities at their own discretion.

Qiu and Yu (2009) find that debt costs increase after the passage of business combination laws, with the increase being more significant for firms in non-competitive industries and firms with speculative-grade ratings. This leads to higher profitability and firm value, as well as a coinsurance effect where firms become less risky after being acquired. Chen et al. (2014) explore cost stickiness and find that selling, general, and administrative (SG&A) costs increase significantly more for firms subject to anti-takeover laws. This finding is consistent with the notion that managers “enjoy a quiet life” after being insulated from takeover threats.

The impact of antitakeover laws on corporate innovation has been a subject of significant consideration. In his study, Atanassov (2013) highlights the preventive influence of state anti-takeover laws on companies, resulting in reduced risks and limited advancements in value. Through empirical analysis, Atanassov (2013) observes a decline in the number of patents and citations among firms incorporated in states that implement antitakeover laws. This decline suggests that the adoption of such legislation promotes risk aversion and diminishes investments in business innovation, primarily due to heightened managerial entrenchment. Furthermore, Amore and Bennedsen (2016) demonstrate that firms governed by business combination laws exhibit a lower number of "green" patents, particularly those with small institutional ownership and greater financing constraints. These findings imply that deficient corporate governance may serve as a substantial barrier to achieving environmental efficiency.

The adoption of antitakeover laws also has implications for dividend policy. Francis et al. (2011) examine the relationship between corporate governance and dividend payout policy. They find that after the adoption of business combination laws, the dividend ratio decreases by 2%, and the likelihood of dividend payments decreases by 9%, aligning with the arguments presented by Easterbrook (1984) and Jensen (1986) that managers strongly prefer not to pay dividends since it reduces cash subject to managerial discretion. Jiang and Lie (2016) find that firms with less concern about threat of takeovers, as proxied by being covered by Business Combination Laws, delay their cash adjustment at high cash ratios. This indicates that self-interested executives are reluctant to disperse excess cash and prefer to maintain high cash levels until external pressure mounts. Fich et al. (2017) examine the effect of state-level antitakeover regulations on the value of internal slack, proxied by the value of internal cash, and find a positive relationship.

Moreover, Pasquariello (2017) investigates the relationship between agency-driven information asymmetry issues and stock liquidity using the staggered adoption of Business Combination Laws as a plausibly exogenous shock that unambiguously reduces the threat of and speculators' information advantage about value-enhancing interventions. The research concludes that following the adoption of business combination laws, corporate stock market illiquidity decreases as dealers perceive a reduced adverse selection risk associated with trading with better-informed speculators, particularly in periods of high fundamental or agency uncertainty and poor governance. Bhargava et al. (2017) find that firms covered by business combination laws exhibit an increased risk of future stock price crashes, as takeover protection helps reduce negative news hoarding.

In summary, antitakeover laws have significant effects on firm-level outcomes from various perspectives. These laws impact managerial power, corporate governance, managerial preferences, risk-related agency conflicts, capital structure decisions, dividend policy, corporate innovation, and stock liquidity.

4.2.3.4 Stockholder Interests Hypothesis

An alternative perspective in the field is the "Stockholder Interests Hypothesis" which suggests a positive relationship between anti-takeover protection and shareholder benefits

(Grossman and Hart, 1980; Knoeber, 1986; Scherer, 1988; Stein, 1988). Supporting this, Grossman and Hart (1980) find that increased veto power enables management to negotiate more favorable deals for shareholders. Besides, Additionally, Anti-takeover measures can effectively mitigate managerial "myopia". Stein's (1988) "myopia" hypothesis, which is based on informational asymmetry, explains why shareholders undervalue assets with long-term cash flows. Consequently, are anxious that setting a low company valuation may lead to unwelcome takeover efforts, leading to a focus on short-term investments over valuable long-term ones. Scherer (1988) further argues that takeover threats result in "short-sighted" decision-making by managers, negatively affecting economic efficiency. By providing managers with long-term job security and protection from replacement, anti-takeover protection boosts their confidence. Consequently, after implementing anti-takeover protections, managers may exhibit an inclination towards pursuing long-term investments, either as a defense against takeovers or to enhance current profitability (Pugh et al., 1992; Stein, 1988).

4.2.3.5 Related empirical evidence supporting Stockholder Interests Hypothesis

To substantiate the "Stockholder Interests Hypothesis", a series of studies have presented empirical evidence. Pugh et al. (1992) find that companies tend to increase their research and development as well as fixed capital expenditures following the amendment of corporate charters for anti-takeover protection. Zhao and Chen (2009) discover that higher levels of anti-takeover protection correspond to reduced abnormal accruals and increased earnings informativeness, indicating a decrease in earnings management and an enhancement in earnings quality.

Chemmanur et al. (2011) conclude that managers shielded from takeover threats and short-term price pressures exhibit superior performance by fostering high-quality initiatives. Additionally, the theoretical model developed by Chemmanur and Jiao (2012) formalize the concept that firm-level takeover defenses encourage risk-increasing and value-enhancing initiatives and promote stimulus efforts. Furthermore, Zeng (2014) investigates the impact of financial constraints on the relationship between anti-takeover protection and corporate innovation, concluding that financially constrained firms experience a significant increase in corporate innovation after adopting anti-takeover laws, thereby supporting the hypothesis that financial constraints mitigate the entrenchment effect by disciplining the allocation of corporate resources.

Callen et al. (2014) find that the adoption of business combination (BC) laws leads to an increase in conditional accounting conservatism, particularly in less competitive industries with higher return on assets (ROA) and lower institutional ownership. This finding suggests that accounting conservatism can serve as an internal governance mechanism to compensate for weakened external governance.

Raff and Verwijmeren (2015) propose that strong corporate governance in established enterprises fosters learning spillovers for potential entrants. Their research demonstrates that the adoption of anti-takeover laws, as a measure of corporate governance, facilitates informed entry choices, particularly in industries with less informative stock prices and higher exposure to industry-level uncertainty, which is in accordance with the learning hypothesis.

Zeng (2015) examines the influence of an active takeover market, measured by the adoption of business combination (BC) laws, on the level and value of company cash holdings. The study concludes that companies incorporated in states without BC laws possess significantly higher cash reserves to defend against unwanted takeovers.

Cen et al. (2016) argue that, aside from mitigating agency problems, the threat of takeovers affects firms' relationships with crucial stakeholders, such as major customers. Their study reveals that the adoption of BC laws improves firms' ability to attract new customers, strengthens relationships with existing customers, and enhances operating performance, as indicated by ROA. This highlights the positive aspects of protection from takeovers when stakeholder relationships play a significant role.

Bhattacharya et al. (2016) contend that corporate governance serves as a substitute for a firm's dividend policy under high idiosyncratic risk and as a complement under low idiosyncratic risk. By using the adoption of BC laws as a proxy for the level of corporate governance, the authors find that firms covered by BC laws exhibit an increased propensity to pay dividends under high idiosyncratic risk. Bharath and Hertzel (2016) measure the level of external governance pressure using BC laws and discover that firms covered by BC laws increase their utilization of bank financing, indicating a substitution effect in governance mechanisms. This implies that firms endogenously substitute governance mechanisms based on the relative strength of alternative external governance mechanisms.

Contrary to Atanassov (2013), Chemmanur and Tian (2018) provide causal evidence demonstrating that the implementation of firm-level takeover protection does not limit risks or opportunities for value enhancement accessible to the company. Specifically, their results indicate that the addition of corporate takeover protection encourages managers to take on more risk and incentives to boost investment in corporate innovation due to their increased attachment of shareholder interests to anti-takeover provisions. Cain et al. (2017) indicate that higher levels of takeover defenses at the state level result in a higher premium when a takeover occurs, which contrasts with the conclusions drawn by Cuñat et al. (2020) regarding the causal effects of takeover defenses at the state level.

4.2.4 Comparative assessment of state-level anti-takeover laws

The initiation of takeover regulations by the US government in 1968 through the enactment of the federal statute, the Williams Act, marked a significant step in regulating takeover activities. Subsequently, states extended the provisions of the Williams Act by passing their own statutes in the following year, collectively referred to as first-generation state-level anti-takeover laws. By 1982, companies in Massachusetts and 37 other states were already protected by first-generation anti-takeover legislation. The adoption of these laws resulted in a significant increase in takeover premiums and a notable decrease in hostile takeover bids (Jarrell and Bradley, 1980; Smiley, 1981). However, the constitutionality of the first-generation laws was successfully challenged, leading to their repeal by a decision of the US Supreme Court in the case of Edgar v. MITE Corp. Consequently, the level of takeover protection for companies previously shielded by the first-generation laws was reduced.

In response to the constitutional objections raised against the first-generation laws, individual states swiftly enacted second-generation anti-takeover laws. To date, 43 states have adopted 157 second-generation anti-takeover laws, including business combination, control share acquisition, fair price, poison pill, and directors' duties (constituency) laws, following the MITE decision and since the first control share acquisition law was adopted by Ohio in 1982.

The constitutionality and legality of these second-generation laws remained uncertain and subject to interpretation until a series of court decisions. Notably, the US Supreme Court affirmed the constitutionality of Indiana's control share acquisition laws in the case of CTS Corp. v. Dynamics Corp. of America, while the constitutionality of business combination laws was established by a ruling of the US Seventh Circuit Court of Appeals in Amanda Acquisition Corp. v. Universal Foods Corp. on May 24, 1989. Similarly, the legality of poison pills for Delaware firms was affirmed in the case of Moran v. Household International, Inc. on November 19, 1985.

Anti-takeover laws can only provide protection to target companies incorporated in states that have adopted such laws, making hostile takeovers more challenging while leaving friendly mergers unaffected. Importantly, cases where only the acquiring company is incorporated in the affected state are not influenced by these laws. It is widely believed that second-generation anti-takeover laws are potent (Karpoff and Wittry, 2018). Several studies have examined the impact of these laws on hostile takeovers individually. For example, Coates (2000), Daines (2001), Daines and Klausner (2001), and Bebchuk et al. (2002) argue that poison pills are particularly effective in deterring takeovers, while Cremers and Ferrell (2014) conclude that poison pills are the most effective among all takeover defenses measured by Gompers et al. (2003) G-index. Guercio et al. (2008) and Bebchuk and Ferrell (1999) acknowledge the effects of directors' duties laws.

Notably, previous literature has predominantly focused on the enactment of business combination (BC) laws following the research by Bertrand and Mullainathan (2003), who argue that only BC laws offer meaningful takeover protection. For instance, Gormley and Matsa (2016) examine managerial preferences for “playing it safe” for risk aversion, focusing on BC laws, which have been extensively studied and share the empirical setting of Bertrand and Mullainathan's (2003) research on managerial preference for enjoying a quiet life.

In contrast, Karpoff and Wittry (2018) challenge the notion that BC laws provide the most stringent protection and argue that it remains unclear which anti-takeover law offers the best defense against unsolicited takeovers. While BC laws do not impose restrictions on the acquiring company's ability to acquire shares, they require a waiting period of two to five years for certain types of transactions, such as mergers or large asset sales, which can increase the bidder's expenses (Subramanian et al., 2010). Control share acquisition regulations, on the other hand, are considered more stringent since their provisions have never been triggered. These laws strip bidders of their voting rights until a majority of other shareholders decide to restore them, while the voting rights of current management remain unaffected. Acquiring a majority of non-interested shares through supermajority support becomes necessary for a successful acquisition, thereby increasing the risk of failure and deterring many unsolicited bids from being initiated.

To measure the threat of hostile takeovers, Cain et al. (2017) developed a takeover index that incorporates fitted values from a model predicting the likelihood of hostile acquisitions. The index includes several elements such as court decisions, state anti-takeover laws (both first-generation and second-generation), macroeconomic conditions, and firm characteristics. However, it has been criticized for potential endogeneity issues as it includes endogenous firm characteristics and may not be applicable to acquisitions classified as non-hostile (Karpoff and Wittry, 2018). Balachandran et al. (2022) employ this index as a proxy for takeovers to investigate its effect on default risk. Despite potential endogeneity concerns, the index represents a combined effect of various anti-takeover protections mentioned earlier, without identifying the most powerful one.

4.3. Hypothesis development

Competing theories have been proposed concerning the implications of anti-takeover provisions, with the "Managerial Entrenchment Hypothesis" suggesting a negative impact on stockholders' interests (Manne, 1965; Walkling and Long, 1984; Williamson, 1975), while the "Shareholder Interests Hypothesis" posits that heightened anti-takeover protection can facilitate managerial activities that primarily benefit shareholders (Grossman and Hart, 1980; Knoeber, 1986; Scherer, 1988).

The "Managerial Entrenchment Hypothesis" recognizes an active takeover market as an external mechanism that effectively disciplines managers (Fama and Jensen, 1983; Jensen and Ruback, 1983; Scharfstein, 1988; Lel and Miller, 2015). In cases of poor managerial performance, companies become more likely to be taken over, leading to managerial replacements (Manne, 1965). However, anti-takeover protections are believed to weaken this disciplinary mechanism by shielding managers from replacement and offering long-term contracts that mitigate career concerns. Moreover, this process grants managers additional voting power (Easterbrook and Fischel, 1981; Kesner and Dalton, 1985), thereby increasing managerial entrenchment and giving rise to agency costs of equity.

Driven by self-interest, managers may engage in value-destroying activities such as "empire building" (Jensen and Meckling, 1976; Scharfstein, 1988; Stein, 1988; Norton, 1998). Building upon this perspective, Balachandran et al. (2022) argue that default risk is often attributed to opportunistic managerial behavior and establish a link between reduced takeover likelihood and heightened default risk. Specifically, they propose that opportunistic managerial conduct, facilitated by weakened disciplinary mechanisms, exacerbates agency conflicts with external stakeholders, resulting in diminished cash flows available for debt payments and ultimately increasing default risk (Driss et al., 2021).

Alternatively, weakened external disciplinary mechanisms for managers, which contribute to an increase in agency costs of equity, can also lead to reduced default risk. Supporting this viewpoint, Garvey and Hanka (1999) find evidence suggesting that companies influenced by the implementation of second-generation state-level anti-takeover laws exhibit a decrease in their reliance on debt. This trend can be attributed to a reduced likelihood of hostile takeover threats, which typically motivate managers to increase their utilization of debt (Zwiebel, 1996; Novaes and Zingales, 1995). The diminished likelihood of managerial termination resulting from anti-takeover laws alleviates managerial career concerns (Grossman and Hart, 1980; Knoeber, 1986; Scherer, 1988; Stein, 1988), enabling managers to exercise discretion in capital structure decisions that may not maximize shareholder wealth (Jung et al., 1996). As a result, managers are likely to decrease debt issuance more than desired by shareholders, as debt constrains their actions (Grossman and Hart, 1982; Stulz, 1990).

Furthermore, the financial leverage ratio can serve as a leading indicator for predicting default risk (Traczynski, 2017; Cathcart et al., 2019). As previously mentioned, default occurs when a company's asset value falls below its debt face value (Merton, 1974), supporting the notion that default risk can be assessed based on this ratio. Additionally, the trade-off theory of capital structure, as proposed by Kraus and Litzenberger (1973), demonstrates that increased leverage amplifies the ex-ante costs associated with financial distress. In summary, strengthened anti-takeover protection has the potential to reduce default risk, which benefits debtholders.

While the "Shareholder Interests Hypothesis" posits that anti-takeover protection may lead to managerial activities that benefit shareholders. It is important to note that in addition to agency costs of equity, anti-takeover protections can also give rise to agency costs of debt. These costs encompass conflicts between shareholders and creditors, which arise in situations where shareholders and managers possess access to internal company information, while creditors have limited information. Managers often prioritize the interests of shareholders over those of debtholders when their respective interests diverge, resulting in agency costs of debt as debtholders curtail their capital allocation (Kim and Sorensen, 1986). This dynamic can create obstacles in securing additional debt capital, particularly for financially distressed firms, ultimately leading to increased default risk.

For instance, managers may choose to undertake long-term investments instead of focusing on defending against takeovers or managing short-term profitability (Pugh et al., 1992), commonly referred to as overinvestment. However, it is important to note that default risk is not solely influenced by variations in the level of debt. Even when debt remains constant, the riskiness of total cash flows is also presumed to affect default risk. Consequently, such risk-increasing and value-destroying investments in risky long-term projects can elevate default risk since there is no guarantee of generating long-term value from these endeavors, despite benefiting shareholders.

H1: Following the passage of anti-takeover laws, the level of default risk can both be increased or decreased for companies incorporated in affected states.

As highlighted earlier, anti-takeover protection can exert diverse effects on managers, leading to varying implications for default risk. The aforementioned predictions are premised on the idea that anti-takeover measures diminish the external disciplinary mechanism inherent in the takeover market, thus shielding managers from replacement and giving rise to agency conflicts. To investigate the incentives driving managerial behavior and elucidate the factors influencing default risk, I delve further into the impact of anti-takeover protection on the agency costs of equity.

There exists a contentious debate surrounding the influence of anti-takeover provisions on managerial activities (Turk, 1992). The first perspective, known as the "Stockholder Interests Hypothesis" (Grossman and Hart, 1980; Knoeber, 1986; Scherer, 1988; Stein, 1988), posits that managerial activities following enhanced anti-takeover protection are beneficial for shareholders. Supporting this viewpoint, Grossman and Hart (1980) contend that increased veto power enables management to negotiate more favorable deals for stockholders.

Contrarily, the second perspective, termed the "Managerial Entrenchment Hypothesis," argues that anti-takeover provisions are detrimental to stockholders' interests (Manne, 1965; Walkling and Long, 1984; Williamson, 1975). As previously elucidated, takeovers are commonly viewed as enhancing market allocation, often involving the replacement of managerial teams (Manne, 1965). Thus, an active takeover market is deemed a crucial external mechanism for disciplining managers (Fama and Jensen, 1983; Jensen and

Ruback, 1983; Lel and Miller, 2015; Scharfstein, 1988). By impeding this disciplining mechanism, anti-takeover protection insulates managers from market-induced discipline and grants them additional voting power (Easterbrook and Fischel, 1981; Kesner and Dalton, 1985), thereby giving rise to various agency conflicts with shareholders (Jensen and Meckling, 1976; Scharfstein, 1988; Norton, 1998).

H2: Following the passage of the anti-takeover laws, agency costs of equity can both be increased or decreased for companies incorporated in affected states.

Finally, to directly illustrate that agency cost of equity can be the possible channel, I examine the ultimate effects of changes in default risk resulting from increased anti-takeover protection on shareholder benefits. The utilization of debt, as a leading indicator of default risk (Traczynski, 2017; Cathcart et al., 2019), elicits differing viewpoints regarding its impact on shareholder benefits. Firstly, the use of debt has been associated with positive effects. Supporting this notion, Jensen (1986) observes that debt utilization can incentivize managers to exert greater effort, reduce agency costs, and mitigate excessive expenditures. Additionally, debt employment can curtail the likelihood of managers accessing excess free cash flow and investing it in projects with negative net present value, instead of distributing it to shareholders, thereby safeguarding shareholders' interests (Jensen, 1988).

Conversely, the use of debt can yield adverse effects. To illustrate this point, the utilization of debt gives rise to agency costs of debt, which pertain to conflicts between shareholders and creditors. These agency costs emerge when debtholders restrict the deployment of their capital due to concerns that management may prioritize shareholders' interests over fulfilling the obligations to creditors (Kim and Sorensen, 1986). In particular, Nini (2012) documents that, in situations of default or even outside bankruptcy proceedings, creditors play a significant role in corporate governance to safeguard their own interests. In fact, creditors possess substantial contractual rights that can be triggered if debt covenants are violated, enabling them to demand immediate or accelerated repayment of the entire loan amount. While debtholders generally prefer to initiate renegotiations of credit agreements, such actions entail a series of costs for shareholders. Furthermore, a higher reliance on debt, by augmenting the manager's equity risk and subsequently increasing their career concerns, can exacerbate agency conflicts related to "costly effort" and "playing it safe" behaviors (Parrino et al., 2005), ultimately undermining shareholder benefits.

H3: Following the passage of the anti-takeover laws, the change of default risk following increased anti-takeover protection can both positively or negatively influence shareholder outcomes for companies incorporated in affected states.

4.4. Methodology

To examine the empirical impact of staggered adoption of anti-takeover laws across different states in the United States on default risk, I employ the difference-in-differences estimation approach following Bertrand and Mullainathan (2003). This estimation strategy takes into account the existence of staggered treatments, which compares the before-after effect of takeover legislation on affected states (the treatment group) with the before-after effect in states in which such a change was not affected (the control group) since multiple exogenous shocks affect different states and firms at different points. By employing this methodology, I effectively eliminate the possibility of reverse causality between the adoption of anti-takeover protection and the level of default risk. In contrast, settings with a single shock face a common identification challenge, as potential noise coincides with the shock that directly influences the dependent variable (Roberts and Whited, 2013). my identification strategy aligns with the approaches used in a series of recent studies, including Gormley and Matsa (2016), Klasa et al. (2018), Ali et al. (2019), among others.

In the firm-level data, I estimate:

$$\begin{aligned} &\boldsymbol{Default_{risk_{i,k,l,t}}} = \\ &\quad \boldsymbol{\alpha_t + \beta CS_{k,t-1} + \gamma_1 Ln(Equity)_{i,k,l,t-1} + \gamma_2 Ln(Debt)_{i,k,l,t-1} + \gamma_3\, {}^{1}\!/_{\sigma_{Ei,k,l,t-1}}} + \\ &\quad \boldsymbol{\gamma_4 ExcessReturn_{i,k,l,t-1} + \gamma_5\, {}^{Income}\!/_{Assets_{i,k,l,t-1}} + CS * lobbying_{firms}} + \\ &\quad \boldsymbol{\theta' Firm + \varphi' IndustrybyYear + \rho' StatebyYear + \varepsilon_{i,k,l,t}} \qquad \textbf{(5)} \end{aligned}$$

where $\boldsymbol{i}$ indexes firms; $\boldsymbol{t}$ indexes years; $\boldsymbol{k}$ indexes state of incorporation; $\boldsymbol{l}$ indexes state of location; $\boldsymbol{FirmFE}$, $\boldsymbol{IndustrybyYearFE}$ and $\boldsymbol{StatebyYearFE}$ are firm, four-digit-SIC industry-by-year and state of location -by-year fixed effects respectively. $\boldsymbol{Default_{risk_{i,k,l,t}}}$is the dependent variable of interest. $\boldsymbol{CS_{k,t-1}}$ is a dummy variable that equals one if an antitakeover law has been passed by time $\boldsymbol{t}$ in state $\boldsymbol{k}$. To control for the direct determinants of default risk, I follow Bharath and Shumway (2008) to include five control variables: $\boldsymbol{Ln(Equity)_{i,k,l,t-1}}$, $\boldsymbol{Ln(Debt)_{i,k,l,t-1}}$, $\boldsymbol{1/\sigma_{Ei,k,l,t-1}}$, $\boldsymbol{ExcessReturn_{i,k,l,t-1}}$, $\boldsymbol{Income/Assets_{i,k,l,t-1}}$. I lag all independent variables by one year to further mitigate the issue of reverse causality. Standard errors are clustered by state of incorporation. $\boldsymbol{Error_{i,k,l,t}}$ is an error term. Specifically, I assign a firm's location based on the location of its headquarters, which is typically also where major plants and operations are located (Henderson and Ono, 2008).

The inclusion of firm fixed effects accounts for unobservable, time-invariant differences among firms, while state-by-year fixed effects control for unobservable, time-varying differences across states. Additionally, industry-by-year fixed effects control for unobservable, time-varying differences across industries. The validity of the state-by-year fixed effect is supported by the fact that over half of my firm samples are incorporated in a different state than their physical location.

The coefficient on the adoption of control share acquisition (CS) laws measures the impact of changes in state litigation regarding CS laws on a firm's default risk compared to rival companies in unaffected states. To mitigate concerns regarding reverse causality, I introduce a one-year lag in the indicator variable for the adoption of control share (CS) laws.

Furthermore, it is important to note that the adoption of anti-takeover laws primarily aims to make hostile takeovers of target firms incorporated in affected states more challenging. Specifically, a control share acquisition law restricts a bidder's voting rights unless a majority of other shareholders vote to restore such rights. This means that the laws do not directly influence corporate default risk. Thus, the effect of anti-takeover laws on default risk is likely an unintended consequence. The adoption of anti-takeover laws serves as a source of variation in external shareholder governance.

Factors such as the political economy or the business cycle are unlikely to undermine my analysis. Studies like Romano (1987) and Bertrand and Mullainathan (2003) indicate that the passage of anti-takeover laws typically does not result from substantial pressure exerted by a coalition of economic players, which suggests that an unobserved economic variable is unlikely to explain the observed effects. Nonetheless, I control for relevant factors by incorporating location-state-by-year and industry-by-year fixed effects in my analysis. By employing high-dimensional fixed effects, I reduce the likelihood of discovering a significant coefficient, thus increasing confidence that my finding of a significant coefficient is not due to unobserved sources of heterogeneous variation related to the firm's industry, location, or year of observation.

Moreover, the passage of state laws, which may be susceptible to lobbying and other political pressures, can potentially lead to reverse causation. To support the view, Werner and Coleman (2015) argue that antitakeover laws are strongly affected by corporate lobbying. Consistent with this argument, Karpoff and Malatesta (1989) identify 19 anti-takeover laws that were passed based on the specific requests of individual companies.

Using more recent data, Gartman (2000) reports that at least 46 firms played a role in the promotion of 49 state anti-takeover laws in 23 states. To address the possibility of lobbying firms influencing my findings, I follow Karpoff and Wittry (2018) by including ***CS*lobbying*** as a control variable.

Finally, a fundamental assumption for a causal interpretation of the difference-in-differences estimation is that treated and control firms exhibit parallel trends prior to the state policy change, which will be illustrated in the subsequent section. In summary, the aforementioned discussion supports the likely validity of my research design.

4.5. Data sources and variable construction

4.5.1 Data sources

My sample comprises US companies in both the Compustat Industrial files and the Center for Research in Security Prices (CRSP) stock file, covering the period from 1975 to 2007. To gather accounting data, I utilized the CCM (merged CRSP/Compustat) database. The adoption dates of anti-takeover laws were obtained from Karpoff and Wittry's (2018) research. Following Gormley and Matsa (2016), Klasa, Ortiz-Molina, Serfling and Srinivasan (2018), Ali et al. (2019) and others, my sample period encompasses a span of more than five years before and after the adoption of the laws in each state. This duration effectively covers almost all second-generation anti-takeover laws' adoption dates and allows sufficient time to observe the persistent effects of CS laws on changes in corporate default risk levels. Specifically, my sample starts more than five years prior to Ohio's adoption of the CS laws in 1982 and ends more than five years beyond Mississippi's adoption in 1991. I excluded utility companies (SIC codes 4900–4999) and financial companies (SIC codes 6000–6999), firms located or incorporated outside the US, as well as firm-year observations with non-positive assets or sales. Additionally, all continuous variables were subjected to winsorization at the 1% level.

4.5.2 Independent variable

Anti-takeover laws can only protect target companies incorporated in states that have adopted the laws, making hostile takeovers more difficult. Therefore, I construct my independent variable as a dummy indicator that equals one if an anti-takeover law has been passed by the year in the state of incorporation of the company. Specifically, for the states that have adopted anti-takeover laws, the indicator variable equals 0 for the years before adoption and 1 for the subsequent years. For other states where the law is not considered, the indicator variable equals 0 every year (Gormley and Matsa, 2016; Klasa et al., 2018; Ali et al., 2019).

My analysis focuses on control share acquisition laws to proxy takeover threats among second-generation anti-takeover laws and other measures. To illustrate the reasons why I do not use other proxies, as mentioned above, the hostile takeover index by Cain et al. (2017), which covers all second-generation anti-takeover laws, is criticized for involving endogeneity issues. This is because it includes endogenous firm characteristics and may not generalize to acquisitions not classified as hostile (Karpoff and Wittry, 2018). Besides, the index represents a mixture effect of all anti-takeover protections, ignoring which one provides the most powerful protection.

It is generally believed that second-generation anti-takeover laws are powerful (Karpoff and Wittry, 2018). However, the separate effect of second-generation anti-takeover laws is still debatable. Even though a series of previous literature focuses on the passage of BC laws following Bertrand and Mullainathan's (2003) argument that only BC laws offer meaningful takeover protection (Karpoff and Wittry, 2018), the authors challenge whether the BC laws are the most stringent laws and argue that it is unclear which anti-takeover law provides the best protection against unsolicited takeovers. The business combination law, on the other hand, places no restrictions on the bidder's ability to acquire shares, while it requires a two- to five-year waiting period on certain types of bidder-firm transactions, such as a merger or large asset sale, which may raise the bidder's expenses (Subramanian et al., 2010). However, on a strictly theoretical level, business combination regulations do not appear to give stronger takeover protection.

Karpoff and Wittry (2018) also illustrate how stringent control share acquisition regulations are since their provisions have never been triggered. A control share purchase law takes away a bidder's voting rights until a majority of other shareholders decide to restore them. On the other hand, the voting rights of current management remain unaffected. To complete an acquisition, a bidder must obtain supermajority support from shares that are not interested, which increases the risk of failure and likely deters many unsolicited bids in the first place.

4.5.3 Dependent variable

Merton (1974) posits a general equilibrium theory that conceptualizes corporate equity as a call option on the underlying value of a firm's assets, with the debt face value serving as the strike price. This means that default occurs when the value of a company's assets falls below its debt face value. Expanding on this notion, Merton (1974) further introduces the structural distance-to-default (DD) model. The Merton model has been widely employed in both academic and practical contexts (Kealhofer and Kurbat, 2001; Crosbie and Bohn, 2019; Vassalou and Xing, 2004; Duffie et al., 2007). However, Bharath and Shumway (2008), along with supporting evidence from Campbell et al. (2008), argue that the predictive power of the Merton model primarily derives from its functional structure rather than the actual default probability it generates.

Building upon Merton's (1974) structural distance-to-default (DD) model, Bharath and Shumway (2008) propose an alternative approach to measure default probability that maintains the structural framework and fundamental inputs of the Merton model while simplifying the calculation process. This alternative method incorporates distance-to-default (DD) into a cumulative standard normal distribution, enabling estimation of the probability that a firm's asset value will dip below its debt face value, which is referred to as the expected default frequency (EDF). Following the methodology presented by Bharath and Shumway (2008), I adopt their measure of expected default frequency (EDF).

Thus, I follow Bharath and Shumway (2008) to compute *EDF* as follows:

$$DD_{i,t} = \frac{\log\left(\frac{Equity_{i,t}+Debt_{i,t}}{Debt_{i,t}}\right) + \left(r_{i,t-1} - \frac{\sigma^2_{Vi,t}}{2}\right) \times T_{i,t}}{\sigma_{Vi,t} \times \sqrt{T_{i,t}}}, \qquad (1)$$

$$\sigma_{Vi,t} = \frac{Equity_{i,t}}{Equity_{i,t} + Debt_{i,t}} \times \sigma_{Ei,t} + \frac{Debt_{i,t}}{Equity_{i,t} + Debt_{i,t}} \times (0.05 + 0.25 \times \sigma_{Ei,t}) \qquad (2)$$

and

$$EDF_{i,t} = N(-DD_{i,t}), \qquad (3)$$

where $Equity_{i,t}$ is the market value of equity (in millions of dollars) calculated as the product of the number of shares outstanding and stock price at the end of the year; $Debt_{i,t}$ is the face value of debt computed as the sum of debt in current liabilities and one-half of long-term debt at the end of the year; r_{it-1}, firm $_i$'s past annual return, is calculated from monthly stock returns over the previous year; $\sigma_{Ei,t}$ is the stock return volatility for firm *i* during year *t* estimated using the monthly stock return from the previous year; $\sigma_{Vi,t}$, calculated from $\sigma_{Ei,t}$, is an approximation of the volatility of firm assets; and $T_{i,t}$ is set to one year. *I* construct $DD_{i,t}$ of all sample firms as of the last day of each year. *N(.)* is the cumulative standard normal distribution function.

4.5.4 Control variables

I follow Bharath and Shumway (2008) and Brogaard et al. (2017) to introduce the following control variables. $\text{Ln}(Equity)$ is the natural log of the market value of equity at the end of the year. $\text{Ln}(Debt)$ is the natural log of the ace value of debt. $1/\sigma_E$ is the inverse of the annualized stock return volatility. $Excess\ Return$ is the difference between the stock's annual return and the CRSP value-weighted return. More specifically, I follow

Bharath and Shumway (2008) to calculate the excess return using market return as the benchmark given my focus on the part of the return that is not explained by the overall market return. Income /Assets is the ratio of net income to total assets. As reported in Bharath and Shumway (2008) and Brogaard et al. (2017), $Ln(Equity)$is significantly and negatively related to EDF at the 1% significance level, while $Ln(Debt)$ is significantly and positively related to EDF at the 1% significance level. $1/_{\sigma_E}$, $Excess\ Return$ and $^{Income}/_{Asset}$ are significantly and negatively related to EDF at the 1% level.

4.5.5 Summary statistics

Finally, based on the illustration above, the summary statistics are shown below. All the key variables listed below are comparable to previous literature.

Table 4.1. Summary statistics

The sample consists of firm-year observations during the 1975 to 2007 period, obtained from the CRSP-Compustat merged database. my sample covers public firms listed on CRSP/ Compustat merged database excluding utilities and financials (SIC codes 4900–4999 and SIC codes 6000–6999) due to different regulatory oversight from others. I also filter on firms incorporated in the U.S. The observations with missing variables and minus assets or sales data are excluded. Variable definitions are provided in Appendix B. All continuous variables are winsorized at the 1st and 99th percentiles.

Variables	n	Mean	S.D.	Min	0.250	Mdn	0.750	Max
CS law	106089	0.190	0.390	0	0	0	0	1
PP law	106089	0.210	0.410	0	0	0	0	1
DD law	106089	0.210	0.400	0	0	0	0	1
FP law	106089	0.200	0.400	0	0	0	0	1
BC law	106089	0.630	0.480	0	0	1	1	1
EDF	106089	0.060	0.210	0	0	0	0	1
Ln (Equity)	106089	4.590	2.130	0.220	3.010	4.450	6.080	9.940
Ln (Debt)	106089	3.650	2.250	-1.320	2.050	3.520	5.180	9.130
$1/\sigma_E$	106089	9.290	5.580	1.730	5.370	7.910	11.72	30.65
Income/Assets	106089	-0.020	0.220	-1.240	-0.020	0.040	0.080	0.250
Excess Return	106089	0.050	0.690	-0.970	-0.350	-0.060	0.250	3.410

4.6. The effects of anti-takeover protection on default risk

4.6.1 Passage of Control Share Acquisition Laws and expected default frequency (EDF)

Based on the aforementioned discussion, I proceed with my baseline regression analysis to examine the impact of the adoption of control share acquisition (CS) laws on default risk, as measured by the expected default frequency (EDF) following Bharath and Shumway (2008). The results are presented in Column (1) and Column (2) of ***Table 4.2***, corresponding to the regression without control variables and with control variables, respectively.

Specifically, the estimated coefficient of CS laws in the regression without control variables is -0.01449, which is statistically significant at the 1% level. Similarly, in the regression with control variables, the estimated coefficient of CS laws is -0.01093, also significant at the 1% level. In addition to assessing statistical significance, I further explore the economic significance of the findings. On average, firms incorporated in states that have implemented CS laws experience an 18.2% reduction in default risk relative to the mean default risk during the sampled period.

The findings demonstrate that worse corporate governance following the adoption of CS laws leads to a decrease in default risk among affected companies. This suggests that the conventional agency conflicts "Empire Building" may not be the dominant reason for managers to engage in activities that deviate from shareholders' preferences. as a consequence of career concerns and risk aversion, managers tend to reduce their reliance on debt when corporate governance weakens, given their discretion in making capital structure decisions (Jung et al., 1996).

Regarding the control variables, my findings align with those of Bharath and Shumway (2008) and Brogaard et al. (2017). Specifically, I observe a significantly negative relationship between $Ln(Equity)$ and EDF at the 1% significance level, while $Ln(Debt)$ exhibits a significantly positive relationship with EDF at the 1% significance level. Additionally, variables such as $1/\sigma_E$, $Excess\ Return$, and *Income/Assets* show a statistically significant negative association with EDF at the 1% level. Furthermore, the introduction of these additional control variables leads to an approximate 17% increase in the R^2, indicating an improved explanatory power of the regression model. The coefficients associated with the control variables demonstrate that firms with higher face value of debt, lower market capitalization, lower annualized stock return volatility, lower annual excess return, and a lower ratio of net income to total assets exhibit higher levels of default risk.

Table 4.2. Effect of second-generation anti-takeover laws on default risk

This table reports coefficients from firm-panel regressions of a firm's default risk proxied by EDF by Bharath and Shumway (2008) on an indicator for whether the firm's state of incorporation has adopted anti-takeover laws, firm fixed effects (FE), state-of-location-by-year FE, and standard industrial classification industry-by-year FE. The dependent variables are default risk. Specifically, in Column (1) I only involve EDF (my main dependent variable) and adoption of control share acquisition laws (my main independent variable) in the regression without control variables, as my baseline analysis. And in Column (2) I further add control variables including $Ln(Equity)_{i,k,l,t-1}$, $Ln(Debt)_{i,k,l,t-1}$, $1/\sigma_{Ei,k,l,t-1}$, $ExcessReturn_{i,k,l,t-1}$, $Income/Assets_{i,k,l,t-1}$, which are explained above in the regression. Then in Column (3) (4) (5) and (6), I replace the dependent variable with the adoption of poison pill laws, directors' duties laws, fair price laws, and business combination laws. In Column (7), I include all the second-generation anti-takeover laws in regression. I lag all independent variables by one year to mitigate the issue of reverse causality. The sample includes firm-year observations from 1975 to 2007. I winsorize continuous variables at the 1st and 99th percentiles. Variable definitions are provided in Appendix B. Standard errors are adjusted for clustering at the state of incorporation level. T values are reported in parentheses. *, **, and ***, indicate statistical significance at the 10, 5, and 1 per cent level, respectively.

	Dependent Variables						
Independent Variables	（1） EDF	（2） EDF	（3） EDF	（4） EDF	（5） EDF	（6） EDF	（7） EDF
CS law	-0.014***	-0.010***					-0.010**
	(-3.678)	(-2.888)					(-2.321)
PP law			-0.004				-0.002
			(-1.333)				(-0.524)
DD law				-0.002			0.005
				(-0.798)			-1.128
FP law					-0.007		-0.005
					(-1.660)		(-1.129)
BC law						0.004	0.003
						(-0.973)	(-1.040)
Ln (Equity)		-0.049***	-0.049***	-0.049***	-0.049***	-0.049***	-0.049***
		(-57.343)	(-57.297)	(-57.255)	(-57.347)	(-57.069)	(-57.199)
Ln (Debt)		0.046***	0.046***	0.046***	0.046***	0.046***	0.046***
		(-35.51)	(-35.460)	(-35.378)	(-35.418)	(-35.464)	(-35.508)
$1/\sigma_E$		-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***
		(-16.152)	(-16.183)	(-16.190)	(-16.129)	(-16.196)	(-16.110)
Income /Assets		-0.064***	-0.063***	-0.063***	-0.064***	-0.063***	-0.064***
		(-13.854)	(-13.914)	(-13.927)	(-13.935)	(-13.872)	(-13.916)
Excess Return		-0.064***	-0.064***	-0.064***	-0.064***	-0.064***	-0.064***
		(-27.623)	(-27.636)	(-27.626)	(-27.630)	(-27.632)	(-27.601)
Constant	0.062***	0.146***	0.145***	0.145***	0.146***	0.142***	0.145***
	(-84.698)	(-33.291)	(-32.680)	(-31.166)	(-30.816)	(-33.000)	(-34.033)
Observations	103,922	103,922	103,922	103,922	103,922	103,922	103,922
R-squared	0.412	0.484	0.484	0.484	0.484	0.484	0.484
Company FE	YES	YES	YES	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES	YES	YES	YES
State-Year FE	YES	YES	YES	YES	YES	YES	YES

4.6.2 Passage of other second-generation anti-takeover laws and expected default frequency (EDF)

I aim to examine the relationship between takeover threats and default risk. Since the MITE decision in 1982, when Ohio introduced the first CS law, a total of 43 states have implemented at least 157 second-generation antitakeover laws. Previous studies have independently demonstrated the effectiveness of these laws in constraining hostile takeovers. Notably, Coates (2000), Daines (2001), Daines and Klausner (2001), and Bebchuk et al. (2002) argue for the efficacy of poison pills as takeover deterrents, while Cremers and Ferrell (2014) conclude that poison pills are the most effective among various takeover defenses. Guercio et al. (2008) and Bebchuk and Ferrell (1999) acknowledge the impact of directors' duties laws, and Bertrand and Mullainathan (2003) emphasize the significance of the most stringent second- and third-generation laws known as business combination laws.

In this section, I replicate my baseline regression analysis by substituting CS laws with business combination, fair price, poison pill, and directors' duties (constituency) laws. Further, these laws are collectively included in the same regression to ensure a comprehensive examination of each second-generation antitakeover law. The results presented in Column (3), (4), (5), (6), and (7) of ***Table 4.2*** yield several key findings. Firstly, all coefficients associated with antitakeover laws exhibit negative signs, except for business combination (BC) laws. When these second-generation laws are considered in one regression, the coefficient for control share acquisition laws remains negative and significant at the 5% significance level, while the coefficients for other laws are deemed statistically insignificant. In terms of the economic significance, on average, firms incorporated in states that have implemented CS laws experience an 17% reduction in default risk relative to the mean default risk during the sampled period, controlling all the other second-generation anti-takeover laws.

These results further substantiate my hypothesis that the adoption of CS laws leads to reduced external monitoring mechanisms and corporate governance in affected companies. Consequently, managers exercise discretion in reducing debt usage, thereby decreasing default risk.

Notably, the impact of CS laws appears to be more pronounced compared to other second-generation antitakeover laws, as evident from the results presented in ***Table 4.2***. This disparity arises due to the requirement under CS laws that bidders secure supermajority support from disinterested shares to complete an acquisition. This condition increases the risk of failure and deters many unsolicited bids in the first place. However, these laws place the right to refuse such transactions in the hands of directors who are influenced greatly by the incumbent management before the acquirer becomes an interested shareholder. Thus, managers gain more power and are freer to act upon their underlying preferences that do not align with shareholders' interests (Sroufe and Gelband, 1990). This power asymmetry is directly linked to the level of corporate default risk. In contrast, other laws do not impose such requirements. For instance, poison pill laws enable firms to adopt poison pill takeover defenses, directors' duties laws mandate corporate directors to consider the interests of all stakeholders along with shareholders' interests, and business combination laws introduce a two-to-five-year delay on specific types of transactions between the bidder and the firm.

4.6.3 Passage of other anti-takeover laws and expected default frequency (EDF)

Furthermore, in order to address concerns regarding potential omitted laws that may impact my findings, I thoroughly examine all additional takeover laws encompassed within the Cain et al. (2017) hostile takeover index. This includes the assessment of first-generation takeover laws and their subsequent repeal, control share cash-out statutes, disgorgement provisions, anti-greenmail laws, golden parachute restrictions, tin and silver parachute blessings, as well as assumption of lab. I analyse the impact of these laws on the measure of default risk (EDF) both individually and by incorporating them as control variables in the regression model, while keeping all other settings unchanged.

I present the empirical results in ***Table 4.3.*** Specifically, when controlling all additional takeover laws encompassed within the Cain et al. (2017) hostile takeover index, the coefficient for control share acquisition laws remains negative and significant at the 5% significance level, while the coefficients for other laws are statistically insignificant. In terms of the economic significance, on average, firms incorporated in states that have implemented CS laws experience an 19.3% reduction in default risk relative to the mean default risk during the sampled period. The results discussed above are consistent with my baseline regression results. And controlling the adoption of these additional laws does not alter the outcomes of my study.

Table 4.3. Effect of other anti-takeover laws on corporate default risk

This table reports coefficients from firm-panel regressions of a firm's default risk proxied by EDF by Bharath and Shumway (2008) on an indicator for whether the firm's state of incorporation has adopted other anti-takeover laws also built in the takeover index, firm fixed effects (FE), state-of-location-by-year FE, and standard industrial classification industry-by-year FE. The dependent variables are default risk. The sample includes firm-year observations from 1975 to 2007. I winsorize continuous variables at the 1st and 99th percentiles. Variable definitions are provided in Appendix B. Standard errors are adjusted for clustering at the state of incorporation level. T values are reported in parentheses. *, **, and ***, indicate statistical significance at the 10, 5, and 1 per cent level, respectively.

	Dependent Variable							
Independent Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	EDF	EDF	EDF	EDF	EDF	EDF	EDF	EDF
CS law								-0.011**
								(-2.272)
AC law	0.001							-0.001
	(-0.423)							(-0.447)
AG law		0.001						-0.000
		(-0.202)						(-0.207)
CSCO law			-0.027***					-0.025***
			(-4.688)					(-4.213)
Disg law				-0.011***				-0.002
				(-3.358)				(-0.577)
FG law					0.001			-0.001
					(-0.266)			(-0.478)
GPR law						-0.002		0.000
						(-0.208)		(-0.016)
TPB law							-0.010***	0.006
							(-3.789)	(-0.847)
Ln (Equity)	-0.049***	-0.049***	-0.049***	-0.049***	-0.049***	-0.049***	-0.049***	-0.049***
	(-57.123)	(-57.181)	(-57.252)	(-57.213)	(-57.207)	(-57.359)	(-57.213)	(-57.343)
Ln (Debt)	0.046***	0.046***	0.046***	0.046***	0.046***	0.046***	0.046***	0.046***
	(-35.479)	(-35.455)	(-35.365)	(-35.417)	(-35.442)	(-35.422)	(-35.450)	(-35.385)
$1/\sigma_E$	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***
	(-16.188)	(-16.155)	(-16.079)	(-16.103)	(-16.183)	(-16.168)	(-16.155)	(-16.070)
Income /Assets	-0.063***	-0.063***	-0.063***	-0.063***	-0.063***	-0.063***	-0.063***	-0.064***
	(-13.898)	(-13.898)	(-13.876)	(-13.857)	(-13.897)	(-13.901)	(-13.865)	(-13.831)
Excess Return	-0.064***	-0.064***	-0.064***	-0.064***	-0.064***	-0.064***	-0.064***	-0.064***
	(-27.637)	(-27.637)	(-27.660)	(-27.621)	(-27.626)	(-27.650)	(-27.615)	(-27.659)
Constant	0.144***	0.144***	0.145***	0.145***	0.144***	0.144***	0.145***	0.148***
	(-35.185)	(-33.226)	(-33.431)	(-33.696)	(-35.147)	(-33.957)	(-33.922)	(-31.744)
Observations	103,922	103,922	103,922	103,922	103,922	103,922	103,922	103,922
R-squared	0.412	0.483	0.483	0.483	0.483	0.483	0.483	0.483
Company FE	YES	YES	YES	YES	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
State-Year FE	YES	YES	YES	YES	YES	YES	YES	YES

4.6.4 Broad-based takeover index and expected default frequency (EDF)

My research objective is to examine the impact of takeover threats on default risk by utilizing the adoption of anti-takeover laws as a proxy for measuring takeover threats. In contrast to the approach taken by Balachandran et al. (2022), who employ a comprehensive takeover index developed by Cain et al. (2017), my study presents contradictory findings. The takeover index developed by Cain et al. (2017) incorporates various factors such as court decisions, state antitakeover laws, macroeconomic conditions, and firm characteristics to derive fitted values for hostile acquisition likelihood. However, this index has faced criticism due to concerns of endogeneity. It is argued that the inclusion of endogenous firm characteristics prevents it from approximating an exogenous measure of takeover protection, and it may not be applicable to acquisitions that are not classified as hostile (Karpoff and Wittry, 2018). In order to enhance the robustness of my results, I extend my analysis to investigate the influence of the Cain et al. (2017) takeover index on default risk.

Balachandran et al. (2022) posit that the market for corporate control functions as a disciplinary mechanism, thereby mitigating default risk resulting from managerial incentives. Their findings indicate that default risk tends to rise when there is an increase in anti-takeover protection, which effectively eliminates external monitoring mechanisms. However, it has been contended that in the absence of external market control, managerial opportunistic behavior can actually benefit debtholders (Bertrand and Mullainathan, 2003; Klock et al., 2005; Chava et al., 2009; Qiu and Yu, 2009; Gormley and Matsa, 2016). This suggests that the strengthening of anti-takeover protection may not necessarily lead to an escalation in default risk. Drawing on this argument, my study offers an alternative viewpoint by demonstrating that managers possess discretion to reduce debt when corporate governance is weakened (Garvey and Hanka, 1999), thereby implying that default risk may decrease subsequent to the enhancement of anti-takeover protection.

Furthermore, Balachandran et al. (2022) incorporate year, state, and industry fixed effects in their baseline regression analysis. In contrast, I introduce CS (country-specific) laws and employ a difference-in-differences methodology with high-dimensional fixed effects, including firm, state-by-year, and industry-by-year fixed effects. This approach, inspired by the methodology proposed by Gormley and Matsa (2016), helps to mitigate potential alternative explanations and enhance the robustness of my analysis.

The time frame examined in Balachandran et al. (2022) extends from 1994 to 2015, whereas my analysis covers the years between 1975 and 2007. This broader time span ensures a minimum of ten years of data both before and after the implementation of each CS law, allowing for an adequate observation of changes in the level of corporate default risk. Notably, Balachandran et al. (2022) incorporate a larger set of control variables compared to my study. This disparity arises because my sample period is significantly longer, and some of the control variable data have substantial gaps. Consequently, I adopt the control variables utilized by Bharath and Shumway (2008), which are also supported by Brogaard et al. (2017).

In Panel A of ***Table 4.4*** presented below, the sample period aligns with that of Balachandran et al. (2022), while in Panel B, the sample period corresponds to my baseline regression. I find that the results in Column (1), (4), (7), and (10) of ***Table 4.4*** are consistent with the findings of Balachandran et al. (2022), which indicate a negative relationship between the hostile takeover index developed by Cain et al. (2017) and the measure of default risk (EDF) by Bharath and Shumway (2008), employing the same fixed effects as Balachandran et al. (2022). The distinction lies in the fact that, in Column 1 and 4, I only incorporate the control variables used in my baseline regression, while in Column 7 and 10, I include the key control variables utilized by Balachandran et al. (2022), suggesting that the choice of control variables does not yield different results. However, all other coefficients are statistically insignificant. Thus, I ascertain that the findings of Balachandran et al. (2022) are not valid when firm fixed effects are considered, indicating the significance of employing fixed effects in my analysis.

Table 4.4. Panel A. Effect of takeover index on default risk - Sample period spanning from 1994 to 2015

This table shows the regression results for the impact of the threat of takeover, proxied by the takeover index following Cain et al. (2017), on default risk measured by EDF. The sample period aligns with that of Balachandran et al. (2022). I lag all independent variables by one year to mitigate the issue of reverse causality. I winsorize continuous variables at the 1st and 99th percentiles. I present the T-values in brackets. Standard errors are clustered at the firm and year levels. Variable definitions are provided in Appendix B. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A	***Dependent Variable***					
Independent Variables	(1) EDF	(2) EDF	(3) EDF	(4) EDF	(5) EDF	(6) EDF
Hostile index	-0.095***	-0.016	-0.058	-0.059***	0.079	-0.001
	(-4.042)	(-0.296)	(-1.234)	(-3.223)	(1.466)	(-0.026)
Ln (Equity)	-0.058***	-0.048***	-0.044***	-0.071***	-0.062***	-0.056***
	(-8.336)	(-8.620)	(-8.723)	(-7.717)	(-7.623)	(-8.130)
Ln (Debt)	0.055***	0.042***	0.040***	0.068***	0.052***	0.049***
	(8.291)	(9.811)	(9.812)	(8.036)	(9.079)	(9.221)
$1/\sigma_E$	-0.004***	-0.003***	-0.002***	-0.000	0.000	0.000
	(-6.269)	(-5.131)	(-5.166)	(-0.270)	(0.208)	(0.903)
Income /Assets	-0.041***	-0.042***	-0.038***	0.115***	0.098***	0.086***
	(-3.898)	(-4.546)	(-4.459)	(4.125)	(4.153)	(4.103)
Excess Return	-0.054***	-0.053***	-0.048***	0.313	0.192	0.129
	(-6.878)	(-7.765)	(-7.852)	(0.688)	(0.565)	(0.409)
Ln (MTB)				0.020***	0.020***	0.018***
				(6.366)	(5.670)	(6.618)
Ln (LOSS)				0.041***	0.034***	0.030***
				(6.199)	(5.835)	(5.770)
Ln (long-term ownership)				-0.028**	-0.030*	-0.062***
				(-2.119)	(-1.941)	(-4.346)
Ln (total ownership)				0.024**	0.026*	0.050***
				(2.363)	(1.929)	(3.896)
Ln (Spread)				0.392***	0.360***	0.351***
				(8.855)	(9.953)	(10.728)
Ln (Total assets)				0.000	0.000**	0.000**
				(1.489)	(2.358)	(2.404)
Ln (Cashflow)				-0.045**	-0.066***	-0.061**
				(-2.247)	(-2.889)	(-2.584)
Ln (Return)				-0.375	-0.253	-0.186
				(-0.833)	(-0.756)	(-0.598)
Constant	0.203***	0.178***	0.163***	0.080	0.069	0.056
	(11.243)	(6.236)	(6.583)	(1.552)	(1.689)	(1.302)
Observations	56,715	55,696	54,545	56,715	55,696	54,545
R-squared	0.254	0.402	0.512	0.303	0.434	0.535
Company FE	-	YES	YES	-	YES	YES
Industry-Year FE	-	-	YES	-	-	YES
State-Year FE	-	-	YES	-	-	YES

Table 4.4. Panel B. Effect of takeover index on default risk - Sample period spanning from 1975 to 2007

This table shows the regression results for the impact of the threat of takeover, proxied by the takeover index following Cain et al. (2017), on default risk measured by EDF. The sample period corresponds to my baseline regression. I lag all independent variables by one year to mitigate the issue of reverse causality. I winsorize continuous variables at the 1st and 99th percentiles. I present the T-values in brackets. Standard errors are clustered at the firm and year levels. Variable definitions are provided in Appendix B. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel B	***Dependent Variable***					
Independent	(7)	(8)	(9)	(10)	(11)	(12)
Variables	EDF	EDF	EDF	EDF	EDF	EDF
Hostile index	-0.089***	-0.003	-0.014	-0.056***	0.080*	0.039
	(-4.065)	(-0.076)	(-0.319)	(-3.254)	(1.747)	(0.940)
Ln (Equity)	-0.057***	-0.048***	-0.045***	-0.070***	-0.061***	-0.056***
	(-8.732)	(-9.720)	(-10.010)	(-8.258)	(-8.399)	(-9.067)
Ln (Debt)	0.054***	0.042***	0.040***	0.067***	0.051***	0.048***
	(8.649)	(9.831)	(9.821)	(8.576)	(9.658)	(9.648)
Stock volatility	-0.004***	-0.002***	-0.002***	-0.000	0.000	0.001
	(-6.954)	(-5.509)	(-5.557)	(-0.131)	(0.410)	(1.121)
Income /Assets	-0.044***	-0.046***	-0.041***	0.074***	0.038**	0.031**
	(-4.457)	(-5.306)	(-5.279)	(4.044)	(2.582)	(2.497)
Excess Return	-0.053***	-0.051***	-0.047***	0.340	0.288	0.240
	(-7.481)	(-8.251)	(-8.310)	(0.820)	(0.871)	(0.776)
Ln (MTB)				0.020***	0.020***	0.018***
				(6.946)	(6.406)	(7.417)
Ln (LOSS)				0.042***	0.035***	0.031***
				(6.564)	(6.377)	(6.223)
Ln (long-term				-0.033**	-0.028*	-0.064***
				(-2.594)	(-1.969)	(-4.847)
Ln (total ownership)				0.026***	0.026**	0.051***
				(2.846)	(2.229)	(4.614)
Ln (Spread)				0.393***	0.359***	0.351***
				(9.563)	(10.519)	(11.443)
Ln (Total assets)				0.000*	0.000***	0.000***
				(1.951)	(3.348)	(3.221)
Ln (Cashflow)				-0.400	-0.348	-0.295
				(-0.975)	(-1.062)	(-0.964)
Constant	0.200***	0.173***	0.157***	0.078	0.075*	0.061
	(11.827)	(7.029)	(7.280)	(1.653)	(1.876)	(1.470)
Observations	66,481	65,453	64,069	66,481	65,453	64,069
R-squared	0.249	0.391	0.502	0.300	0.423	0.526
Company FE	-	YES	YES	-	YES	YES
Industry-Year FE	-	-	YES	-	-	YES
State-Year FE	-	-	YES	-	-	YES

4.7. The effects of anti-takeover protection on agency costs and shareholder outcomes

4.7.1 The influence of anti-takeover protection on agency conflicts

As demonstrated earlier, the impact of anti-takeover protection on default risk can be attributed to various agency conflicts encompassing both the agency cost of debt and equity. Building upon the illustrations and empirical findings in the previous section, I have already established a general confirmation that the adoption of control share acquisition (CS) laws leads to a reduction in default risk for companies incorporated in affected states. This finding eliminates the possibility that the agency cost of debt alone can account for my results. Hence, to validate my hypothesis and explore a potential mechanism, I investigate the effects of anti-takeover protection on agency costs of equity.

To proxy agency costs, I adopt the expense ratio following the approach by Angetul (2000). Specifically, the expense ratio is computed as the ratio of operating expenses to revenues, serving as a measure of managerial control over operating costs, including excessive perquisite consumption and other direct agency costs. This means that if agency costs of companies increase, this measure tends to also increase.

The results, presented below in Column (1) of ***Table 4.5***, indicate that the estimated coefficient of CS laws on the expense ratio is 0.05967, which is statistically significant at the 5% level. In addition to assessing statistical significance, I further explore the economic significance of the findings. On average, firms incorporated in states that have implemented CS laws experience an 5.2% increase in expense ratio relative to the mean expense ratio (1.13) during the sampled period. The findings indicate that the adoption of anti-takeover laws is associated with a decrease in managerial control over operating costs, resulting in an escalation of agency costs of equity, which support my second hypothesis. Additionally, these empirical results provide evidence supporting the argument that managers exercise discretion in favoring debt, thereby reducing default risk.

4.7.2 The influence of decreased default risk following the adoption of CS laws on shareholder outcomes

Based on the aforementioned analysis, I have obtained a general confirmation that the reduction in default risk resulting from the adoption of control share acquisition (CS) laws is primarily driven by the agency cost of equity. And I further investigate the direct impact of decreased default risk on shareholder outcomes. To ensure a robust assessment of the effect of reduced default risk on shareholder outcomes subsequent to the implementation of anti-takeover laws, I employ a difference-in-difference model. This model incorporates key indicators such as LOW_EDF (indicator for samples with EDF lower than the sample mean), the adoption indicator for CS laws, and their interaction term.

The results in Column (2) and (3) of ***Table 4.5*** reveal that the coefficient estimates of the interaction term on return on assets (ROA) and return on equity (ROE) are both negative and statistically significant. This suggests that a lower default risk following the enactment of anti-takeover laws leads to a decrease in shareholder outcomes. My findings provide support for the argument that managers, when provided with increased free cash flow resulting from reduced debt, tend to allocate it towards projects with a negative net present value (NPV) rather than distributing it to shareholders, which adversely affects shareholders' outcomes (Jensen, 1988). These results further substantiate the notion that managers possess the discretion to choose debt and subsequently reduce default risk. Moreover, they indicate that managers prioritize their own interests and harm the interests of shareholders following the adoption of CS laws, which supports my third hypothesis.

4.7.3 The influence of anti-takeover protection on investment

As I have empirically illustrated above, the implementation of anti-takeover laws leads to a reduction in default risk but adversely affects shareholder benefits for companies incorporated in affected states. In other words, the protection against takeovers harms shareholders while benefiting debtholders, which contradicts the research conducted by Balachandran et al. (2022), suggesting that a decreased threat of takeovers motivates managers to increase default risk for their personal gains. However, it is important to note that both my study and Balachandran et al. (2022) support the argument that corporate governance weakens following the adoption of anti-takeover laws. This implies that the conventional agency conflicts of "active empire building" may not be the norm for managers' actions that deviate from the interests of shareholders.

Recent investigations by Bertrand and Mullainathan (2003) and Gormley and Matsa (2016) provide new insights into agency conflicts. These studies explore the objectives pursued by managers beyond the traditional notion of "active empire building." Anti-takeover protections, which make it challenging to terminate underperforming managers, diminish managers' concerns about their career prospects as they are potentially offered long-term contracts (Grossman and Hart, 1980; Knoeber, 1986; Scherer, 1988; Stein, 1988). Consequently, managers also have an incentive to "enjoy the quiet life" and exert less effort (Holmström, 1979; Grossman and Hart, 1983; Bertrand and Mullainathan, 2003). Additionally, Stein (1988) argues that the implementation of anti-takeover laws encourages managers to be risk-averse. They opt for lower risk-taking to minimize negative outcomes that could have personal costs (Sundheim, 2013), even engaging in value-decreasing activities to reduce the firm's overall risk (Jensen and Meckling, 1976; Amihud and Lev, 1981; Smith and Stulz, 1985; Holmstrom, 1999). my findings align with these perspectives, supporting the view that the increased adoption of anti-takeover provisions motivates managers to "enjoy a quiet life" or "play it safe," thereby detrimentally impacting shareholders but satisfying debtholders' interests (Bertrand and Mullainathan, 2003; Klock et al., 2005; Chava et al., 2009; Qiu and Yu, 2009; Gormley and Matsa, 2016).

The primary distinction between the agency conflicts of "enjoying a quiet life" and "playing it safe" lies in the level of effort exerted by managers. To differentiate between these two conflicts, I examine the impact of anti-takeover protection on investment levels, following Richardson's (2006) approach for measuring managerial investment level. Richardson (2006) employs an accounting-based framework to define and measure over-investment, thereby allowing a more powerful test of the agency-based explanation for why firm level investment is related to internally generated cash flows. This is similar to the topic of this paper, which is why I chose Richardson's (2006) approach for my study.

My empirical results, displayed below in Column (4) of ***Table 4.5***, show a significant decrease in the investment level of companies incorporated in affected states after the adoption of anti-takeover laws. Specifically, the estimated coefficient of CS laws is -0.00941, which is statistically significant at the 5% level. This decrease indicates underinvestment. This implies that the agency conflict of 'enjoying a quiet life' may be the underlying reason for managers' reduced reliance on debt. And the findings provide further empirical evidence that, following the adoption of CS laws, agency costs of equity are positively influenced for companies incorporated in affected states. Additionally, the decreased default risk resulting from increased anti-takeover protection negatively influences shareholder outcomes for companies incorporated in affected states.

Table 4.5. Effect of anti-takeover protection on managerial activities and shareholder outcomes

This table reports coefficients from firm-panel regressions of a firm's agency costs, shareholder benefits measured as ROA and ROE and investment level on an indicator for whether the firm's state of incorporation has adopted a control share acquisition law, firm fixed effects (FE), state-of-location-by-year FE, and standard industrial classification industry-by-year FE. I lag all independent variables by one year to mitigate the issue of reverse causality. The sample includes firm-year observations from 1975 to 2007. I winsorize continuous variables at the 1st and 99th percentiles. I present the T-values in brackets. Standard errors are clustered at the firm and year levels. Variable definitions are provided in Appendix B. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variables			
Independent Variables	(1) Expense ratio	(2) ROA	(3) ROE	(4) Investment
CS law	0.059**	0.006	0.041*	-0.009**
	(-2.136)	(0.898)	(1.922)	(-2.217)
LOW_EDF		0.075***	0.476***	
		(20.762)	(40.453)	
LOW_EDF * CS		-0.010*	-0.040**	
		(-1.678)	(-2.301)	
Ln (Equity)	0.056***	0.000	0.032***	0.006***
	(-10.746)	(0.084)	(9.434)	(5.989)
Ln (Debt)	-0.148***	-0.001	-0.042***	-0.048***
	(-12.927)	(-1.219)	(-20.109)	(-35.468)
$1/\sigma_E$	-0.001***	0.001***	0.001***	0.001***
	(-3.077)	(6.510)	(7.246)	(5.849)
Income /Assets	-0.666***	0.203***	0.065***	-0.001
	(-15.708)	(23.307)	(3.846)	(-0.277)
Excess Return	-0.023***	0.028***	0.030***	0.029***
	(-2.805)	(27.527)	(19.296)	(23.227)
Constant	1.398***	-0.100***	-0.515***	0.144***
	(-32.791)	(-22.541)	(-54.156)	(29.007)
Observations	102,528	106,644	106,559	69,221
R-squared	0.697	0.638	0.487	0.200
Company FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
State-Year FE	YES	YES	YES	YES

4.7.4 Role of institutional shareholders

Based on previous analyses, I have identified that the implementation of control share acquisition (CS) laws leads to an increase in agency costs of equity, thereby negatively impacting shareholders outcomes. my study demonstrates that the existence of agency conflicts serves as a channel linking anti-takeover protections with default risk. In other words, the change in default risk levels among affected firms primarily stems from weakened corporate governance subsequent to the implementation of anti-takeover protections. If the influence of anti-takeover protection on default risk indeed arises from the role of hostile takeovers in overseeing managerial behavior, I anticipate a more pronounced effect of CS laws' adoption on firms subject to heightened monitoring by external governance mechanisms.

In the realm of corporate governance, institutional investors are widely recognized as significant monitoring agents (Hartzell and Starks, 2003; Chen et al., 2007; Chung and Zhang, 2011). Institutional investors use both "voice" and "exit" strategies to exert their influence (Levit, 2013; Edmans, 2014). The "voice" strategy involves direct intervention measures such as proxy voting, submitting shareholder proposals, corresponding with the board, and maintaining communication with company management. These actions are taken to express dissatisfaction with management (Shleifer and Vishny, 1986; Maug, 1998; Harris and Raviv, 2010; Levit and Malenko, 2011). On the other hand, the "exit" strategy involves selling shares or threatening to sell them (Parrino et al., 2003; Admati and Pfleiderer, 2009; Edmans, 2009; Edmans and Manso, 2011). Companies with poor corporate governance practices are seen as imprudent and fail to fulfill their fiduciary responsibility to institutional investors. Such companies may reduce dividends and experience volatile stock prices (Parrino et al., 2003). As a result, institutional investors often divest from underperforming companies. Thus, I introduce institutional shareholding as a variable to explore the heterogeneous treatment effects.

Furthermore, my investigation delves into the specific categorization of institutional investors that assume a more pronounced monitoring role. Building upon Bushee's (1998) classification framework, I differentiate institutional investors as either "long-term" or "short-term" entities, based on their anticipated investment horizon. Long-term institutions distinguish themselves by providing capital with an extended time horizon, affording them the capacity to adopt a patient stance in their interactions with portfolio firms (Porter, 1992). This patient approach is accompanied by a heightened level of influence and accountability, as evidenced by their engagement in activism (Black, 1992; Gibson, 1990; Millstein, 1991). Consequently, long-term institutional investors exhibit a robust incentive to allocate resources towards actively monitoring their portfolio firms (Parrino et al., 2003). Moreover, their ability to distribute the costs and benefits of ownership over a prolonged period confers upon them a comparative advantage in effectively overseeing managerial actions (Gaspar et al., 2005; Chen et al., 2007). Empirical support for this perspective is provided by Harford et al. (2018), who establish that the monitoring presence of long-term institutional investors contributes to decision-making processes aimed at maximizing shareholder value.

To test this conjecture, I classify my sample by the median of institutional shareholding and long-term institutional shareholding to re-conduct the baseline regression using subsamples separately, examining the effects of the passage of control share acquisition (CS) laws on default risk measured by EDF, following Bharath and Shumway's (2008) methodology. As expected, in the high institutional shareholding sample, the effects of CS laws' passage on default risk are more pronounced. More importantly, the effects of CS laws' passage on default risk are more pronounced in the high long-term institutional shareholding sample as well. The results presented in ***Table 4.6*** are consistent with my prediction in the hypothesis development section that, following the adoption of anti-takeover laws, the decreased level of default risk in affected companies is due to worse corporate governance and their discretion to decrease the use of debt.

Table 4.6. Cross-sectional variation in the effect of CS laws on default risk

This table report the results on how the relation between the adoption of CS laws and default risk varies between different levels of institutional ownership. I use two measures of institutional ownership: total institutional shareholding and long-term institutional shareholding. For each fiscal year in the sample period, I sort firms into two groups based on the median value of each of the institutional ownership measures. The sample includes firm-year observations from 1975 to 2007. I winsorize continuous variables at the 1st and 99th percentiles. I present the T-values in brackets. Standard errors are clustered at the firm and year levels. Variable definitions are provided in Appendix B. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable			
Independent Variables	(1) High Institutional Shareholding EDF	(2) Low Institutional Shareholding EDF	(3) High LT-Institutional Shareholding EDF	(4) Low LT-Institutional Shareholding EDF
CS law	-0.017***	-0.012	-0.018***	-0.024
	(-4.137)	(-1.422)	(-3.941)	(-1.555)
Ln (Equity)	-0.028***	-0.041***	-0.032***	-0.036***
	(-23.057)	(-29.762)	(-21.252)	(-20.176)
Ln (Debt)	0.020***	0.049***	0.024***	0.048***
	(19.403)	(34.269)	(19.618)	(22.891)
$1/\sigma_E$	-0.001***	-0.003***	-0.001***	-0.003***
	(-6.808)	(-12.805)	(-6.704)	(-10.457)
Income /Assets	-0.021***	-0.048***	-0.027***	-0.048***
	(-3.916)	(-10.505)	(-4.010)	(-8.232)
Excess Return	-0.030***	-0.050***	-0.036***	-0.047***
	(-22.546)	(-24.330)	(-25.780)	(-17.565)
Constant	0.127***	0.127***	0.133***	0.115***
	(15.594)	(20.009)	(16.121)	(12.450)
Observations	31,489	30,602	41,910	20,083
R-squared	0.483	0.602	0.485	0.652
Company FE	YES	YES	YES	YES
Industry-Year FE	YES	YES	YES	YES
State-Year FE	YES	YES	YES	YES

4.8. Other robustness and diagnostic tests

4.8.1 Adoption of CS laws and alternative measures of default risk

To ensure the robustness of my findings, I also consider alternative measures of default risk. First, the complete general equilibrium theory proposed by Merton (1974) illustrates that corporate equity can be viewed as a call option on the underlying value of the firm's assets, with the debt face value representing the strike price of the option. These arguments imply that default occurs when the value of the company's assets falls below its debt face value. Hence, there is a strong rationale for considering the financial leverage ratio as a leading indicator for predicting default risk (Traczynski, 2017; Cathcart et al., 2019). Furthermore, Garvey and Hanka (1999) find that managers tend to decrease the use of debt following the enactment of anti-takeover laws, suggesting that these laws serve as a substitute for debt in extracting premiums from potential acquirers. Therefore, in this section, I explore the impact of the adoption of control share acquisition (CS) laws on the leverage ratio. Specifically, I utilize the total liabilities divided by total assets as a measure of the leverage ratio. The result is presented in ***Table 4.7.*** Notably, the statistically significant negative relationship between the passage of CS laws and the leverage ratio persists even after controlling for firm characteristics that are relevant for predicting the leverage ratio. More specifically, the estimated coefficient of CS laws, accounting for the control variables in the regression, is -0.00645, which is statistically significant at the 10% level. In terms of economic significance, on average, firms incorporated in states that have adopted CS laws experience an 3.4 % reduction in leverage ratio relative to the mean (0.19) during the sampled period. These results align with my hypothesis that companies influenced by the adoption of anti-takeover laws exhibit reduced reliance on debt due to managerial discretion, as illustrated above.

In addition, I incorporate the Z-score as an alternative metric for assessing default risk. The Altman Z-score represents a weighted average of five accounting ratios that assess operating efficiency, total asset turnover, leverage ratio, asset liquidity, and earning power, which is developed as a tool for predicting company bankruptcy. In order to classify companies, I create a dummy variable, Z-score, which takes a value of 1 if the original Altman Z-score falls below the bankruptcy threshold of 1.81, and 0 otherwise.

Building upon the aforementioned demonstration, I perform a regression analysis to investigate the effects of the passage of control share acquisition (CS) laws on the Z-score, as outlined above. The result is presented in ***Table 4.7***. Importantly, even after controlling for firm characteristics that are known to be associated with default risk, a significantly negative relationship between the passage of CS laws and default risk remains evident. The estimated coefficient of CS laws, when considering the control variables in the regression, is -0.10252, which is statistically significant at the 1% level. This means the adoption of CS laws leads to a decrease in the possibility of default risk. These results align with my prediction in the hypothesis development section that companies influenced by the adoption of anti-takeover laws experience weakened corporate governance, leading managers to decrease default risk due to career concerns and risk aversion.

Table 4.7. Effect of takeover protection on alternative measures of default risk

This table reports coefficients from firm-panel regressions of effect of takeover protection on alternative measures of default risk. The sample for Z score and leverage includes firm-year observations from 1975 to 2007. I winsorize continuous variables at the 1st and 99th percentiles. I present the T-values in brackets. Standard errors are clustered at the firm and year levels. Variable definitions are provided in Appendix B. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variables	
Independent Variables	(1) Z score	(2) Leverage
CS law	-0.102*	-0.006*
	(-2.749)	(-1.769)
Ln (Equity)	0.167***	-0.014***
	(-7.312)	(-9.426)
Ln (Debt)	0.046***	-
	(-5.264)	-
$1/\sigma_E$	0.015***	-0.001***
	(-9.016)	(-13.341)
Income /Assets	3.273***	-0.027***
	(-49.364)	(-7.242)
Excess Return	0.192***	-0.025***
	(-24.536)	(-25.113)
Constant	1.226***	0.274***
	(-19.744)	(36.697)
Observations	103,108	103,448
R-squared	0.195	0.749
Company FE	YES	YES
Year FE	YES	
Industry-Year FE		YES
State-Year FE		YES

4.8.2 Stacked difference-in-differences estimation

This study examines the impact of anti-takeover protection on default risk by introducing the staggered adoption of control share acquisition (CS) laws in different states of the United States. To account for staggered treatments, I employ a difference-in-differences estimation method. This approach allows me to compare the effects of takeover legislation on states subject to the treatment (referred to as the treatment group) with states unaffected by such changes (referred to as the control group). This methodology helps address the challenge of multiple exogenous shocks occurring at different time points.

By using this method, I successfully address the issue of reverse causality between the adoption of anti-takeover protection and the level of default risk. In contrast, single shock situations pose an identification challenge as the shock itself coincides with incidental noise, directly impacting the dependent variable (Roberts and Whited, 2013).

Nonetheless, Cengiz et al. (2019) have drawn attention to potential econometric concerns associated with aggregating discrete DiD estimates using ordinary least squares (OLS), including the presence of heterogeneous treatment effects and potential negative weights assigned to specific treatments. To ensure a more accurate examination, I employ stacked difference-in-differences (DID) estimates as a robustness check. This approach transforms the staggered adoption design into a two-group, two-period design, allowing for a better assessment of treatment effects, taking into account the relative sizes of the group-specific datasets and the variance of treatment status within those datasets. Separate datasets are stacked, each containing observations on treated and control units for each treatment group. This approach improves the purity of the control group by excluding firm-year observations that have been treated, thus mitigating bias due to heterogeneous treatment effects (Goodman-Bacon, 2019).

I create a new dataset for each treatment event (i.e., when a state adopts the CS law), which includes firm-year observations within a window of five years before and after the event. The indicator variable Stacked CS Law takes the value of one after the firm is treated in an event year (i.e., $\tau > 0$) in each group, and zero otherwise. These group-specific datasets are stacked in event-time, and outcomes are regressed on treatment status, fixed effects for firm by Cohort combinations, and fixed effects for relative year by Cohort combinations. Standard errors are clustered by group by state.

These stacked regressions are of the form:

$$\boldsymbol{Default\ Risk_{itd}} = \boldsymbol{\alpha} + \boldsymbol{\beta} \times (\boldsymbol{T_{sd}} \times \boldsymbol{P_{td}}) + \boldsymbol{\gamma} \times \boldsymbol{X_{itd}} + \boldsymbol{\theta_{sd}} + \boldsymbol{\gamma_{td}} + \boldsymbol{\varepsilon_{itd}} \qquad \textbf{(6)}$$

where $\boldsymbol{i}$ indexes firms; $\boldsymbol{t}$ indexes relative year to each CS law adoption; $\boldsymbol{d}$ indexes dataset group by each CS law adoption event; $\boldsymbol{Default\ Risk_{itd}}$ is the dependent variable of interest. $\boldsymbol{T_{sd}}$ is an indicator that company s is a treated unit in sub-experiment ***d.*** $\boldsymbol{P_{td}}$ is an indicator that period ***t*** is in the post period in sub-experiment ***d***. I utilize the same control variables as my baseline regression. $\boldsymbol{\theta_{sd}}$ and $\boldsymbol{\gamma_{td}}$ are Firm by Cohort and relative year by Cohort fixed effects respectively. $\boldsymbol{\varepsilon_{itd}}$ is an error term. Specifically, I assign a firm's location based on the location of incorporation, which is typically also where major plants and operations are located (Henderson and Ono, 2008).

The coefficient on interaction term measures the impact of changes in state litigation regarding CS laws on a firm's default risk compared to rival companies in unaffected states. I present the estimation result in Column (1) of ***Table 4.8***. The difference-in-differences estimate is -0.00652, which is statistically significant at the 10% level, which confirms that my difference-in-differences estimates are not sensitive to heterogeneous treatment effects.

Table 4.8. Stacked difference-in-differences estimation and dynamic difference-in-differences estimation

Column (1) of this table reports results from stacked OLS difference-in-differences estimation of default risk on the indicator for the adoption of CS laws, by focusing on a window that contains the five years before and after the adoption of CS laws (and dropping states that ever-adopted CS laws). Column (2) and (3) report coefficients from firm-panel regressions of a firm's default risk proxied by EDF by Bharath and Shumway (2008) on a series of indicators for the timing of states passing control share acquisition law, firm fixed effects (FE), state-of-location-by-year FE, and standard industrial classification industry-by-year FE. The sample includes firm-year observations from 1975 to 2007. I winsorize continuous variables at the 1st and 99th percentiles. I present the T-values in brackets. Standard errors are clustered at the firm and year levels. Variable definitions are provided in Appendix B. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable		
Independent Variables	(1) EDF	(2) EDF	(3) EDF
Stacked CS law	-0.006*		
	(-1.712)		
$CSPassage^{-6}$		-0.006	-0.006
		(-1.249)	(-1.237)
$CSPassage^{-5}$		-0.001	-0.001
		(-0.144)	(-0.161)
$CSPassage^{-4}$		-0.004	-0.005
		(-0.555)	(-0.617)
$CSPassage^{-3}$		0.003	0.003
		(0.530)	(0.417)
$CSPassage^{-2}$		-0.009	-0.010
		(-1.193)	(-1.100)
$CSPassage^{-1}$			-.0137
			-1.543
$CSPassage^{0}$		-0.005	
		(-0.804)	
$CSPassage^{+1}$		-0.020***	-0.018***
		(-2.743)	(-3.220)
$CSPassage^{+2}$		-0.011*	-0.009
		(-1.736)	(-1.454)
$CSPassage^{+3}$		-0.008	-0.007*
		(-1.348)	(-1.745)
$CSPassage^{+4}$		-0.007	-0.006
		(-1.079)	(-1.039)
$CSPassage^{+5}$		-0.008	-0.007*
		(-1.162)	(-1.698)
$CSPassage^{+6}$		-0.006	-0.005
		(-0.935)	(-1.227)
Control variables	YES	YES	YES
Constant	0.214***	0.145***	0.145***
	(44.655)	(31.534)	(34.008)
Observations	687,405	103,424	103,424
R-squared	0.285	0.483	0.483
Company FE	-	YES	YES
Industry-Year FE	-	YES	YES
State-Year FE	-	YES	YES
Company-Cohort FE	YES	-	-
Event-Year- Cohort FE	YES	-	-

4.8.3 Dynamic difference-in-differences estimation

The fundamental assumption underlying the difference-in-differences methodology, employed in my baseline regression, is that in the absence of the law, two sets of companies would exhibit parallel trends. Specifically, in this study, I expect the change in default risk levels for firms incorporated in states that adopted control share acquisition laws to be similar to the change observed for firms incorporated in states that did not adopt such laws.

Therefore, this section focuses on examining the timing of changes in the level of default risk relative to the timing of adoptions of the CS laws to assess the validity of my assumption within the sample. If reverse causality is driving my findings, I would anticipate a declining trend in default risk levels for firms in affected states prior to the implementation of the control share acquisition laws.

To perform the dynamic difference-in-differences regression analysis, I adopt two different settings. The first setting follows the fully saturated model proposed by Gopalan et al. (2021), wherein the number of estimated parameters matches the number of data points. Additionally, this study designates the year immediately preceding the adoption year as the base year, excluding the corresponding dummy variable. my alternative setting aligns with Beck et al. (2010), also employing a fully saturated model, but excludes the treatment year from the analysis.

The key variables of interest are $CS\ Passage^{-6}$, $CS\ Passage^{-5}$, $CS\ Passage^{-4}$,

$CS\ Passage^{-3}$, $CS\ Passage^{-2}$, $CS\ Passage^{-1}$, $CS\ Passage^{0}$, $CS\ Passage^{+1}$, $CS\ Passage^{+2}$, $CS\ Passage^{+3}$, $CS\ Passage^{+4}$, $CS\ Passage^{+5}$ and $CS\ Passage^{+6}$. Specifically, $CS\ Passage^{-5}$, $CS\ Passage^{-4}$, $CS\ Passage^{-3}$, $CS\ Passage^{-2}$, $CS\ Passage^{-1}$, $CS\ Passage^{+1}$, $CS\ Passage^{+2}$, $CS\ Passage^{+3}$, $CS\ Passage^{+4}$ and $CS\ Passage^{+5}$ are equal to one if the firm is incorporated in a state that will pass the CS laws in five, four, three, two years and one year, in that year, adopted the CS laws one year ago, adopted the CS laws two, three, four and five years ago, and zero otherwise. At the end points, $CS\ Passage^{-6}$ equals one for all years that are six or more years before CS laws' adoption, while $CS\ Passage^{+6}$ equals one for all years that are 15 or more years after CS laws' adoption.

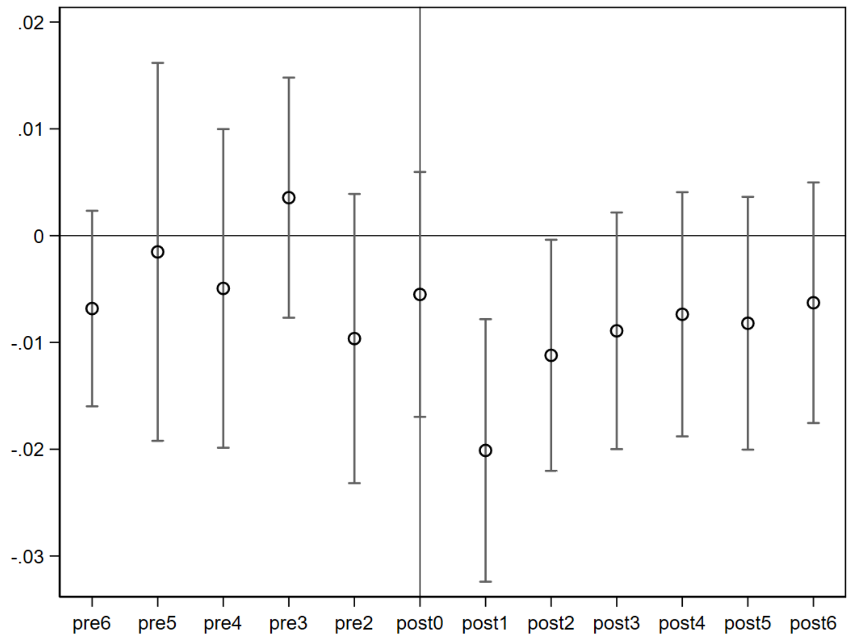
I present the estimation result in Column (2) and (3) of ***Table 4.8***. Importantly, my analysis reveals that the coefficients on the post indicators in both of the settings exhibit no statistical significance, aligning with the requirement that coefficients should not demonstrate significant results during the pre-event period. This finding indicates that firms in states that have adopted control share acquisition (CS) laws experience a decrease in their default risk levels relative to control firms only after the implementation of the CS laws, but not before. Therefore, there appears to be no discernible difference between the treatment group and the control group prior to the adoption of CS laws, suggesting that the parallel trend assumption of the difference-in-differences approach remains intact (Roberts and Whited, 2013).

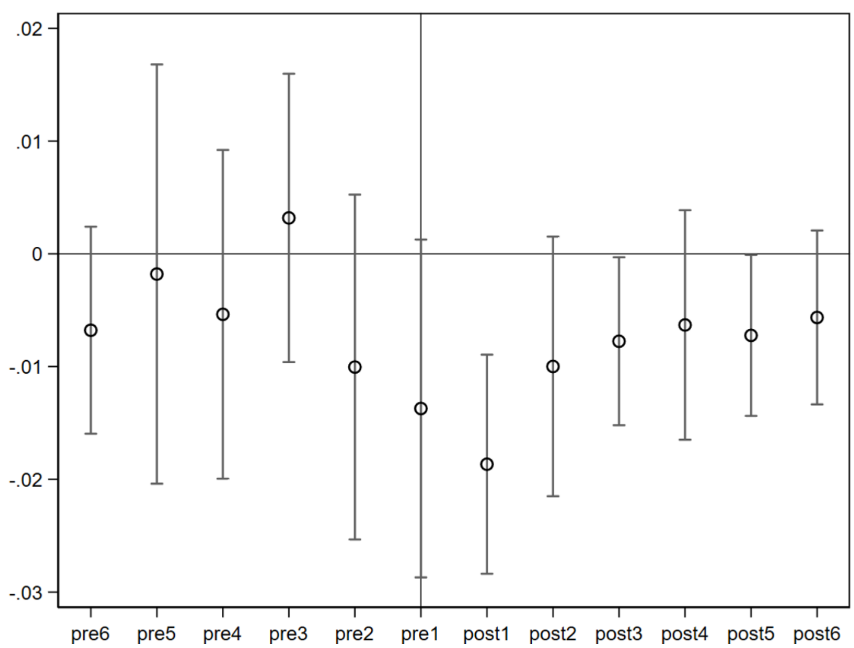
In comparison to the pre-treatment years, I observe an immediate decline in default risk following the enactment of CS laws, indicating a rapid impact of anti-takeover laws on default risk. In both settings, $CS\,Passage^{+1}$ exhibits a negative and statistically significant association at the 1% level. Moreover, these effects persist for a period of two years and five years in the first and second settings, respectively, demonstrating a lasting impact. These results further strengthen the argument that the negative effect of CS laws on default risk is not driven by reverse causality.

In addition, I provide graphs to visually illustrate the dynamic effects of CS law adoption on default risk for both of the above two settings. Specifically, the above graph shows the results of dynamic difference-in-differences regression following the setting of Gopalan et al. (2021), which designates the year immediately preceding the adoption year as the base year. The below graph, on the other hand, shows the results of dynamic difference-in-differences regression following the setting of Beck et al. (2010), which excludes the treatment year from the analysis. The graphs clearly depict that treatment and control companies exhibit statistically indistinguishable parallel trends prior to the adoption of CS laws in both settings. Furthermore, a decrease in default risk is observed immediately after the enactment of CS laws, accompanied by a remotely persistent effect. Overall, these empirical findings alleviate concerns regarding reverse causality and provide support for a causal effect.

Graph 4.1. Dynamic difference-in-differences regression

These graphs report results from OLS regressions of default risk proxied by EDF by Bharath and Shumway (2008) on a series of indicators for the timing of states passing control share acquisition law, which reflects the dynamic effects of CS law. Specifically, the above graph shows the results of dynamic difference-in-differences regression following the setting of Gopalan et al. (2021), which designates the year immediately preceding the adoption year as the base year. The below graph, on the other hand, shows the results of dynamic difference-in-differences regression following the setting of Beck et al. (2010), which excludes the treatment year from the analysis. The confidence interval is 95%. The sample spans 1975 to 2007. The key variables of interest are pre 6, pre 5, pre 4, pre 3, pre 2, pre 1, post 0, post 1, post 2, post 3, post 4, post 5 and post 6, which are equal to one are equal to one if the firm is incorporated in a state that will pass the CS laws in six or more, five, four, three, two years and one year, in that year, adopted the CS laws one year ago, adopted the CS laws two, three, four, five and six or more years ago, and zero otherwise.





4.8.4 Propensity Score Matching (PSM) Analysis

The potential for bias arises from the possibility that a disparity in treatment outcomes, such as the average treatment effect, between treated and untreated groups may be influenced by a factor that predicts treatment rather than the treatment itself. Randomized experiments effectively address this issue, which ensures unbiased estimation of treatment effects. Through randomization, treatment groups are expected to be balanced on average for each covariate, as dictated by the law of large numbers. However, in observational studies, treatment assignments are typically non-random. To mitigate the bias introduced by treatment assignment, matching techniques are employed to emulate randomization and create comparable samples of units that either received or did not receive the treatment.

In order to investigate the relationship between the adoption of CS laws and default risk with greater precision, I also employ Propensity Score Matching (PSM). PSM is a statistical matching technique used to estimate the effect of a treatment, policy, or intervention by accounting for the covariates that predict receiving the treatment. PSM attempts to reduce the bias due to confounding variables that could be found in an estimate of the treatment effect obtained from simply comparing outcomes among units that received the treatment versus those that did not (Rosenbaum and Rubin,1983).

In my study, I compare the default risk of companies facing a high takeover threat with the default risk of companies facing a low takeover threat but are otherwise equivalent. Firms headquartered in states that have adopted the CS laws constitute my treatment group, while those in states that have not adopted the CS laws serve as my control group. For each year, I employ matching techniques to pair treatment firms with control firms based on firm characteristics used as control variables in my baseline regression. I estimate the probability of being assigned to the treatment or control group employing a logit regression using all control variables and year, state of headquarter, and industry fixed effects as in my baseline regression. Subsequently, I utilize the propensity scores derived from this logit estimation and conduct matching within a caliper of 0.01 without replacement.

The empirical results are presented in ***Table 4.9***. For the majority of the variables used in the matching process, there are no statistically significant differences between the firm characteristics of the treatment and control groups. Furthermore, the EDF for companies with a low threat of takeover is significantly lower compared to its counterpart. This suggests that the negative relationship between the adoption of CS laws and default risk does not stem from sample selection bias. Notably, the coefficient estimate of the CS law is -0.055 and statistically significant at the 5% level. This finding emphasizes that my results are not influenced by systematic disparities between firms facing high and low levels of takeover threat. Overall, the results obtained through the PSM method, which controls for sample selection bias, align with my baseline findings, indicating that following the implementation of anti-takeover laws, managers tend to reduce default risk due to their discretion in minimizing debt usage, in line with the trade-off theory of capital structure.

Table 4.9. Propensity Score Matching (PSM) test

This table reports coefficients from firm-panel regressions of a firm's default risk proxied by EDF by Bharath and Shumway (2008) on the indicator for the passing control share acquisition law, firm fixed effects (FE), state-of-location-by-year FE, and standard industrial classification industry-by-year FE using propensity score matching (PSM) approach. Panel A shows the results of the comparison of the characteristics of the treatment and control firms. Panel B presents the results of the impact of CS laws' passage on EDF based on the matched sample. I winsorize continuous variables at the 1st and 99th percentiles. I present the T-values in brackets. Standard errors are clustered at the firm and year levels. Variable definitions are provided in Appendix B. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Descriptive statistics for the matched sample

Control Variables	Treatment Firms	Control Firms	t-test
EDF	0.058	0.066	-4.75
Ln (Equity)	4.731	4.664	2.52
Ln (Debt)	3.731	3.699	1.15
Stock volatility	9.507	9.483	0.32
Income /Assets	-0.015	-0.011	-1.55
Excess Return	0.035	0.036	-0.13

Panel B: PSM Regression Analysis

Dependent Variable	EDF
CS law	-0.054**
	(-2.659)
Ln (Equity)	-0.046***
	(-17.239)
Ln (Debt)	0.045***
	-8.684
$1/\sigma_E$	-0.002***
	(-5.448)
Income /Assets	-0.077***
	(-5.267)
Excess Return	-0.062***
	(-17.991)
Constant	0.168***
	-10.716
Observations	22,398
R-squared	0.592
Company FE	YES
Industry-Year FE	YES
State-Year FE	YES

4.8.5 Placebo test

In accordance with the findings of Bertrand et al. (2004), who demonstrate that difference-in-differences analyses conducted on long time series may lead to an inflation of t-statistics and significance levels when there is correlation within observations within each unit, I take measures to ensure that my results are not purely attributable to chance. To accomplish this, I perform a placebo test. Specifically, for each state that adopts CS laws, I assign a pseudo passage year, randomly selected, in order to ensure that each state has an equal probability of adopting CS laws. This approach guarantees that any observed differences between and within states are not systematically driven. Subsequently, I estimate the baseline regressions based on these pseudo-event years. If I contend that the impact on default risk can be attributed to and is causally linked to takeover threats, I should not observe a positive and statistically significant relationship between the Expected Default Frequency (EDF) and the randomly assigned passage of CS laws. The outcomes of the placebo tests are presented in ***Table 4.10***, which clearly illustrates that the randomly assigned passage of CS laws does not exert any influence. Additionally, I employ a placebo test for the utilization of debt and Z-score to provide further support for my primary findings. Collectively, my placebo test affirms the validity of my previous findings.

Table 4.10. Placebo test

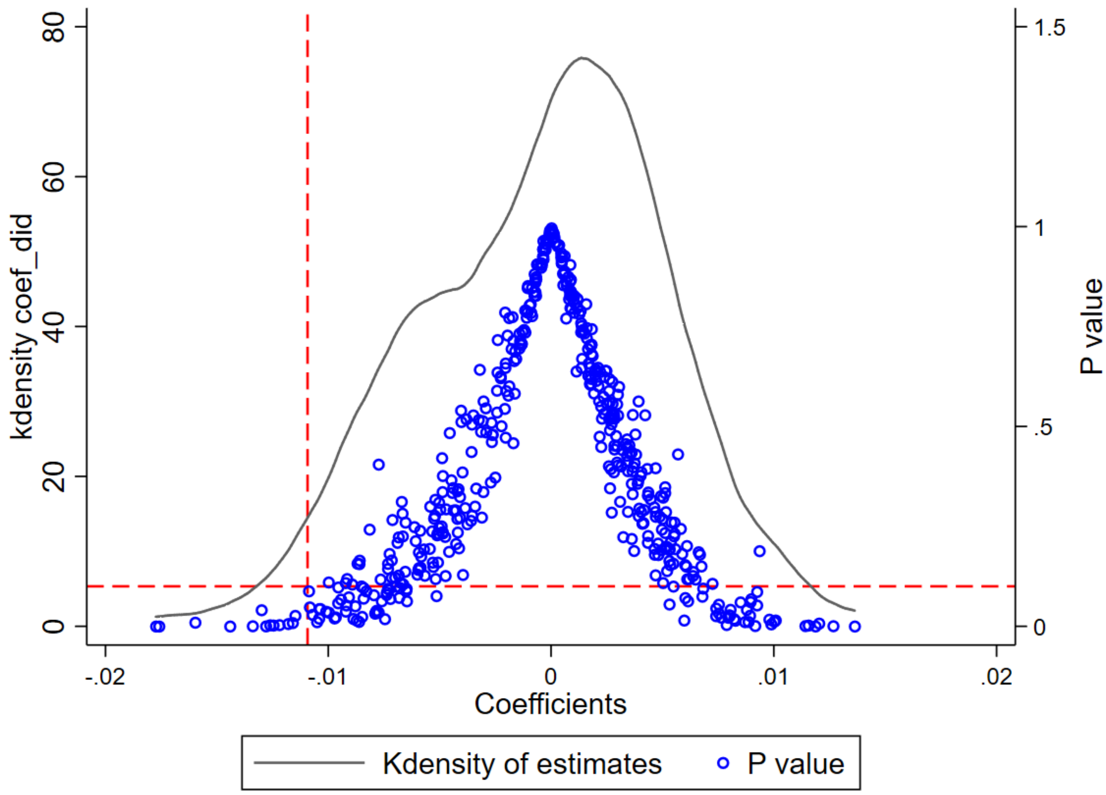
This table reports coefficients from firm-panel regressions of series measures of default risk on the indicator for randomly assigned passage of control share acquisition laws, firm fixed effects (FE), state-of-location-by-year FE, and standard industrial classification industry-by-year FE. I winsorize continuous variables at the 1st and 99th percentiles. I present the T-values in brackets. Standard errors are clustered at the firm and year levels. Variable definitions are provided in Appendix B. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variables		
Independent Variables	(1) EDF	(2) Leverage	(3) Z score
Placebo CS law	0.001	0.003	-0.012
	(0.557)	(0.997)	(-1.519)
Ln (Equity)	-0.049***	0.172***	0.187***
	(-55.842)	(37.781)	(6.202)
Ln (Debt)	0.046***	-	0.077***
	(33.020)	-	(6.822)
$1/\sigma_E$	-0.002***	0.000	0.017***
	(-15.026)	(1.350)	(11.497)
Income /Assets	-0.062***	-0.036***	3.166***
	(-11.425)	(-4.213)	(45.386)
Excess Return	-0.063***	-0.006***	0.182***
	(-26.704)	(-3.065)	(21.468)
Constant	0.143***	1.214***	0.089
	(34.633)	(54.246)	(0.495)
Observations	103,424	103,383	100,442
R-squared	0.483	0.976	0.837
Company FE	YES	YES	YES
Year FE	YES	YES	YES

Furthermore, for each year in which one or multiple states adopt the CS law, I randomly designate an equal number of states as the pseudo-treatment group, while the remaining states serve as the control group. I save the coefficient estimates of CS laws and repeat this process for a total of 1000 times. Subsequently, I construct a histogram displaying the distribution of the coefficient estimates concerning CS laws when the dependent variable is the Expected Default Frequency (EDF). The results are presented in ***Graph 4.2*** below. The horizontal axis represents the coefficients of the regression result, and the vertical axis represents the corresponding P values. The results reveal that the coefficients obtained from the pseudo-regressions follow a normal distribution with a mean of 0. Furthermore, the corresponding P-values are predominantly greater than 0.1. Notably, the vertical dotted line in the figure represents the actual regression coefficient obtained from the main regression analysis presented in column (2) of ***Table 4.2.*** I find that the coefficient estimate of the true effect lies well to the left of the distribution of coefficient estimates from the placebo tests. These findings collectively suggest that the observed relationship between the adoption of CS laws and default risk is improbable to be spurious.

Graph 4.2. Distribution of coefficients of placebo test

This table reports the distribution of coefficients from OLS regressions of default risk proxied by EDF by Bharath and Shumway (2008) on the indicator for fictitious passage of control share acquisition law 1000 times. The placebo test is conducted by randomly assigning states passing the CS law, which ensures that each state has the same chance to adopt the CS law and thus guarantees that any difference between and within states is not systematic. The horizontal axis represents the coefficients of the regression result, and the vertical axis represents the corresponding P values. The vertical dotted line in the figure is the real regression coefficient obtained in the main regression presented above, shown in column (2) of Table 4.2.



4.8.6 Excluding the Dot-com Crisis and the Global Financial Crisis periods

In light of the fact that my sample period encompasses periods marked by the dot-com crisis and the global financial crisis, during which specific stresses and pressures have arisen, I undertake an analysis to examine the potential impact of these crisis periods on the relationship between the adoption of CS laws and the Expected Default Frequency (EDF). To achieve this, I re-estimate my primary regression model while excluding observations from the crisis periods, namely 2001-2002 and 2008-2009.

The results are presented in ***Table 4.11.*** Specifically, the estimated coefficient of CS laws in the regression without control variables is -0.01449, which is statistically significant at the 1% level. Similarly, in the regression with control variables, the estimated coefficient of CS laws is -0.01093, also significant at the 1% level. In addition to assessing statistical significance, I further explore the economic significance of the findings. On average, firms incorporated in states that have implemented CS laws experience an 18.2% reduction in default risk relative to the mean default risk during the sampled period.

Remarkably, even after excluding the crisis periods, the significantly negative relationship between the passage of CS laws and default risk persists. Consequently, my results remain consistent with my initial predictions, which posit that following the implementation of anti-takeover laws, companies subject to such influence experience worse corporate governance. Moreover, it supports my contention that managers are inclined to mitigate default risk by exercising their discretion to reduce the utilization of debt, in line with the trade-off theory of capital structure. This finding not only strengthens my baseline regression results but also eliminates alternative explanations that may cast doubt on the observed relationship.

For control variables, the coefficients are also consistent with my baseline regression and are as expected with Bharath and Shumway (2008) and Brogaard et al. (2017). Specifically, I find that $Ln(Equity)$ is significantly and negatively related to EDF at the 1% significance level, while $Ln(Debt)$is significantly and positively related to EDF at the 1% significance level. I also find that $1/_{\sigma_E}$, $Excess\ Return$ and $^{Income}/_{Asset}$ are significantly and negatively related to EDF at the 1% level.

Table 4.11. Effect of CS laws on corporate default risk excluding dot-com crisis and the global financial crisis periods and control lobbying companies

This table reports coefficients from firm-panel regressions of a firm's default risk proxied by EDF by Bharath and Shumway (2008) on an indicator for whether the firm's state of incorporation has adopted a control share acquisition law, firm fixed effects (FE), state-of-location-by-year FE, and standard industrial classification industry-by-year FE, excluding dot-com crisis and the global financial crisis periods as well as control lobbying companies The dependent variables are default risk. The sample includes firm-year observations from 1975 to 2007 excluding observations from the crisis periods, 2001-2002 and 2008-2009. I winsorize continuous variables at the 1st and 99th percentiles. I present the T-values in brackets. Standard errors are clustered at the firm and year levels. Variable definitions are provided in Appendix B. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Dependent Variable				
Independent Variables	(1) EDF	(2) EDF	(3) EDF	(4) EDF	(5) EDF
CS law	-0.013***	-0.010**	-0.010**	-0.011***	0.011*
	(-3.353)	(-2.666)	(-2.241)	(-2.948)	-1.812
PP law			-0.003		-0.002
			(-0.746)		(-0.385)
DD law			0.006		0.003
			(-1.518)		(-0.725)
FP law			-0.004		-0.007
			(-0.731)		(-1.266)
BC law			0.003		0.005
			(-0.941)		(-1.421)
AC law					-0.006
					(-1.373)
AG law					-0.000
					(-0.031)
CSCO law					-0.025***
					(-4.463)
Disg law					0.005
					(-1.08)
FG law					-0.002
					(-0.657)
GPR law					0.004
					(-0.284)
TPB law					0.005
					(-0.536)
CS * CTS					-0.026***
					(-3.656)
CS * lobbying				0.007	0.010
				(-0.368)	(-0.494)
Control variables		(-23.519)	(-23.506)	(-27.725)	(-27.703)
Constant	0.056***	0.133***	0.131***	0.147***	0.149***
	(-76.606)	(-21.543)	(-22.711)	(-33.530)	(-29.737)
Observations	92,485	92,485	92,485	103,922	103,922
R-squared	0.416	0.482	0.482	0.484	0.484
Company FE	YES	YES	YES	YES	YES
Industry-Year	YES	YES	YES	YES	YES
State-Year FE	YES	YES	YES	YES	YES

4.9. Conclusion

In this study, my objective is to comprehensively re-evaluate the causal relationship between takeover protection and default risk. To address concerns regarding endogeneity, I employ a difference-in-differences approach, introducing the adoption of Second-Generation State Antitakeover Laws as a proxy for takeover threats. my findings demonstrate a significant reduction in default risk, approximately 18.2% relative to the mean default risk during the sample period, for firms incorporated in states that have implemented control share acquisition (CS) laws. This empirical result substantiates my initial prediction that managers strategically decrease debt usage and subsequently reduce default risk in response to weakened external monitoring mechanisms following the adoption of CS laws (Fama and Jensen, 1983; Jensen and Ruback, 1983; Lel and Miller, 2015; Scharfstein, 1988; Garvey and Hanka, 1999). Importantly, I observe that the effect manifests one year after the passage of pay secrecy laws, mitigating concerns of reverse causality.

To illustrate that the decreased default risk following the implementation of CS laws is due to managerial opportunistic activities and to investigate the underlying mechanism of managers, I find an increase in agency costs of equity and observe that the reduction in default risk subsequent to the implementation of CS laws adversely affects shareholders. These findings support the argument that anti-takeover protection weakens the external disciplining mechanism of the takeover market, shielding managers from replacement and increasing the agency cost of equity, aligning with the "Managerial Entrenchment

Hypothesis," suggesting that anti-takeover provisions have adverse effects on stockholders' interests (Manne, 1965; Walkling and Long, 1984; Williamson, 1975).

Furthermore, I uncover a contradiction to the findings of Balachandran et al. (2022), as I demonstrate that decreased takeover threats are associated with a lower likelihood of default. Additionally, I find that the adoption of CS laws exacerbates managerial underinvestment. These findings indicate a preference for a "enjoy a quiet life" among executives, suggesting that the utilization of anti-takeover provisions prompts managers to prioritize personal interests, negatively impacting shareholders but benefiting debtholders (Bertrand and Mullainathan, 2003; Klock et al., 2005; Chava et al., 2009; Qiu and Yu, 2009; Gormley and Matsa, 2016). Furthermore, my study reveals that the effects of CS laws' adoption on default risk are more pronounced in firms with higher levels of institutional shareholding, particularly when such shareholding is long-term, supporting the notion that the decline in default risk for affected firms results from deteriorated corporate governance and heightened agency conflicts.

To strengthen the robustness of my findings, I perform various additional analyses, including alternative measures of takeover threats and default risk. Moreover, I assess parallel trend assumptions, employ Propensity Score Matching (PSM), conduct placebo tests, and utilize stacked difference-in-difference estimation, among other techniques. Encouragingly, all of these supplementary analyses consistently support my primary regression results.

The implications of my study extend to executives, shareholders, and creditors. For managers, the interests of shareholders and debtholders must be carefully considered when their objectives diverge. The presence of agency costs of debt arises when debtholders restrict the use of capital due to concerns that management may prioritize shareholders over creditors (Kim and Sorensen, 1986). Consequently, obtaining additional debt capital, especially for financially distressed companies, becomes more challenging. Therefore, managers must navigate the conflicts between shareholders and creditors and weigh the interests of both parties. For shareholders, my study highlights that the traditional agency conflicts arising from "private benefits" alone may not explain managers' opportunistic actions that deviate from shareholders' interests. Consideration of the "enjoy a quiet life" agency conflict is necessary. As for debtholders, my findings underscore the diverse effects of takeover protection on default risk, implying that debtholders may benefit from anti-takeover provisions.

Appendix A

Table I
First-Generation State Antitakeover Laws, 1968 to 1982

This table lists in chronological order the first-generation state antitakeover laws adopted by 38 states from 1968 through 1981, which were effective until a U.S. Supreme Court ruling in Edgar us. MITE Corp. on June 23, 1982. First-generation laws regulated cash tender offers and generally imposed extremely strong takeover protections for the covered corporations. The protections included requirements that the bidding firm files extensive disclosure statements with the state securities commissioner, provisions that required long and variable open periods for tender offers, state administrative overview of the tender offer, and potential civil and criminal liability for bidding firms and their managers for violations of the antitakeover provisions.

State	Effective Date	Repeal Date	State	Effective Date	Repeal Date
Virginia	03/05/1968	07/01/1989	Kentucky	07/01/1976	07/15/1986
Nevada	03/04/1969	10/01/1991	Maryland	07/01/1976	07/01/1986
Ohio	10/09/1969		Michigan	07/01/1976	04/01/1988
Wisconsin	07/01/1972		New York	11/01/1976	
Minnesota	08/01/1973		Georgia	03/23/1977	03/28/1986
Hawaii	05/24/1974	04/23/1985	Arkansas	03/24/1977	03/24/2000
Kansas	07/01/1974	04/21/1988	New Hampshire	03/25/1977	
Indiana	05/01/1975		Nebraska	04/27/1977	04/08/1988
Colorado	07/01/1975	07/01/1984	New Jersey	04/27/1977	
Idaho	07/01/1975	07/01/1986	Texas	05/06/1977	
South Dakota	07/01/1975	07/01/1990	North Carolina	06/28/1977	10/01/2001
Utah	02/05/1976	07/01/1983	Mississippi	07/01/1977	
Pennsylvania	03/03/1976		Illinois	09/08/1977	07/01/1984
Tennessee	03/17/1976		Florida	10/01/1977	09/01/1979
Delaware	05/01/1976	07/01/1987	Maine	03/24/1978	07/16/1986
Massachusetts	05/22/1976		South Carolina	06/12/1978	01/01/1989
Connecticut	06/02/1976		Missouri	08/13/1978	
Alaska	06/12/1976		Iowa	01/01/1979	01/01/2005
Louisiana	06/28/1976	08/15/1987	Oklahoma	07/21/1981	07/22/1985

Table II
Second-Generation State Antitakeover Laws, 1982 to 2013

This table lists the adoption dates for the five most common types of antitakeover laws adopted by states since 1982. The law types are control share acquisition laws (CS), business combination laws (BC), fair price laws (FP), directors' duties laws (DD), and poison pill laws (PP). A total of 43 states have adopted 153 of these laws, including 33 states that have business combination laws. The different types of laws are described in the Appendix. Dates in parentheses represent the effective date of the law if it is different from the adoption date.

State	CS	BC	FP	DD	PP
Arizona	07/22/1987	07/22/1987	07/22/1987	07/22/1987	
Colorado					03/31/1989
Connecticut		06/07/1988	06/04/1 984	06/07/1988	06/26/2003 (10/01/2003)
Delaware		02/02/1988 (12/23/1987)			
Florida	07/02/1987		07/02/1987	06/27/1989	06/27/1989 (07/01/1990)
Georgia		03/03/1988[a]	03/27/1988[a] (07/01/1985)	04/10/1989 (07/01/1989)	04/07/1988 (07/01/1989)
Hawaii	04/23/1985			06/07/1989	06/17/1988
Idaho	03/22/1988	03/22/1988	03/22/1988	03/22/1988	03/22/1988
Illinois		08/02/1989	08/23/1985	08/23/1985	08/02/1989
Indiana	03/05/1986 04/01/1986)	03/05/1986 (01/07/1986)	03/05/1986 (04/01/1986)	03/05/1986 (04/01/1986)	03/05/1986 (04/01/1986)
Iowa		05/02/1997 (07/01/1997)		06/01/1989 (12/31/1989)	06/01/1989 (12/31/1989)
Kansas	04/14/1988 04/21/1988)	04/10/1989 (07/01/1989)			
Kentucky		03/28/1986	04/09/1984 (07/13/1984)	07/15/1988	07/15/1988
Louisiana	06/11/1987		07/13/1984	07/10/1988	
Maine		04/06/1988		06/21/1985 (09/19/1985)	04/08/2002 (07/01/2003)
Maryland	04/11/1989	04/11/1989 (01/11/1989)	06/21/1983	05/13/1999 (06/01/1999)	05/13/1999 (06/01/1999)
Massachusetts	07/21/1987	07/18/1989		07/18/1989	07/18/1989
Michigan	3/19/1988[b] 04/01/1988)	05/24/1989	05/24/1984 (05/29/1984)		07/23/2001
Minnesota	04/25/1984 08/01/1984)	06/25/1987 (06/01/1987)	05/02/1991 (08/01/1991)	06/25/1987 (06/01/1987)	05/05/1995 (08/01/1995)
Mississippi	03/15/1990 01/01/1991)		03/29/1985 (07/01/1985)	04/04/1990 (07/01/1990)	04/20/2005
Missouri	06/13/1984	06/23/1986	06/23/1 986	05/06/1986	07/13/1999 (08/28/1999)
Nebraska	04/08/1988	04/08/1988		04/08/1988[c]	
Nevada	06/06/1987 07/01/1987)	06/25/1991 (10/01/1991)	06/25/1991 (10/01/1991)	06/25/1991 (10/01/1991)	06/21/1989 (10/01/1989)
New Jersey		08/05/1986 (01/23/1986)	08/05/1986 (01/23/1986)	02/04/1989	06/29/1989
New Mexico				04/09/1987	

(Continued)

State	CS	BC	FP	DD	PP
New York		12/16/1985	12/16/1985	07/23/1987	12/21/1988 (07/24/1986)
North Carolina	05/13/1987		04/23/1987	07/24/1993 (10/01/1993)	06/08/1989 (07/01/1990)
North Dakota				04/12/1993 (08/01/1993)	
Ohio	11/18/1982	04/11/1990	04/11/1990	07/11/1984 (10/10/1984)	11/22/1986
Oklahoma	06/24/1987	04/09/1991 (09/01/1991)			
Oregon	07/18/1987	04/04/1991		03/05/1989	03/05/1989
Pennsylvania	04/27/1990	03/23/1988	03/23/1988	04/27/1990	03/23/1988
Rhode Island		07/03/1990	07/03/1990	07/03/1990	07/03/1990
South Carolina	04/22/1988	04/22/1988	04/22/1988		06/09/1998
South Dakota	02/20/1990 (07/01/1990)	02/20/1990 (07/01/1990)	02/20/1990 07/01/1990)	02/20/1990 (07/01/1990)	02/20/1990 (07/01/1990)
Tennessee	03/11/1988[d]	03/11/1988	03/11/1988	03/11/1988	05/29/1989
Texas		05/28/1997 (09/01/1997)		05/29/2003 (01/01/2006)	05/29/2003 (01/01/2006)
Utah	05/29/1987				03/13/1989 (04/24/1989)
Vermont				04/16/1998	06/06/2008
Virginia	02/22/1989 (07/01/1989)	03/31/1988	03/24/1985 06/01/1985)	03/31/1988	04/02/1990
Washington		08/11/1987	05/13/1985 07/28/1985)		03/23/1998 (06/11/1998)
Wisconsin	04/18/1984[e] (04/24/1984)	09/17/1987 (09/10/1987)	04/18/1984 04/24/1984)	06/09/1987 (06/13/1987)	09/17/1987 (04/30/1972)
Wyoming	03/20/1990	03/11/1989		03/09/1990 (01/01/1990)	03/03/2009 (07/01/2009)

[a] The Georgia business combination and fair price laws are opt-in.
[b] The Michigan control share statute was repealed effective 01/06/2009.
[c] The Nebraska directors' duties statute was repealed effective 04/25/1995, but was later reenacted effective 03/07/2007.
[d] Tennessee control share statute is opt-in.
[e] The Wisconsin control share statute was repealed, and a new version reenacted, both effective 04/22/1986.

Appendix B

Variable	*Definition*
Dependent Variables	
DD	Distance-to-default, calculated following Merton (1974) and Bharath and Shumway (2008).
EDF	Expected default frequency, computed as N(-DD), where N(.) is the cumulative standard normal distribution function.
Hostile index	Takeover index following Cain et al., (2017)
Z-score	A dummy variable equals to 1 if the original Altman Z-Score falls in the bankruptcy level below 1.81, and 0 otherwise.
Leverage	Total debt (dltt+dlc) normalized by total assets (at)
Violation	Electronic covenant violation filings for all SEC-registered firms
ROA	Income before extraordinary items normalized (ib) by lagged total assets (at)
ROE	Net income (ni) divided by total shareholders' equity value (csho*prcc_f)
Institutional shareholding	Ratio of shares outstanding held by institutional investors to the total number of shares outstanding
Long term	Long-term institutional shareholding for each company
Agency costs	Operating expenses (xopr) divided by revenues (ni)
Investment	Investment level model following Richardson (2006)
Independent Variable	
CS law	A dummy indicator that equals one if a control share acquisition law has been passed by the year in the state of incorporation of the company, zero otherwise.
PP law	A dummy indicator that equals one if a poison pill law has been passed by the year in the state of incorporation of the company, zero otherwise.
FP law	A dummy indicator that equals one if a fair price law has been passed by the year in the state of incorporation of the company, zero otherwise.
DD law	A dummy indicator that equals one if a directors' duties law has been passed by the year in the state of incorporation of the company, zero otherwise.
BC law	A dummy indicator that equals one if a business combination law has been passed by the year in the state of incorporation of the company, zero otherwise.
Control Variables	
Equity	Market value of equity (in millions of dollars) calculated as the product of the number of shares outstanding and stock price at the end of the year.
Debt	Face value of debt, in millions of dollars, computed as the sum of debt in current liabilities (Compustat quarterly data #45) and one-half of long-term debt (Compustat quarterly data #51).

$1/\sigma_E$	The inverse of the annualized stock return volatility. Annualized stock return volatility computed as the standard deviation of stock monthly returns over the prior year.
Income /Assets	Ratio of net income (Compustat quarterly data #69) to total asset (Compustat quarterly data #44).
Excess Return	Annual excess return, calculated as the difference between firm stock return and market return over the same period.

Chapter 5. CONCLUSION

This thesis addresses three key research inquiries within the domain of corporate finance, employing the Difference-in-Differences methodology as a means to address potential endogeneity concerns. The first essay of this thesis endeavors to establish a causal association between pay transparency and employee productivity, drawing upon an extensive and comprehensive dataset of U.S. publicly listed companies spanning the period from 1977 to 2019. However, establishing a causal link between pay transparency and corporate outcomes poses empirical challenges, even when compensation data is available (Heckman, 1998), given the inherent difficulty in isolating exogenous variations in the compensation of the pertinent peer group (Gao et al., 2021). Hence, in this investigation, I introduce Pay Secrecy Laws that advocate for pay transparency and prohibit organizations from implementing pay secrecy policies and practices (Kim, 2013, 2015) to effectively employ the difference-in-differences framework. To bolster precision and mitigate confounding variables, my study introduces an innovative proxy measure for employee productivity. This measure is derived by dividing EBITDA, while excluding incomes and incorporating expenses unrelated to employees or primarily influenced by managerial factors, by the total number of employees.

This research findings present a novel perspective by demonstrating that heightened levels of pay transparency exhibit an inverse relationship with employee productivity, even when accounting for high-dimensional fixed effects. On average, companies headquartered in states where such legislation has been adopted encounter a decline in employee productivity by 1.58% compared to the mean value within my sample. This supports the proposition that wage comparisons may engender reduced job satisfaction. It is noteworthy that these laws were not primarily designed to impact employee productivity, thus suggesting that this effect is likely an unintended consequence, rendering my utilization of these laws particularly valuable. Furthermore, my analysis reveals that the diminishing effect on productivity becomes discernible two years subsequent to the implementation of pay secrecy laws, providing additional evidence to counter concerns regarding reverse causality.

The potential endogeneity concerns surrounding the causal effects of pay secrecy laws' adoption on employee productivity necessitate the implementation of various diagnostic and robustness tests to alleviate alternative explanations. My analysis reveals that the pre-treatment trends in employee productivity exhibit no discernible differences between the treatment and control groups, thereby supporting the validity of the parallel trend assumption inherent in the difference-in-differences approach. Additionally, a placebo test is conducted by randomly assigning states to adopt pay secrecy laws, effectively excluding chance-driven outcomes as a potential driver. To mitigate potential self-selection bias stemming from firm-related characteristics that could influence my results, a Propensity Score Matching (PSM) test is also employed. For each year, treatment firms are matched with control firms based on firm characteristics utilized as control variables in my baseline regression model. The results, after accounting for sample selection bias through the PSM method, align with my baseline findings, confirming that my results are not influenced by systematic disparities between firms with varying levels of pay transparency.

Furthermore, stacked difference-in-differences estimates are employed as a robustness check to mitigate the impact of heterogeneous treatment effects and prevent potential negative weights associated with specific treatments. Consistent findings are obtained, affirming that my difference-in-differences estimates remain stable regardless of heterogeneous treatment effects. These supplementary analyses consistently reinforce the causal interpretation of my primary findings, indicating that the adoption of pay secrecy laws detrimentally affects firm-level employee productivity. Moreover, alternative measures of employee productivity, control for other state-level laws, and an extended sample period are incorporated to strengthen the robustness of my conclusions.

Finally, my findings indicate that the ramifications of pay secrecy laws are amplified in firms located in states characterized by lower levels of social capital, thereby bolstering my contention that the influence of pay secrecy laws on employee productivity is associated with the curbing of pay secrecy practices and regulations aimed at addressing gender pay disparities. Furthermore, I note a subsequent reduction in average employee salaries following the implementation of pay secrecy laws. This observation provides insight into the underlying mechanism contributing to the diminished productivity observed among employees in companies situated in states with pay secrecy laws.

This essay carries significant policy implications. It offers valuable insights into the imperative for employers to explore the tangible consequences of extensively adopted pay secrecy policies and practices. While these measures strive to curtail wage comparisons and mitigate employee discontent within organizations, they have also been implicated in perpetuating instances of pay discrimination (Kim, 2013, 2015; Cullen and Perez-Truglia, 2018; Baker et al., 2019). This also prompts policymakers to meticulously deliberate on the unintended consequences of pay secrecy laws, which can unintentionally impact employee productivity. In essence, an equilibrium appears to emerge between gender and race equality and employee satisfaction.

Moreover, this study highlights how important it is for employees to understand the details of their salaries. Many important factors, beyond gender and race, make a significant difference. This makes it harder for employees to easily compare their pay with that of their colleagues and figure out the fairness (Colella et al., 2007; Gely and Bierman, 2003).

The second essay within this thesis endeavors to examine the influence of executive mobility on institutional ownership, drawing upon an extensive and comprehensive dataset of U.S. publicly listed firms spanning the period from 1989 to 2018. The study introduces the staggered recognition of the Inevitable Disclosure Doctrine (IDD), which imposes stringent restrictions on managerial mobility in order to safeguard trade secrets, utilizing the difference-in-differences framework to establish a causal relationship. The recognition of the IDD engenders a plausibly exogenous reduction in executive mobility by enhancing the firm's capacity to prevent employees possessing knowledge of its trade secrets from joining competitors or establishing new enterprises.

This study presents pioneering empirical evidence regarding the influence of restricted mobility on the equity holdings of institutional investors. Notably, companies headquartered in states that acknowledge the IDD encounter an average decline of 2% in institutional share ownership, relative to the mean of the sample period, even after considering high-dimensional fixed effects. These findings underscore the motivations of institutional investors to target portfolio companies, aiming to mitigate monitoring costs and fulfill their fiduciary responsibilities. They also suggest a propensity among institutional investors to divest shares instead of using "voice" when they express dissatisfaction with executive management. Significantly, my observations indicate that the diminishing effect manifests two years subsequent to the enactment of the IDD, thereby alleviating concerns pertaining to reverse causality.

In addition, the study conducts a detailed investigation into the impact on different categories of institutional investors. By employing the classification system introduced by Bushee (1998) based on expected investment horizon, the institutional investors are categorized as "long-term" or "short-term" institutions. And the fiduciary standard outlined by Bushee (2001) is also taken into account, distinguishing between banks, insurance companies, investment advisers (including mutual fund companies), and pensions and endowments. Within these categories, activist investors are identified based on their active involvement in buying or selling shares to influence managerial decisions, as opposed to passive institutions that primarily rely on "voice" mechanisms due to their alignment with benchmark portfolio weights. The study finds that among various classifications of institutional investors, activist and long-term institutions exhibit sensitivity to executive mobility constraints, thereby reinforcing their incentives for monitoring and being influenced more by corporate governance quality.

We further advance the understanding of the causal relationship between executive mobility and institutional ownership, elucidating the role of opportunistic behavior exhibited by managers in response to career concerns arising from mobility restrictions, as indicated by agency costs. In addition, I present additional empirical evidence that bolsters the proposition linking the impact of the IDD to institutional shareholding, specifically highlighting the association with trade secret protection. My findings reveal a more pronounced effect of the IDD on institutional shareholding in companies headquartered in states with greater knowledge-focused investments.

To enhance the credibility of my empirical findings, I conduct a range of diagnostic and robustness tests, including stacked difference-in-differences estimation, placebo tests, propensity score matching (PSM) tests, controlling for Non-Compete Agreements (NCA), utilizing alternative IDD adoption dates, and accounting for the rejection of IDD. Importantly, all of these tests consistently support my main regression results, providing further substantiation for the argument that institutional investors divest their shares when they express dissatisfaction with management quality.

This study stimulates employers to contemplate strategies that uphold robust corporate governance practices, as the adoption of IDD aims to protect trade secrets while unintentionally harming corporate governance. There tends to be a balance between the protection of trade secrets through limiting executive mobility and corporate governance.

Moreover, it is important for companies to consider the targeting mechanism of institutional investors due to their substantial equity positions, allowing them to conduct a monitoring role. Furthermore, I emphasize the necessity for further investigation into specific classifications of institutional investors. As illustrated above, different classifications of institutional investors exhibit varying levels of engagement in the corporate governance of the companies in which they hold shares.

The third essay within this thesis seeks to conduct a thorough reassessment of the causal association between takeover protection and default risk, utilizing a comprehensive dataset of U.S. listed firms spanning the period from 1996 to 2008. To mitigate potential endogeneity concerns, I introduce the adoption of Second-Generation State Antitakeover Laws, effectively isolating companies from hostile takeover threats, to employ a difference-in-differences methodology.

Our findings reveal a significant decrease in default risk, amounting to approximately 18.2% relative to the mean default risk observed during the sample period, among firms incorporated in states that have implemented CS laws. This empirical outcome provides substantial support for my initial hypothesis, suggesting that managers strategically adjust their debt utilization and consequently mitigate default risk in response to weakened external monitoring mechanisms following the enactment of anti-takeover laws (Fama and Jensen, 1983; Jensen and Ruback, 1983; Lel and Miller, 2015; Scharfstein, 1988; Garvey and Hanka, 1999). Notably, I observe that this effect becomes apparent one year subsequent to the implementation of pay secrecy laws, effectively mitigating concerns regarding potential reverse causality.

We further elucidate that the underlying mechanisms driving the reduced default risk observed following the implementation of control share acquisition (CS) laws are due to managerial opportunistic activities. I find a notable increase in the agency costs of equity, and the reduction in default risk subsequent to the enactment of CS laws adversely affects shareholders. These findings provide empirical support for the proposition that anti-takeover protections weaken the external disciplining mechanism of the takeover market, thus shielding managers from potential replacement and exacerbating the agency costs of equity. This aligns with the well-established "Managerial Entrenchment Hypothesis," which posits that anti-takeover provisions have detrimental effects on the interests of stockholders (Manne, 1965; Walkling and Long, 1984; Williamson, 1975).

Our study challenges the findings of Balachandran et al. (2022) by revealing a contradiction: I establish that decreased takeover threats are linked to a lower likelihood of default. Furthermore, I discover that the adoption of CS laws amplifies managerial underinvestment. These findings indicate a preference among executives for a "quiet life," where the utilization of anti-takeover provisions leads managers to prioritize personal interests. This prioritization negatively impacts shareholders but benefits debtholders, as supported by previous studies (Bertrand and Mullainathan, 2003; Klock et al., 2005; Chava et al., 2009; Qiu and Yu, 2009; Gormley and Matsa, 2016).

Moreover, my research unveils that the effects of CS laws' adoption on default risk are more pronounced in firms with higher levels of institutional shareholding, particularly when such shareholding is long-term. This finding supports the notion that the decline in default risk for affected firms stems from deteriorated corporate governance and heightened agency conflicts.

To ensure the robustness of my findings, I conduct various additional analyses. These include using alternative measures of takeover threats and default risk, assessing parallel trend assumptions, employing Propensity Score Matching (PSM), conducting placebo tests, and utilizing stacked difference-in-difference estimation, among other techniques. Importantly, all of these supplementary analyses consistently support my baseline regression results, further strengthening the validity and reliability of my findings.

This essay expands the scope of my investigation to shed light on the effects of CS laws, an area that has received limited attention thus far. My research unveils that, in addition to business combination (BC) laws, CS laws significantly influence corporate governance dynamics by providing robust anti-takeover protection. Furthermore, I uncover that CS laws exert a negative impact on default risk, revealing their noteworthy implications in shaping the financial stability of firms.

This study carries significant implications for executives, shareholders, and creditors alike. Executives are advised to carefully consider the interests of both shareholders and debtholders when their objectives diverge. The existence of agency costs of debt arises from concerns that management may prioritize shareholders over creditors, leading debtholders to limit the use of capital (Kim and Sorensen, 1986). Consequently, accessing additional debt capital, particularly for financially distressed companies, becomes more challenging. Thus, managers must navigate the conflicts between shareholders and creditors and carefully weigh the interests of both parties.

For shareholders, this study highlights that traditional agency conflicts stemming solely from "private benefits" may not fully explain managers' opportunistic actions that deviate from shareholders' interests. It is crucial to consider the presence of the "enjoy a quiet life" agency conflict. In the case of debtholders, my findings emphasize the diverse effects of takeover protection on default risk, suggesting that anti-takeover provisions may benefit debtholders. This underscores the importance of considering the implications of takeover protection measures for creditors.

Throughout the three essays, I introduce the staggered adoption of Pay Secrecy Laws, the Inevitable Disclosure Doctrine, and Anti-takeover Laws to address endogeneity issues. It is crucial to note that multiple exogenous shocks impact different states and firms at various time points. To account for the presence of staggered treatments, I employ a difference-in-differences estimation, which allows for a comparison of the pre- and post-effects of legislation on the states subject to the treatment (referred to as the treatment group) versus those states that were not affected by such changes (referred to as the control group). Through the implementation of this methodology, I effectively eliminate the potential for reverse causality between the adoption of legislation and changes in corporate outcomes. In contrast, situations involving a single shock face a common identification challenge, as incidental noise coincides with the shock itself, exerting a direct influence on the dependent variable (Roberts and Whited, 2013).

We acknowledge the potential econometric challenges associated with staggered difference-in-differences (DiD) analysis. Notably, Cengiz et al. (2019) have highlighted the econometric concerns arising from aggregating discrete DiD estimates using ordinary least squares (OLS), such as heterogeneous treatment effects and potential negative weights assigned to specific treatments. To ensure a more precise examination of the relationship, I adopt stacked difference-in-differences (DID) estimates as a robustness check.

The stacked DID method serves to transform the staggered adoption setting into a two-group, two-period design. Through this transformation, the difference in differences estimates the average treatment effect on the treated, considering both the relative sizes of the group-specific datasets and the variance of treatment status within those datasets. Furthermore, in order to mitigate endogeneity concerns and facilitate a rigorous causal inquiry while accounting for the existence of staggered treatments, the methodology of Difference-in-Differences (DID) analysis continues to evolve. It necessitates the development of updated techniques to enable more comprehensive and causal investigations.

We acknowledge the assumption of exogeneity between the examined legislation and corporate outcomes in order to establish a causal interpretation of my analysis. Although I cannot assert complete exogeneity of such legislation or entirely eliminate all potential endogeneity concerns, I contend that the political factors underlying these laws, my explanation of firms' and managers' resistance to such legislation, and my comprehensive empirical analyses (including additional checks for staggered difference-in-differences, dynamic effect analysis, placebo test, propensity score matching test, analysis of heterogeneous treatment effects, and all mechanism tests) collectively contribute to mitigating endogeneity concerns to a certain degree.

Moreover, our study acknowledges several limitations pertaining to the measurement of variables of interest. Notably, concerns may arise regarding the relative immunity of our novel measure of employee productivity to earnings management. This concern arises from the method employed to measure employee productivity, which involves dividing employee output by employee input. Since employee output is closely tied to managerial earnings management, there exists a trade-off between adopting a broader-based measure of productivity and the potential measurement error associated with our chosen approach.

Another area of concern is related to the classification of institutional investors. Following the method established by Bushee (1998) and Bushee (2001), we classify institutional investors based on expected investment horizon and fiduciary standard to gauge their motivation to engage in corporate governance of the firms they hold shares in. However, it should be noted that alternative methods of classifying institutional investors exist, leading to varying perspectives on their governance motives.

Finally, our selection of the adoption of Control Share Acquisition (CS) Laws over other anti-takeover laws may raise some concerns. Although we have illustrated the potency of CS laws, drawing from evidence presented by Karpoff and Wittry (2018) on the stringency of control share acquisition regulations, previous literature such as Karpoff and Wittry (2018) contests the notion that Business Combination laws (BC laws) provide the most stringent protection. This debate leaves the optimal choice of anti-takeover law for safeguarding against unsolicited takeovers still an open question.

We acknowledge the assumption of exogeneity between the examined legislation and corporate outcomes in order to establish a causal interpretation of my analysis. Although I cannot assert complete exogeneity of such legislation or entirely eliminate all potential endogeneity concerns, I contend that the political factors underlying these laws, my explanation of firms' and managers' resistance to such legislation, and my comprehensive empirical analyses (including additional checks for staggered difference-in-differences, dynamic effect analysis, placebo test, propensity score matching test, analysis of heterogeneous treatment effects, and all mechanism tests) collectively contribute to mitigating endogeneity concerns to a certain degree.

This thesis carries significant policy implications. The three essays presented herein introduce state-level doctrines or laws and employ the difference-in-differences methodology to address endogeneity concerns. However, the effects of these doctrines or laws do not directly impact my dependent variables and are likely unintended consequences. For example, pay secrecy laws prevent pay secrecy policies and practices that strive to curtail wage comparisons and mitigate employee discontent within organizations, while pay secrecy policies and practices have also been implicated in perpetuating instances of pay discrimination (Kim, 2013, 2015; Cullen and Perez-Truglia, 2018; Baker et al., 2019). The Inevitable Disclosure Doctrine (IDD) protects trade secrets while restricting executive mobility and leads to worse corporate governance (Li et al., 2017; Kim et al., 2020; Ali and Li, 2019; Li et al., 2018; Gao et al., 2018; Islam et al., 2020; Na, 2020). Anti-takeover protection laws offer the best defense against unsolicited takeovers while being believed to weaken this disciplinary mechanism by shielding managers from replacement and offering long-term contracts that mitigate career concerns (Fama and Jensen, 1983; Jensen and Ruback, 1983; Scharfstein, 1988; Lel and Miller, 2015).

These circumstances provide ample motivation for regulators to study the economic implications of these laws. As of now, nine states have adopted pay secrecy laws, and 15 out of 50 US states have embraced the Inevitable Disclosure Doctrine aiming to protect trade secrets by limiting executive mobility. Similarly, to date, 43 states have adopted 157 second-generation anti-takeover laws, encompassing business combination, control share acquisition, fair price, poison pill, and directors' duties (constituency) laws, following the MITE decision and subsequent adoption of the first control share acquisition law by Ohio in 1982. The remaining states are still considering whether to follow suit, partly due to limited understanding of the legislation’s economic effects.

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