How do firms respond to reduced private equity buyout activity?*

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Abstract

Exploiting the state-by-state adoption of laws that increase the cost of undertaking private equity (PE) buyouts, I use a difference-in-differences approach to study how firms respond to reduced PE buyout activity. I find that the firms raise less capital, reduce payouts and investments, and cherry-pick positive NPV projects with low risk, indicative of managers enjoying the quiet life. Yet, despite investing less, the firms hire more employees, consistent with managers forming alliances with employees. Further analyses show that the firms also become less likely to default on their debt or go bankrupt, consistent with lower risk-taking by quiet-life managers.

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1. Introduction

The market for corporate control is one of the most controversial issues in corporate governance. Theories offer diverging views on how the threat of a takeover affects managerial incentives in target firms. On one hand, takeovers may threaten managers' control rights if they perform poorly, prompting them to act in the interests of their shareholders (e.g., Manne, 1965; Jensen and Ruback, 1983). Hence, takeover threats function as governance mechanisms that discipline managers into making value-enhancing decisions for their firms. On the other hand, takeover threats may induce managers to sacrifice the long-term value of their firms for short-term profits, to prevent their firms from being acquired at an undervalued price (e.g., Stein, 1988). In this regard, takeover threats adversely affect managerial incentives and are detrimental to firm value.

Several factors may characterize a given takeover, such as the method of payment, the motive behind the takeover, and the ownership structure of the acquirer. Yet previous empirical investigations of the impact of takeover threats have not distinguished between these different types of takeovers. Further, the evidence is mixed as to whether and how the target firms' managers are entrenched when takeovers are less likely. For instance, Bertrand and Mullainathan (2003) provide evidence that firms with reduced takeover threats invest less, and that they are less profitable and productive, consistent with the firms' managers exerting less effort and enjoying a quiet life. By contrast, Frattaroli (2020) finds no evidence that managers altered their firms' policies to serve their private interests in response to reduced takeover threats. However, the firms' board of directors responded to the weakening shareholder governance by increasing the pay-for-performance sensitivity for CEOs, in order to align managers' interests with those of shareholders.

In this paper, I provide new evidence on how the market for corporate control affects managerial behavior by examining how firms respond to reduced private equity (hereafter, PE) buyout activity. I exploit the staggered adoption of the constructive fraud provision (hereafter,

CFL) in fraudulent transfer laws by U.S. state courts as a shock to PE buyout activity. The CFL provides the original unsecured creditors of PE buyout targets the right to legally challenge the buyouts as fraudulent if the targets default on their debts. A successful lawsuit can unwind a buyout and force repayment to the targets' original creditors. The unwinding of buyouts could result in the targets' selling shareholders having to return the sale proceeds, as well as the lending banks for the buyout loans losing their lien on the targets' assets. Considering the risk of buyouts being unwound, the targets' selling shareholders may demand a higher takeover premium. At the same time, the lending banks may require a higher interest rate for making the buyout loan. As it becomes more costly to undertake PE buyouts, PE buyout activity is predicted to decline.

There are several advantages to using the passage of the CFL as a source of variation in PE buyout activity. First, the CFL applies to a wide range of transactions, seeking to prevent debtors from defrauding their creditors. Due to the law's broad coverage, it seems unlikely that states adopted the CFL to restrict PE buyout activity specifically, and therefore the impact of the CFL on PE buyouts is arguably unintentional. Second, as opposed to hostile takeovers, managers often respond positively to PE buyouts—they did so in 99% of the cases in the sample of PE buyouts examined in this paper. Thus, firms are unlikely to have lobbied for the passage of the law to thwart buyouts, alleviating the concern that the law's adoption could be endogenous to firm-specific conditions.

The first part of my analysis investigates how the CFL impacts PE buyout activity. I begin by documenting the economic mechanisms through which the CFL leads to a reduction in PE buyouts. With the adoption of the CFL, the takeover premiums for PE buyouts and the interest rates on PE buyout loans are predicted to increase, which in turn will reduce PE buyout activity. Since data on takeover premiums is only available for a small sample of PE buyouts, I focus on changes in the spreads on PE buyout loans. I find that the yield spread of PE buyout loans increases following the adoption of the law. In particular, the loan spreads increase by 55 basis

points on average, representing a 28% increase relative to the sample mean. By contrast, the spreads for all other loans—i.e., non-PE buyout loans—remain unchanged.

Next, I test the prediction that PE buyout activity declines following the adoption of the CFL. I find that states that adopt the law experience a substantial reduction in PE buyout activity. Specifically, the number of PE buyout deals decreases by 43% relative to the sample mean. Consistent with this result, I also find that firms are less likely to become PE buyout targets following the law change. Further analysis reveals that PE firms whose investments are more likely to be affected by the law invest more in non-adopting states, compared to PE firms whose investments are less likely to be affected. This implies that the large magnitude of the law's estimated impact on PE buyout activity is partly driven by PE firms reallocating their investments from law-adopting states, where buyout costs are higher, to non-adopting states with lower costs. In cross-sectional analysis, I explore whether financial and strategic acquirers respond differently to the law adoption. Because the CFL makes it more costly to expropriate wealth from the targets' original creditors, financial acquirers may find buyouts less appealing—unlike strategic acquirers, they are unable to generate operational synergies with the target firms. Indeed, I find that the reduction in PE buyout activity is concentrated within buyouts by financial acquirers. Finally, I show that these results are unlikely to be driven by pre-treatment trends. In particular, there is no evidence that the law had any effect in the years before its adoption.

The second part of my analysis investigates whether and how firms respond to the reduced likelihood of being targeted in PE buyouts. Following PE buyouts, managers of target firms are often provided with additional equity ownership, to realign their incentives with those of shareholders. However, managers are also at risk of losing their jobs post-buyout, should they perform poorly. The likelihood of being acquired by PE firms thus reduces managerial entrenchment since an increase in equity ownership and the threat of job loss both improve managerial incentives. I find that firm valuation, as measured by Tobin's Q, declines following

the law's adoption. In addition, the cumulative abnormal returns around the law's effective dates are negative, on average. The fact that the law has a negative impact on shareholder value supports the prediction that managers become more entrenched when their firms are less likely to become PE buyout targets.

There are a number of ways entrenched managers could pursue their private interests. Among the more well-known examples include engaging in empire-building behavior to increase their power and compensation (e.g., Baumol, 1959; Marris, 1964; Williamson, 1964; Jensen, 1986), exerting less effort to enjoy a quiet life (e.g., Grossman and Hart, 1983; Bertrand and Mullainathan, 2003), and taking their careers into consideration when making decisions (e.g., Holmström, 1999). To understand the behavior of entrenched managers, I examine the impact of the CFL on a range of firm outcomes, including measures of performance, growth, and risk, as well as its impact on firm policies. The main findings are as follows. First, firms raise less capital, reduce payouts to shareholders, and cut down on capital expenditures, consistent with the firms' managers slacking off. Second, while firms become more profitable and operational risks decrease, firm growth appears to decline. This suggests that managers cherry-pick positive NPV projects with low risk, which could be the result of managers exerting less effort or of managers reducing firm risk and improving firm profitability due to career concerns. Third, employment expenses increase as firms hire more employees. While this could reflect managers' empire-building behavior, it seems to be more consistent with managers forming alliances with employees for job security, considering that their firms are making less investments. The findings therefore suggest that, even when managers enjoy a quiet life, career concerns also appear to affect their decision-making. Finally, I find that firms are less likely to default on their debts or file for bankruptcy, consistent with the view that managerial entrenchment reduces risk-shifting from shareholders to debtholders (e.g., Francis et al., 2010).

I examine three potential alternative explanations for these trends, but find suggestive evidence that they are unlikely to drive the results. First, because the CFL provides increased

creditor rights for unsecured creditors, it may have resulted in firms using less unsecured debt. In this regard, it is possible that firms reduced their payouts and investments because of increased financial constraints. This is unlikely, however, since the sample of firms examined are public firms, which do not depend heavily on unsecured debt as a major source of funding. Further, I find no evidence that firms' use of unsecured debt has changed. Second, since PE buyouts can be a means for firms to obtain capital (e.g., Boucly et al., 2011), the reduced likelihood of firms becoming PE buyout targets could potentially explain their reduction in payouts and investments. However, public firms are less likely to experience financial constraints absent a buyout, because they have various ways to access public capital markets. Third, a potential concern is that the adoption of the CFL could be endogenous to state-specific economic conditions. To alleviate this concern, I examine the dynamics of various state-level macroeconomic factors prior to the adoption of the law. I find no evidence that the laws were adopted in response to changes in economic conditions.

This paper contributes in two ways to the literature on the corporate governance role of takeovers. First, while prior research has examined how the threat of takeovers affects the target firms' behavior by exploiting the passage of anti-takeover provisions (e.g., Bertrand and Mullainathan, 2003; Low, 2009; Gormley and Matsa, 2016; Frattaroli, 2020), the evidence is mixed. Instead of exploiting provisions intended to thwart takeovers, this paper provides new evidence by exploiting the passage of a law that was not designed to restrict buyouts, thereby eliminating the potential concern that firms may have lobbied for the law's passage. Second, contrary to the literature that examines takeover activity in general, this paper focuses on firms' responses to reduced PE buyout activity. The findings in this paper differ from those in the literature, suggesting that firms may respond differently to changes in their likelihood of becoming a takeover target based on the characteristics of the takeovers in question.[1]

[1]For instance, Bertrand and Mullainathan (2003) find that firms with reduced takeover threats become less profitable and pay their employees higher wages, whereas I find evidence of higher ROA and no evidence of wage increases. Low (2009) and Gormley and Matsa (2016) find a reduction in firms' overall risk, while I only find a reduction in firms' operational risk. Frattaroli (2020) finds no evidence of changes in managerial behavior.

This paper also contributes to the literature on the real effects of PE buyouts. Several papers examine the effects of PE buyouts on target firms (e.g., Kaplan, 1989; Lichtenberg and Siegel, 1990; Smith, 1990; Boucly et al., 2011; Guo et al., 2011; Lerner et al., 2011; Bernstein and Sheen, 2016) and their stakeholders (e.g., Davis et al., 2014; Agrawal and Tambe, 2016; Cohn et al., 2021) following the buyouts. In contrast, I investigate how managerial behavior changes as the probability of the manager's firm becoming a PE buyout target declines.

Finally, this paper is related to earlier work by Ersahin et al. (2020), which also examines the impact of the CFL. In particular, they show that the CFL strengthens unsecured creditors' rights and leads to entrepreneurs using less unsecured debt. The adoption of the CFL thus increases entrepreneurs' financial constraints, resulting in a reduction in entrepreneurial activity. In contrast, my paper examines public firms and documents that the adoption of the CFL reduces firms' likelihood of becoming PE buyout targets. Further, my findings suggest that creditor rights may affect managerial decisions differently depending on the ownership structure of the firm. In particular, public firms differ from small private firms in that the managers' personal assets are not tied to the firms. It is therefore less likely that public firm managers fear losing their personal assets to litigation under the CFL, which may explain why public firms do not adjust their use of unsecured debt after the law change.

2. Institutional background

2.1. Fraudulent transfer law: the constructive fraud provision

In 1571, the British Parliament passed the Statute of 13 Elizabeth, prohibiting debtors from making transactions with the intent to "delay, hinder, or defraud creditors."[2] However, creditors were responsible for proving debtors' fraudulent intentions. To ease the burden of proof on creditors, the English courts developed a set of factors known as "badges of fraud," which

[2]See the *Statute of 13 Elizabeth*, also known as *The Fraudulent Conveyance Act 1571*.

served as presumptive evidence of fraudulent intent. *Twyne's Case* was a leading case that formed the basis of these badges of fraud. The case involved an English farmer, Mr. Pierce, who attempted to defraud his creditors by selling his sheep to Mr. Twyne while remaining in possession of the sheep. Several factors identified from the case were labeled as badges of fraud, including for instance, "the debtor's continued possession of the property" and "the transfer made in trust for the benefit of the debtor."[3]

The Statute of 13 Elizabeth, along with the badges of fraud, were adopted by the U.S. system of fraudulent transfer law. With time, however, the weighting of the badges of fraud and conditions for determining fraudulent transactions varied among jurisdictions. To ensure consistency and predictability, the National Conference of Commissioners on Uniform State Laws (NCCUSL, also known as the Uniform Law Commission) developed the Uniform Fraudulent Conveyance Act (UFCA) in 1918. Most significantly, the UFCA developed various objective criteria for proving constructive fraud. These criteria are known as the constructive fraud provision (i.e., CFL). Under the CFL, creditors who are unable to receive payment from their debtors may challenge their debtors' transactions as fraudulent if the following conditions are met, regardless of whether their debtors intended to defraud: (1) the debtors become insolvent following these transactions, and (2) the debtors receive less than fair value in exchange. A successful lawsuit against the debtors would result in the debtors' transactions being unwound and the creditors being repaid. Prior to the CFL, creditors who were unable to get paid by their debtors had only the option of challenging their debtors' transactions under the actual fraud provision, which required them to prove the debtors' fraudulent intent.

Over the following decades, the NCCUSL made two amendments to the UFCA, and the act was renamed the Uniform Fraudulent Transfer Act (UFTA) and subsequently, the Uniform Voidable Transactions Act (UVTA). The main purpose of both amendments was to reduce ambiguity regarding the courts' interpretation of the law. Both new acts retain the structure

[3]For details on *Twyne's Case* and the badges of fraud, see *76 Eng. Rep. 809 (Star Chamber 1601).*

and organization of the UFCA, and the substance of the law remained largely unchanged.

2.2. PE buyout as a fraudulent transfer: the Gleneagles Case

The CFL provides creditors with the right to challenge their debtors' transactions as fraudulent if the debtors receive less than fair value in return for the transfers and if they fail to repay their creditors afterwards. Creditors have thus filed lawsuits to challenge many different insolvency-related asset transfers under the CFL, including PE buyouts that preceded the target firms' bankruptcy. A prominent case where a PE buyout was deemed fraudulent is the *Gleneagles Case*.[4] In 1973, Raymond Colliery Co. (hereafter, Raymond) was acquired by an investor group through Great American Coal Co., a shell company. The purchase of Raymond's stock was financed by a loan issued by the Institutional Investors Trust, in which the assets of Raymond were pledged as collateral for the loan. Shortly after the buyout, Raymond struggled to make its tax and loan payments. In 1980, the federal government filed a lawsuit against Raymond for failing to pay its taxes. Due to Raymond's insolvency following the buyout, the court declared that the transactions constituted constructive and actual fraud under the Pennsylvania UFCA. Specifically, the loan proceeds merely flowed through Raymond to its selling shareholders and thus were not considered as receiving fair value in exchange for Raymond. In addition, the selling shareholders were accused of breaching their fiduciary duty, in the sense that they were aware the transaction would injure Raymond and its original creditors. Having thus determined that the buyout was a fraudulent conveyance, the deal was unwound.

[4]For details on the *Gleneagles Case*, see *565 F. Supp. 556 (M.D. Pa. 1983)*.

3. Predictions

3.1. PE buyout activity

PE buyouts refer to acquisitions in which PE firms acquire target firms using a large amount of debt secured by the targets' assets. Typically, a PE buyout works as follows. First, the PE firm contributes a small amount of equity with its own capital and forms a shell company. The PE firm then uses the shell company to obtain an unsecured loan and purchases the target from the selling shareholders. The shell company is then merged into the target, with the target surviving. Following the merger, the target's assets are used as collateral for the buyout loan and the target's future cash flows are used to service the loan.

Following a PE buyout, the target may be more at risk of going bankrupt due to its highly leveraged capital structure.[5] In the event that the target goes bankrupt, its original unsecured creditors may receive little or no payment since their claims are not backed by collateral. Under the CFL, however, the target's original creditors may file a lawsuit and challenge the buyout deal as fraudulent if the target defaults on its debt obligations to them. This is for two reasons. First, the debt proceeds raised to finance the buyout deal went, effectively, from the lenders to the target's selling shareholders, rather than to the target itself. This constitutes a fraudulent transfer given that the target received nothing in exchange for pledging assets as collateral and repaying the loan.[6] Even though the buyout deal may have indirectly benefited the target, this type of transfer is nonetheless considered fraudulent since the assets available for debt repayment have been reduced as a result. Second, the target's original creditors may argue that the newly incurred debt obligations impaired the target's financial condition, which subsequently led to its bankruptcy.

[5] Ayash and Rastad (2021) show that firms targeted in PE buyouts are more likely to file for bankruptcy within ten years after the buyout, compared to a matched sample of control firms.

[6] According to court rulings (e.g., *Robinson v. Wangemann, 75 F.2d 756*), a firm receiving its own treasury stock is not considered as receiving equivalent value in exchange.

Given a successful lawsuit against the target, the buyout deal will be unwound to repay the target's original unsecured creditors. The unwinding of the buyout may prove costly to the target's selling shareholders, since they may have to return the sale proceeds, and to the lending bank that financed the buyout deal, since it could now lose its lien on the target's assets.

An important feature of PE buyouts is that the target's assets are pledged as collateral for the buyout loan, which results in the target's highly leveraged structure and increases the target's bankruptcy risk. Due to this increased bankruptcy risk, the CFL represents a significant concern to the target's selling shareholders and to the lending bank that financed the buyout loan, since the deal could be legally challenged and unwound if the target goes bankrupt. Following the adoption of the CFL, the target's selling shareholders and the lending bank may factor in the risk of buyouts being unwound. They may therefore demand higher takeover premiums and higher interest rates for buyout loans. As buyout costs increase, fewer buyouts will be economically feasible, resulting in a decline in buyout activity:[7]

Prediction 1. Following the adoption of the CFL, the cost of undertaking PE buyouts increases.

Prediction 2. Following the adoption of the CFL, PE buyout activity decreases.

3.2. Firms' responses to reduced PE buyout activity

A primary purpose of PE buyouts is to increase the value of the target firms, since the acquirers' objective is to exit the target at a profit. One of the ways in which PE firms can achieve this goal is by improving managerial incentives within the targets. On one hand, PE firms provide the targets' managers with increased equity ownership (e.g., Kaplan and Stromberg, 2009; Gompers et al., 2016), thus increasing the target managers' financial rewards should they work to improve their firms' value. On the other hand, PE firms often threaten to replace the targets' managers should they perform poorly.

[7]I provide a simple framework to illustrate how the CFL impacts PE buyout activity in the Appendix.

Suppose that the cost of losing his or her job outweighs the personal benefits that a manager receives from acting against shareholders' interests. In response to the increased likelihood of their firms being targeted in a PE buyout, managers may thus exert more effort to increase firm value, to avoid being replaced after the buyout. Firms with higher valuations, however, have lower chances of being bought out since the acquirers are more likely to initiate buyouts if they believe they can further increase their targets' value. It follows that managers of firms with higher valuation are less likely to receive the additional equity ownership that comes with being acquired in PE buyouts, thereby reducing their chances of earning higher financial rewards. For this reason, potential targets' managers will exert effort to the point where their marginal benefit of effort equals their marginal cost of effort, in order to maximize their own utility. In other words, these managers have an incentive to exert more effort to increase firm value so as to prevent job losses, but not to the extent that the buyouts become unprofitable for PE firms, so that they may potentially benefit from increased equity ownership. In sum, the increased likelihood of becoming PE buyout targets reduces managerial entrenchment in potential target firms by providing managers with the incentives to improve the value of their firms.

Prediction 3. Managers become more entrenched as the likelihood of their firms becoming PE buyout targets decreases.

4. Data

I use data from multiple sources. The sample of PE buyouts comes from Refinitiv's Security Data Company (SDC) Platinum M&A database. I retrieve all completed leveraged buyouts for which the target's state of headquarters is in the U.S., excluding partial buyouts, self-tenders, and recapitalizations. I obtain additional information on the buyout deals from VentureXpert, including the names and locations of all PE firms involved in each deal, as well as the dates on which each PE firm invested. My sample begins in 1976, which is the year that SDC first

recorded a PE buyout. I include five years of data before and after each state adopted the CFL. Since the last state to adopt the CFL did so in 1999, my sample ends in 2004.

I obtain data on default events and bankruptcy filings from Moody's Default and Recovery Database (DRD). Specifically, I extract all observations between the years 1976 and 2004 that constitute a debt default or bankruptcy under Moody's definition. From Refinitiv's Dealscan database, I collect all U.S. dollar-denominated loans made to U.S. firms originating between 1982 and 2004.[8] I exclude loans with missing information on all-in spread drawn, loan amount, maturity, and the state in which the borrower primarily operates. I consider a loan to be a PE buyout loan if Dealscan classifies the loan's primary or secondary purpose as either an "LBO" or an "MBO."

I construct a panel of U.S. firms and obtain financial data from Compustat. I exclude regulated utility firms (SIC codes from 4900 to 4999), financial firms (SIC codes from 6000 to 6999), and firms headquartered outside the U.S. I also exclude observations with a negative book value of assets or negative net sales. A limitation of the Compustat database is that it only provides information on firms' most recent state of incorporation and headquarters. Since the difference-in-differences identification strategy in this paper relies on variations generated from states where firms are headquartered, it is important to use the correct information on the locations of the firms' headquarters. To this end, I obtain historical data on firms' headquarters states from Bill McDonald's Augmented 10-X Header Data, which was extracted and compiled from 10-K and 10-Q filings on EDGAR.[9] The panel of U.S. firms begins in 1994, as firms' headquarters information is not available before then.

Table 1 presents the summary statistics. To minimize the effect of outliers, all continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are provided in the Appendix (Table A.1). Panel A reports statistics on PE buyout activity at the state-year level.

[8]Data coverage for loans with U.S. borrowers begins in 1982.

[9]https://sraf.nd.edu/data/augmented-10-x-header-data

The average PE buyout loan size is USD 7.4 million, and the average number of PE buyouts completed is 3.0. Panel B reports statistics for the sample of Compustat firms, including measures relating to firm policies, performance, valuation, and risk. The average leverage ratio for firms is 28%. On average, firms pay their shareholders 2% of their total assets, invest 12% of their total assets, and pay their employees USD 98 million in total annually, with 2,933 employees working for these firms. On average, firms' annual growth rate is around 22%, ROA is -5%, Tobin's Q is 2.7, the market-to-book ratio is 2.5, and annualized stock return volatility is 0.9. Panel C reports statistics for the sample of Dealscan loans. The main variable of interest is the loan spread, which is the amount a borrower pays in basis points over the LIBOR plus loan origination fees. The sample consists of 1,605,064 loan tranches, with an average loan spread of 191 basis points.

5. Empirical analysis

The empirical analysis exploits the adoption of the CFL by U.S. state courts, where the timing of the law adoption is staggered across states. I consider states that adopt the CFL to be those that pass any version of the fraudulent transfer act legislated by the NCCUSL or introduce the concept of the constructive definition of fraud into their statutory or case law.[10]

To examine the impact of the CFL, I use a difference-in-differences framework with a stacked regression approach (e.g., Gormley and Matsa, 2011; Cengiz et al., 2019). During the sample period of 1976 to 2004, ten states adopted the CFL, a process I refer to as an "event". For each event, I create a dataset that includes five years of observations before and after the event. The treatment group consists of firms headquartered in the state of the event. The control group consists of firms headquartered in states where the law was not adopted in the five years

[10]Columns (1) and (2) of Table B.1 in the Appendix list the earliest adoption of any version of the NCCUSL fraudulent transfer acts for each state. Column (3) indicates whether a state adopted a constructive definition of fraud through statutory or case law prior to the enactment of the NCCUSL fraudulent transfer acts. Details on the NCCUSL acts are retrieved from NCCUSL and Thomson Reuters West Law. Pre-existing statutory or case law is identified from Ersahin et al. (2020).

before and after the event, as well as firms headquartered in states that had already adopted the law before the start of the sample period (i.e., before 1976). The reason for including firms in already-treated states in the control group is to increase statistical power. I then stack the ten datasets together, and the resultant panel consists of data from ten events.

One may be concerned that states that had already adopted the law may not be appropriate controls. If the treatment effects of the law take more than one year to fully materialize and are hence dynamic, the difference-in-differences estimates could be biased (e.g., Goodman-Bacon, 2021; Baker et al., 2022). Considering that only states that adopted the law long before the beginning of the sample period are included in the control group in my analysis, it is likely that the law had already fully taken effect in these states. Therefore, biases arising from dynamic treatment effects should be less of an issue. Further, Table E.1 shows that my results are robust to the exclusion of already-treated states.

The baseline specifications are as follows. To examine the impact of the CFL at the state level, I estimate the following stacked difference-in-differences regression:

$$Y_{s,t,k} = \beta CFL_{s,k} \times Post_{t,k} + \gamma' X_{s,k} \times Post_{t,k} + \alpha_{s,k} + \lambda_{t,k} + \varepsilon_{s,t,k}, \tag{1}$$

where s indexes states, t indexes years, and k indexes events (i.e., states' adoption of the law). Ten states adopted the law during the sample period, therefore, $k = 1, 2, ..., 10$. $Y_{s,t,k}$ is the outcome of interest for state s in year t. $CFL_{s,k}$ is an indicator that equals one if state s will be adopting the law in event k or had already adopted the law before the start of the sample period (i.e., before 1976). For each event k, $Post_{t,k}$ is an indicator that equals one if year t is in the year when event k occurred or in the post-event period. The main coefficient of interest is β, which captures the average treatment effect of the law across the ten events. $\alpha_{s,k}$ and $\lambda_{t,k}$ denote event-specific state fixed effects and event-specific year fixed effects. $X_{s,k}$ is a vector of state-level characteristics, consisting of *Ln(Per capita income)*, *Ln(Taxes)*, *Ln(Number of firms)*, *Ln(Population)*, *Unemployment rate*, and *Homeownership rate*. I use the pre-treatment

values of these control variables, measured as the average over the five years before the law's adoption. The reason for using pre-treatment values is because the contemporaneous values of the variables could be endogenous to the outcome variable of interest (e.g., Angrist and Pischke, 2009). Standard errors are clustered by state, the level at which the law is implemented (e.g., Bertrand et al., 2004).

To examine the impact of the CFL at the firm level, I estimate the following stacked difference-in-differences regression:

$$Y_{i,s,t,k} = \beta CFL_{s,k} \times Post_{t,k} + \gamma' X_{i,k} \times Post_{t,k} + \alpha_{s,k} + \lambda_{t,k} + \omega_{i,k} + \varepsilon_{i,s,t,k}, \tag{2}$$

where i indexes firms, s indexes states, t indexes years, and k indexes events (i.e., states' adoption of the law). $Y_{i,s,t,k}$ is the outcome of interest for firm i headquartered in state s in year t. $CFL_{s,k}$ and $Post_{t,k}$ are defined as before. $\alpha_{s,k}$ and $\lambda_{t,k}$ are event-specific state fixed effects and event-specific year fixed effects. The reason for including event-specific state fixed effects is that some firms may relocate their headquarters to another state. $\omega_{i,k}$ denotes event-specific firm fixed effects. $X_{i,k}$ is a vector of the pre-treatment values of firm-level characteristics, which is the average of firm-level characteristics over the five years before the law's adoption. The vector of firm-level characteristics in my main analysis consists of *Ln(Assets)*, *Leverage*, *Cash/Assets*, *ROA*, *Tobin's Q*, *Sales growth*, and *Stock volatility*. Standard error are clustered at the state level, as before.

Similarly, I estimate equation (2) to examine the impact of the CFL at the loan level, but with some changes to its notation. Here, $Y_{i,s,t,k}$ refers to the outcome of interest for loan i issued to a borrower headquartered in state s in year t. $\omega_{i,k}$ refers to an event-specific lender fixed effect. All other variables are defined as before.

6. Results

6.1. The impact of the CFL on PE buyout activity

6.1.1. Evidence on the underlying mechanism

I begin by investigating the mechanism through which the CFL leads to a reduction in PE buyout activity. As discussed in section 3.1, the adoption of the CFL increases the risk of PE buyouts being unwound, thereby increasing the costs of buyouts and reducing buyout activity. Increases in takeover premiums and higher interest rates are two ways that the CFL could increase the cost of PE buyouts. However, the analysis of takeover premiums is complicated by the fact that data are only available for a fraction of the sample. I thus investigate whether lenders charge higher interest rates for PE buyout loans after the CFL is adopted. To test this, I estimate equation (2) separately for the sample of PE buyout loans and for the sample of all other loans (i.e., non-PE buyout loans), then compare the coefficients on *CFL* across the two regressions. The dependent variable, *Spread*, is the all-in spread drawn of loan i issued to a borrower headquartered in state s in year t. I use contemporaneous values for the control variables since the dataset on Dealscan loans is cross-sectional and not a panel dataset. The control variables are *Ln(Amount)*, *Ln(Maturity)*, *Secured*, *Senior*, *Covenant*, *Sole lender*, *Refinance*, and *Performance pricing*.

Table 5 presents the results. As shown in Column (1), the spread of PE buyout loans increases by 55 basis points after the law's adoption, which represents a 28.8% increase relative to the sample mean. By contrast, Column (2) shows that the effect is insignificant for the sample of all other loans (i.e., non-PE buyout loans). In Columns (3) and (4), I show that the results are robust to controlling for loan characteristics. Further, in both specifications, the difference between the coefficient estimates on *CFL* for the two samples of loans is statistically significant at the 1% level. These findings are consistent with the prediction that the cost of financing PE buyouts increases following the adoption of the CFL.

6.1.2. PE buyout activity at the state level

Next, I test the prediction that the adoption of the CFL results in a reduction in PE buyout activity. I examine the impact of the CFL adoption on PE buyout activity at the state level, using the number of completed PE buyouts as a measure for PE buyout activity. [11]

Table 2 presents the results. Columns (1) and (3) examine the impact of the CFL on the number of PE buyout deals. Column (1) shows that the coefficient estimate on *CFL* is negative and statistically significant at the 5% level, and Column (3) shows that the result is robust to controlling for state-level characteristics. The coefficient estimate on *CFL* suggests that the number of PE buyouts declines by 1.31. This estimate implies a 43% decrease relative to the sample mean. Overall, these findings are consistent with the prediction that PE buyout activity declines after the adoption of the CFL.

As the difference-in-differences estimate captures the differences in outcomes between law-adopting and non-adopting states, the large magnitude in the estimated impact of the CFL on PE buyout activity may imply that not only has PE buyout activity declined in law-adopting states, but it has also increased in non-adopting states. In other words, it may be that PE firms are shifting their buyout activities from law-adopting states, where buyout costs are higher, to non-adopting states in pursuit of lower costs. In Section 6.1.4, I provide a formal test of whether PE firms indeed reallocate their investments to non-adopting states.

The difference-in-differences framework assumes that the outcomes of interest for the treatment and control groups would trend in parallel prior to the treatment. The presence of a pre-treatment trend could therefore undermine the validity of the analyses. To address this concern, I examine the dynamic impact of the law by introducing lead-lag terms into equation

[11] The size of the buyout deal could also be used as a measure of PE buyout activity. As with the takeover premium, data on deal size is limited to only a subsample of deals, since most deals are proprietary. Another alternative could be to use the dollar amount of PE buyout loans as a measure of PE buyout activity. However, while a reduction in PE buyout loan amount could indicate a decline in PE buyout activity, it could also indicate that the acquirers are using less leverage in the buyout deals. In untabulated tests, the findings are nonetheless robust when PE buyout activity is measured by deal size and by the dollar amount of PE buyout loans.

(1):

$$Y_{s,t,k} = \sum_{\tau=-5,\tau\neq-1}^{\tau=5} \beta_\tau \times CFL_{s,k} \times \mathbb{1}\{t=\tau\} + \sum_{\tau=-5,\tau\neq-1}^{\tau=5} \gamma'_\tau \times X_{s,k} \times \mathbb{1}\{t=\tau\} + \alpha_{s,k} + \lambda_{t,k} + \varepsilon_{s,t,k}, \tag{3}$$

where $\mathbb{1}\{t=\tau\}$ is an indicator for τ years relative to the adoption of the CFL, and all other variables are defined as before. Consider the following example. Colorado adopted the CFL in 1999. Therefore, $\tau = 1$ equals one in the year 2000, since the law has been in effect for one year. $\tau = -1$ equals one in the year 1998, since the law will be adopted in one year. The period right before the law change ($\tau = -1$) serves as the reference point and is thus excluded.

In Table 2, Columns (2) and (4) examine the dynamic impact of the CFL on the number of PE buyouts. Figure 1 graphically illustrates the impact of the CFL on PE buyout activity over time, with point estimates and 90% confidence intervals plotted. In all periods before the law change (i.e., $\tau < 0$), the coefficient estimates on the interactions between *CFL* and $\mathbb{1}\{t=\tau\}$ are statistically insignificant, whereas the coefficient estimates are negative and significant in the years following the law change, except for the year right after the change. In other words, the reduction in PE buyout activity coincides with the passage of the law, and the law did not seem to have an impact on PE buyout activity prior to its passing. These findings alleviate the concern that differential pre-treatment trends are driving the results.

I next investigate how PE buyout activity varies cross-sectionally in response to the law. Specifically, I examine whether changes in PE buyout activity differ between financial acquirers and strategic acquirers. Gains from buyouts to the acquirers could be partly attributed to wealth transfers from pre-buyout debtholders of target firms (e.g., Marais et al., 1989; Asquith and Wizman, 1990; Warga and Welch, 1993). As the adoption of the CFL increases the creditor rights of the target firms' original unsecured creditors, it becomes more costly to expropriate wealth from these creditors. In this case, financial acquirers could find buyouts less attractive compared to strategic acquirers, which would make them more likely to reduce their investments

in law-adopting states. The idea is that strategic acquirers have pre-existing operations and are therefore able to generate long-term operational synergies with their target firms, whereas financial acquirers use shell companies to acquire their target firms and are therefore unable to do so. To test this, I estimate equation (1) separately for the sample of PE buyouts undertaken by financial acquirers and for the sample of PE buyouts undertaken by strategic acquirers.

Table 2 presents the results. Column (5) shows that the number of PE buyouts undertaken by financial acquirers decreases by 0.9 deals. The coefficient estimate on *CFL* represents a 55% decrease relative to the sample mean. By contrast, Column (9) shows that the reduction in the number of PE buyouts undertaken by strategic acquirers is noticeably smaller, and that the coefficient estimate is statistically insignificant. Columns (6) and (10) further examine the dynamic impact of the law on the subsample of PE buyouts undertaken by financial and strategic acquirers. There is no evidence that pre-treatment trends drive the results. Columns (7), (8), (11), and (12) show that the results are robust to controlling for state-level characteristics. Overall, these findings suggest that the reduction in buyout activity following the adoption of the CFL is concentrated within buyouts undertaken by financial acquirers.

6.1.3. The likelihood of firms becoming PE buyout targets

So far, the evidence suggests that PE buyout activity declines following the adoption of the CFL. I next investigate whether firms are less likely to become PE buyout targets. I conduct the analysis at the firm-year level by estimating a linear probability model using equation (2). The dependent variable, *Target*, is an indicator that equals one if firm i was acquired in a PE buyout in year t. I include a set of control variables considered in Opler and Titman (1993) that are known to affect the probability of a firm being acquired in a PE buyout. Specifically, the vector of controls consists of *Operating income/Assets*, *Tobin's Q*, *Machinery indicator*, *R&D/Sales*, *Selling expenses/Sales*, *Ln(Assets)*, *HHI*, *HighOpinc*$\times$*LowTobinQ*, *LowOpinc*$\times$*HighTobinQ*, and *HighHHI*$\times$*LowTobinQ*.

Table 3 presents the results. To improve readability, the coefficient estimates and standard errors are multiplied by 100. The coefficient estimate on *CFL* in Column (1) suggests that firms are 0.24% less likely to be targeted in a PE buyout following the law change. Column (2) shows that the results are robust to including controls. Given that the unconditional probability of being targeted is 0.16%, the coefficient estimates on *CFL* in both columns imply a reduction of more than 100% in the probability of becoming a buyout target. This again points to the possibility that PE firms may be reallocating their investments from law-adopting states to non-adopting states.

In summary, the results at both the state level and the firm level provide strong and consistent evidence that the CFL leads to a reduction in PE buyout activity.

6.1.4. Reallocation of PE buyout activity

The large magnitude of the coefficient estimates in Tables 2 and 3 could suggest that PE firms are reallocating their investments from law-adopting states to non-adopting states. The idea is that when states adopt the CFL, PE firms that had planned to invest in these states may now shift their investments to non-adopting states, where buyout costs are presumably lower. To explore this possibility, it would be ideal to compare the buyout activity of these affected PE firms with their buyout activity had the law not been adopted. However, since one cannot observe the counterfactual, it is impossible to know whether an investment would have been made had the law not been adopted.

To overcome this challenge, I compare the buyout activity of PE firms that are more likely to be affected by the law with that of PE firms that are less likely to be affected. I define PE firms that are more likely to be affected by the law as those that either: (1) are headquartered in states that adopt the law during the sample period (i.e., eventually-treated states), (2) have historically invested in eventually-treated states, or (3) have historically invested in states other than their state of headquarters. The first criterion is based on the idea that investors tend to

have preferences for familiar investments, and may therefore be more likely to invest in firms in close proximity (e.g., Coval and Moskowitz, 1999; Bernile et al., 2015; Ellis et al., 2020). In this view, PE firms may have planned to acquire firms in the same states where they are based, and therefore their investments are likely to be affected if their state of headquarters adopts the law. Likewise, the second criterion is based on the idea that PE firms may continue to seek out buyout targets in states where they have previously made investments, and therefore their investments will likely be affected if these states adopt the law. The third criterion is based on the idea that PE firms investing in more than one state are less likely to be those with a particular focus on local investments and will have fewer restrictions on where they invest, making them more likely to have planned to invest in states that adopt the law.

To investigate whether PE firms reallocate their buyout activity, I focus on PE firms' buyout activity in states where the law has not yet taken effect. I examine whether PE firms that are more likely to be affected by the law adoption increase their buyout activity in these states, compared to PE firms that are less likely to be affected. To test this, I estimate the following regression based on a sample of PE firms' buyout activity in non-adopting states:

$$Y_{i,j,t,k} = \beta TreatedPE_{i,k} \times Post_{t,k} + \omega_{i,k} + \lambda_{t,k} + \varepsilon_{i,t,k}, \tag{4}$$

where i indexes PE firms, j indexes states, t indexes years, and k indexes events (i.e., states' adoption of the law). The dependent variable, $Y_{i,j,t,k}$, is an indicator that equals one if PE firm i undertook a buyout in state j in year t. $TreatedPE_{i,k}$ is an indicator that equals one if the investments of PE firm i are more likely to be affected by the law. For each event k, $Post_{t,k}$ is an indicator that equals one if year t is the year of the event or in the post-event period. β is the parameter of interest, which, if positive, implies that buyout activity has indeed been reallocated to non-adopting states.

Table 4 presents the results. The coefficient estimate on the interaction term $TreatedPE \times Post$ is positive and statistically significant at the 5% level, and the estimate implies a 120%

increase relative to the sample mean. This finding indicates that PE firms that are more likely to be affected by the law invested more in non-adopting states than those that are less likely to be affected by the law, which supports the prediction that PE firms would shift their investments away from law-adopting states to non-adopting states. However, this finding also implies that the estimated effects of the law on PE buyout activity in Tables 2 and 3 are overstated, since the estimates capture the difference between increased activity in non-adopting states and reduced activity in law-adopting states. It is important to keep this in mind when interpreting the magnitude of the law's impact, while also noting that the sign of the coefficient estimates remains unchanged, and thus the implications of my findings remain the same.

6.2. How do firms respond to reduced PE buyout activity?

Thus far, the evidence suggests that PE buyout activity declines following the adoption of the CFL. In this section, I examine whether and how firms respond to the reduction in PE buyout activity. Though the firms' managers are predicted to become entrenched, the manner in which they would act against shareholders' interests is theoretically ambiguous. For instance, managers may be tempted to engage in empire-building behavior, since managing larger firms comes with private benefits, such as increased compensation and power (e.g., Baumol, 1959; Marris, 1964; Williamson, 1964; Jensen, 1986). In this regard, managers may invest excessively in an effort to grow their firms beyond their optimal size. Alternatively, managers may be tempted to exert less effort or avoid difficult decisions in pursuit of a quiet life (e.g., Grossman and Hart, 1983; Bertrand and Mullainathan, 2003). In this regard, managers may make fewer investments than are optimal, since investing is a difficult task that requires much effort. Another example is that managers' career concerns could affect their decision-making (e.g., Holmström, 1999), such as undertaking acquisitions to reduce firm risk, even though they could lead to value destruction for their firms (e.g., Gormley and Matsa, 2016).

Theories on entrenched managers' preferences are not mutually exclusive, making it difficult

to conclude how managerial preferences change by observing only a single aspect of the firm. To address this issue, I examine the impact of the law change on firm valuation, performance, risk, and various firm policies. I then interpret the findings collectively to better understand how managerial preferences change under the CFL.

6.2.1. Firm valuation, performance, and risk

I begin by testing the prediction that managers become entrenched when their firms are less likely to become PE buyout targets. Based on previous evidence that managerial entrenchment is negatively associated with firm value (e.g., Gompers et al., 2003; Bebchuk et al., 2009), I examine whether there is a reduction in Tobin's Q following the law change. Panel A of Table 6 presents the results. Column (1) shows that Tobin's Q declines following the adoption of the CFL, and Column (2) shows that this finding is robust to including controls. Further analysis of the cumulative abnormal returns around the effective date of the law, in Table E.2, shows that the returns are negative on average. Overall, the law's detrimental effects on firm valuation are consistent with the view that managers become entrenched following the law change.

Next, I examine the impact of the CFL on firm performance, using ROA as a measure of firm performance. Column (3) shows that the coefficient estimate on *CFL* is positive and statistically significant at the 1% level, and Column (4) shows that this result is robust to including controls. These findings suggest that operating performance improves following the law change. This contrasts with Bertrand and Mullainathan's (2003), who find that firms with reduced takeover threats have lower profitability, which is indicative of the firms' managers enjoying a quiet life. Yet, one cannot reject the quiet life hypothesis based solely on the findings of improved firm performance, since no clear prediction exists as to how firm performance is affected if managers enjoy a quiet life. For instance, the improved firm performance could result from managers' cherry-picking high NPV projects with low risk, which would not contradict the hypothesis that managers are enjoying a quiet life.

In Panel B of Table 6, I examine the impact of the CFL on firm growth. Columns (1) and (2) examine asset growth, which is measured as the year-over-year growth in firms' total assets. As shown, the coefficient estimates on *CFL* are negative and statistically significant at the 1% level, both with and without the inclusion of controls. Columns (3) and (4) examine PP&E growth, which is measured as the year-over-year growth in firms' net PP&E. Again, the coefficient estimates on *CFL* are negative and statistically significant at the 1% level. Finally, Columns (5) and (6) examine sales growth, which is measured as the year-over-year growth in firms' total revenue. The results show that the coefficient estimates on *CFL* are negative, but statistically significant only with the inclusion of controls. Overall, these findings suggest that firm growth decreases following the adoption of the CFL. Given that empire-building managers are predicted to expand their firms, the finding of lower firm growth is inconsistent with the hypothesis that managers are empire-builders.

To examine the changes in managers' risk-taking behavior, I also examine the impact of the law on firm risk, using three measures to capture the different aspects of firm risk. Panel C of Table 6 presents the results. Columns (1) and (2) examine stock return volatility, which is a summary measure of firm risk that reflects the financial and non-financial aspects of risk. Columns (3) and (4) examine idiosyncratic volatility, which is the risk unique to each firm. The results from Columns (1) to (4) indicate that there is no evidence that the overall risk of firms or firm-specific risks have changed. Columns (5) and (6) examine operational risk, which is measured by cash flow volatility (e.g., Gormley and Matsa, 2016). As shown, the coefficient estimates on *CFL* are negative and statistically significant at the 1% level. This finding of lower operational risk, together with the findings of lower sales growth and higher ROA, suggests that the firms' managers are cherry-picking positive NPV projects that are of low risk. The decision to forego positive NPV projects that are risky may indicate that managers are exerting less effort, which would be consistent with managers enjoying a quiet life, or it may indicate that managers are reducing firm risk and enhancing firm profitability owing to their own career

concerns.

Finally, I explore the implications of the law change for creditors by examining the changes to the likelihood of debt defaults and bankruptcies. Managers whose interests are aligned with those of shareholders have incentives to engage in risk-shifting (e.g., Jensen and Meckling, 1976), which adversely affects debtholders by increasing the agency costs of debt. Since the CFL induces managerial entrenchment, adoption is predicted to reduce defaults and bankruptcy risks on debt. To test this prediction, I estimate a linear probability model using equation (2), where the dependent variables are *Default* and *Bankrupt*. *Default* is an indicator that equals one if firm i defaulted on its debt in a given year t, whereas *Bankrupt* is an indicator that equals one if firm i filed for bankruptcy in a given year t. Panel D of Table 6 presents the results. Column (1) shows that firms are around 0.4% less likely to default on their debt, with a statistical significance at the 1% level. Similarly, the coefficient estimate on *CFL* in Column (3) reveals that firms are around 0.3% less likely to file for bankruptcy. Columns (2) and (4) show that the results are robust to including controls. Overall, these findings are consistent with the view that managerial entrenchment reduces risk-shifting from shareholders to debtholders (e.g., Francis et al., 2010).

6.2.2. Firm policies

The next step is to examine the impact of the CFL on firm policies, in order to gain further insight into the behavior of entrenched managers. I begin by investigating the impact of the law on firms' capital structure. On one hand, entrenched managers may prefer less leverage. Among the reasons are managers' desire to reduce firm risk in order to protect their own human capital (e.g., Fama, 1980), as well as to avoid high levels of debt that limit their ability to pursue private interests (e.g., Jensen, 1986; Berger et al., 1997; Morellec, 2004). On the other hand, corporate control rights can be adjusted through leverage, wherein entrenched managers are likely to favor more leverage to increase their voting power and deter takeovers (e.g., Milton

and Raviv, 1988; Stulz, 1988). Panel A of Table 7 reports the estimated impact of the law on capital structure. Since there is no evidence of a change in book leverage and market leverage, none of the above theories can be rejected.

I next investigate how the firms' financial policies have changed. Specifically, I examine the changes in firms' new financing activity, which is defined as the sum of net debt and equity issuances. The results are presented in Columns (1) and (2) of Panel B, in Table 7. The coefficient estimate on *CFL* is negative and statistically significant at the 1% level, and it is robust to including controls. These findings suggest that firms are raising less capital. To further investigate firms' financing decisions, I break down firms' financing activities into debt and equity financing. The results, presented in Columns (3) to (6), suggest that the decline in new capital raised is the result of a reduction in both debt and equity financing.

Reductions in equity financing can be caused by firms reducing new equity issuance or increasing share repurchases. To determine which of these possibilities is more likely, I investigate how payout policies have changed. A finding of no change or reduced payouts would suggest that the reduction in equity financing is more likely the result of fewer shares being issued. Table 8 presents the results. Columns (1) and (2) examine total payouts to shareholders, which is defined as the sum of common dividends and share repurchases scaled by total assets. The coefficient estimate on *CFL* is negative and statistically significant at the 1% level, indicating a reduction in payouts. There are several reasons why entrenched managers might prefer to reduce payouts. For instance, payouts lead to a reduction in firms' free cash flow, which may limit managers' ability to engage in wasteful spending (e.g., Jensen, 1986), or may increase their need to raise external capital, which may place them under increased scrutiny (e.g., Rozeff, 1982; Easterbrook, 1984). While both these theories may point to managers as empire builders, the latter may also point to managers' inclinations to enjoy a quiet life. Columns (3) to (6) further reveal that the reduction in payouts is largely the result of a reduction in dividend payments. This implies that the reduction in equity financing is due to firms issuing fewer new shares

rather than an increase in share repurchases.

Examining changes in investment policy may provide additional insight into whether the managers' behaviors are more consistent with building empires or enjoying a quiet life. Empire-builders are predicted to make excessive investments, whereas managers who prefer a quiet life tend to invest less. To test these opposing views, I examine firms' total investment expenditures, which is defined as the sum of capital expenditures and R&D expenditures, scaled by total assets. As shown in Columns (1) and (2) of Table 9, the coefficient estimates on *CFL* are negative and statistically significant at the 5% level. This finding of a reduction in investments is more consistent with managers enjoying a quiet life. Further analysis in Columns (3) to (6) shows that there is no evidence of a change in R&D expenditures and that the reduction in investments is mainly the result of lower capital expenditures.

Finally, I examine the number and wages of employees to evaluate how management has changed its policies regarding employment. Table 10 presents the results. Columns (1) and (2) show an increase in total wages paid to employees, and the coefficient estimates are statistically significant at the 1% level. In Columns (3) to (6), I investigate whether this increase is the result of a higher average wage per employee or due to the hiring of more employees. Based on the estimates in Columns (3) and (4), it appears that the average wage per employee has not changed. Columns (5) and (6) show the impact of the law on the number of employees. The coefficient estimates on *CFL* are positive and statistically significant at the 1% level. These findings suggest that the increase in total wages is attributable to the hiring of more employees. It is important to note, however, that only a fraction of firms report wage data. The results should therefore be interpreted with this caveat in mind. Nevertheless, the coefficient estimate on the number of employees remains positive and statistically significant, regardless of whether the sample is restricted to firm-year observations with wage data, which is reassuring.

It is unclear whether this trend of hiring more employees is indicative of managers enjoying a quiet life, their concern for their careers, or their desire to build empires. On one hand, it

may be that managers are offering long-term employment contracts to workers, which would support the idea that quiet-life managers form alliances with employees to enjoy job security (e.g., Pagano and Volpin, 2005). On the other hand, hiring more employees could be a way for managers to expand their firms, which would be indicative of empire-building behavior (Bertrand and Mullainathan, 2003).

6.2.3. Empire building, quiet life, or career concern?

The finding of reduced payouts to shareholders supports Jensen's (1986) view that entrenched managers prefer to retain free cash flow. However, the implication that managers misuse free cash flow to build empires is not supported by the evidence here of reduced firm growth. While hiring more employees may be attributed to managers' empire-building behavior, career concerns, or their preference for a quiet life, once we take into account the changes in other aspects of the firm, the big picture appears to be more consistent with the latter two explanations. The rationale is as follows. First, firms are cutting down capital expenditures, and it appears that there is also a decrease in firm growth, which is more consistent with the firms' managers slacking off. Second, the combination of reduced sales growth, higher return on assets, and lower cash flow volatility suggests that managers are cherry-picking positive NPV projects with low risk. As discussed earlier, this could be explained either by managers enjoying a quiet life or by their career concerns. Therefore, it seems more likely that the increased number of employees is an indication of managers' desire to form alliances with employees to ensure their own job security, rather than of managers' inclination towards firm expansion. Together, these findings suggest that managers enjoy a quiet life when their firms are less likely to become PE buyout targets, and at the same time, their career concerns appear to affect their decision-making as well.

6.3. Robustness tests

6.3.1. Alternative explanations: increased financial constraints?

A concern may be that the firms' reduced payouts and investments are the result of firms' increased financial constraints rather than weakened governance caused by their reduced likelihood of becoming PE buyout targets. The CFL may increase the firms' financial constraints in two ways. First, under the CFL, unsecured creditors are in a stronger position since they have the right to challenge the transactions of their debtors as fraudulent if their debtors fail to meet their debt obligations. Because of the increased litigation risk posed by unsecured creditors, managers may be less inclined to take on unsecured debt. In this case, the findings in previous sections could therefore reflect firms' increased financial constraints as a result of managers' avoiding unsecured debt. However, this seems unlikely since the sample of firms examined in this paper are public firms. In general, public firms do not rely on unsecured debt as a major source of funding. Further, as public firms have better access to public capital markets, they are less likely to be financially constrained even if they reduce their use of unsecured debt.

Nevertheless, to rule out this possibility, I examine whether debt composition changes following the adoption of the CFL, as a means of determining whether firms reduce their use of unsecured debt. Following the literature (e.g., Benmelech et al., 2020, Giambona et al., 2021), I compute the share of unsecured debt in total debt as one minus secured debt, divided by the sum of the book value of total long-term and short-term debt. The result, presented in Table E.3, suggests that there is no evidence of a change in the composition of firms' debt, alleviating the concern that a reduction in of unsecured debt is driving the results.

These findings further highlights the fact that the strengthening of unsecured creditor rights may affect managerial decisions differently depending on the ownership structure of a firm. In particular, Ersahin et al. (2020) show that the stronger unsecured creditor rights following the adoption of the CFL result in entrepreneurial firms reducing their use of unsecured debt, and

thus leads to a reduction in entrepreneurial activity. For entrepreneurial firms, the managers' personal assets are often tied to the firm and therefore managers are at risk of losing their personal assets if unsecured creditors sue their firm under the CFL. By contrast, the personal assets of public firm managers are not tied to the firm, so they are less likely to be concerned, which may explain why public firms do not adjust their use of unsecured debt.

The reduction in PE buyout activity may also increase financial constraints, since PE buyouts may be a way for firms to obtain capital and alleviate credit constraints (e.g., Boucly et al., 2011). It is unlikely that this is an explanation for my results, however, for two reasons. First, the sample of firms examined in this paper are public firms. Since public firms tend to have easier access to public capital markets, they are less likely to experience financial constraints in the absence of a buyout. Second, these firms are increasing employee expenses, suggesting that increased financial constraints are unlikely to be driving the results.

6.3.2. Endogeneous to state-specific economic conditions?

Another concern may be that the adoption of the CFL is driven by macroeconomic factors at the state level. For instance, states may decide to adopt the CFL in economic downturns to protect creditor rights. In this case, the reduction in PE buyout activity could be the result of a slowdown in the economy. This seems unlikely, however, given that firms are hiring more employees following the adoption of the CFL, which is unlikely to be explained by a slowdown in economic activity. To further alleviate this concern, I examine the dynamics of state-level macroeconomic factors prior to the adoption of the law. Specifically, I examine GDP growth, per capita personal income, total taxes, number of firms, population, unemployment rate, and home ownership rate. Table E.4 shows that the effect of the law is not present in the five years prior to the adoption of the law, suggesting that state-level macroeconomic conditions are unlikely to have contributed to the states' decision to adopt the law.

6.3.3. Robustness to excluding firms that relocate their state of headquarters

Another potential concern is that firms may decide whether to be subject to the CFL by relocating their state of headquarters. I therefore exclude firms that relocated their state of headquarters to another state in the five years before the CFL was adopted in their original state of headquarters, and re-estimate the impact of the CFL. Table E.5 presents the results and suggests that my findings remain robust. However, one should note that this analysis is limited by the fact that it is impossible to determine which of the firms chose to remain in their state of headquarters due to the adoption of the law. Consequently, it remains possible that the findings could be confounded by firms' decisions of where to locate their state of headquarters. Yet, as shown in previous sections, the law does not appear to have an impact on buyout activity before its adoption, reducing the possibility that firms had anticipated the adoption of the law and had based their decisions about where to locate their headquarters accordingly.

7. Conclusion

Using the staggered adoption of the constructive fraud provision by U.S. state courts that increase the cost of PE buyout financing as a source of variation in PE buyout activity, I examine how firms respond to reduced PE buyout activity. I first show that firm valuation declines following the law change, consistent with the prediction that managers become entrenched when their firms are less likely to become PE buyout targets. I then examine the impact of the law change on outcomes that capture various aspects of the firm. Specifically, I find that firms raise less capital, reduce payouts and investments, and cherry-pick positive NPV projects with low risk, consistent with the firms' managers slacking off and reducing their risk-taking. Despite making fewer investments, firms hire more employees, consistent with the firms' managers forming alliances with employees for job security. These findings suggest that while managers enjoy a quiet life when their firms are less likely to become PE buyout targets,

career concerns also affect their decision-making. Further analysis shows that the firms are less likely to default on their debts or go bankrupt, consistent with managerial entrenchment reducing risk-shifting behavior. Finally, the findings in this paper differ from the literature that investigates firms' responses to reduced takeover threats in general, suggesting that firms may respond differently depending on the characteristics of the potential takeover.

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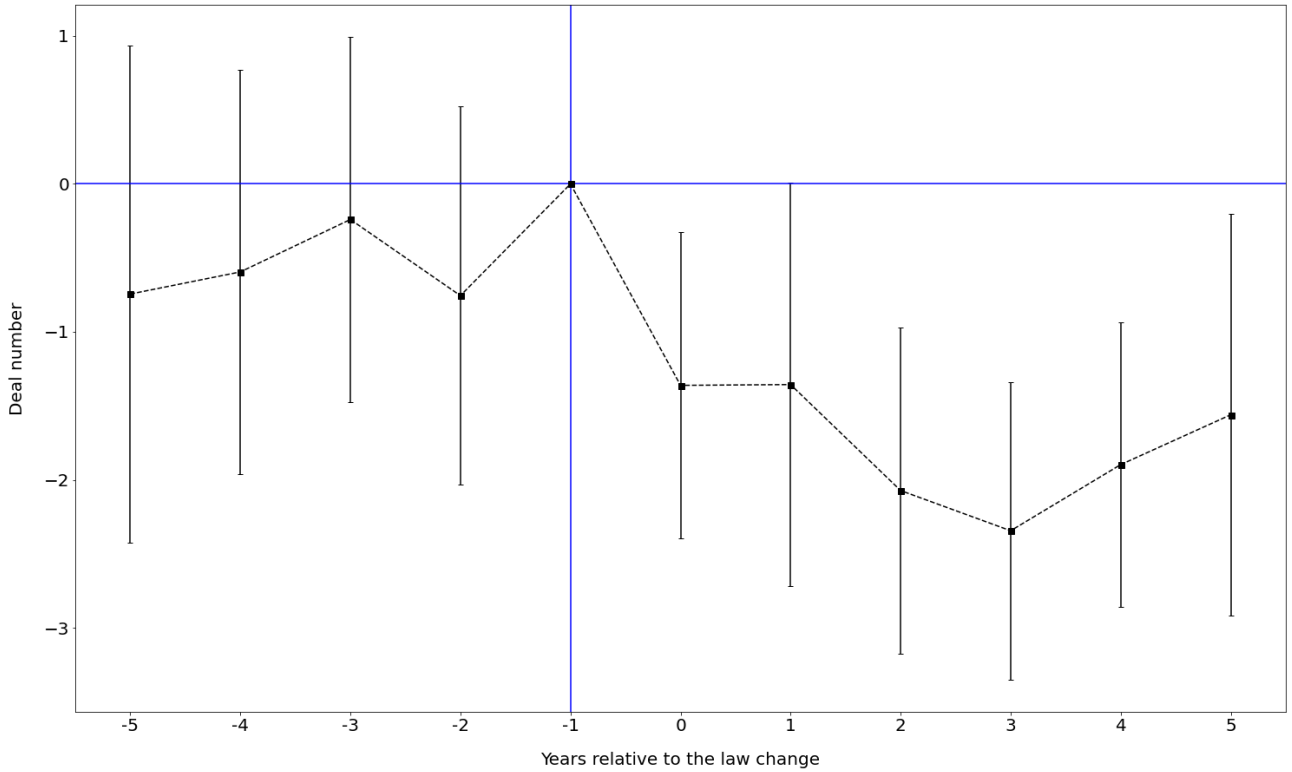

Figure 1: Timing of changes in PE buyout activity around the adoption of the CFL

This figure shows the decline in PE buyout activity at the state-level following the adoption of the CFL. Specifically, the figure plots the estimated β_τ coefficients and 90% confidence intervals from the following regression: $Y_{s,t,k} = \sum_{\tau=-5,\tau\neq-1}^{\tau=5} \beta_\tau \times CFL_{s,k} \times \mathbb{1}\{t=\tau\} + \alpha_{s,k} + \lambda_{t,k} + \varepsilon_{s,t,k}$, where s indexes states, t indexes years, and k indexes events (i.e., states' adoption of the CFL). $Y_{s,t,k}$ is the number of completed PE buyouts in state s in year t. $CFL_{s,k}$ is an indicator equal to one if state s will be adopting the law in event k or has already adopted the law before the start of the sample period (i.e., before 1976). For each event k, τ is the number of years relative to the event year (i.e., adoption of the CFL). The period before the law change ($\tau = -1$) serves as the reference year and is thus omitted.

Table 1: Summary statistics

Variable	Observations	Mean	SD	Min	p10	p50	p90	Max
Panel A: State-year level variables								
Deal number	4,985	3.048	5.363	0.000	0.000	1.000	9.000	46.000
Panel B: Firm-year level variables								
Book leverage	117,364	0.283	0.359	0.000	0.000	0.214	0.586	2.869
Market leverage	116,340	0.186	0.190	0.000	0.000	0.130	0.474	0.748
New financing	104,248	0.160	0.759	-0.490	-0.101	0.007	0.389	9.489
Net debt issuance	104,248	0.051	0.282	-0.731	-0.101	0.000	0.212	2.174
Net equity issuance	104,248	0.094	0.544	-0.174	-0.034	0.000	0.127	7.426
Payout/Assets	106,991	0.022	0.046	0.000	0.000	0.000	0.066	0.288
Dividend/Assets	106,991	0.006	0.015	0.000	0.000	0.000	0.022	0.100
Repurchases/Assets	106,991	0.014	0.036	0.000	0.000	0.000	0.044	0.226
Investment/Assets	115,788	0.120	0.149	0.000	0.014	0.075	0.263	1.061
Capex/Assets	115,788	0.062	0.066	0.000	0.009	0.042	0.136	0.401
R&D/Assets	115,788	0.057	0.131	0.000	0.000	0.000	0.166	0.944
Ln(Wage)	7,332	4.584	2.510	-2.590	1.063	4.716	7.906	8.955
Ln(Wage per employee)	7,332	-3.417	0.788	-5.994	-4.640	-3.251	-2.659	-1.239
Ln(Employee)	7,332	7.984	2.453	0.693	4.654	8.285	11.002	11.516
Asset growth	117,578	0.218	0.928	-0.782	-0.232	0.058	0.587	10.595
PPE growth	115,969	0.222	0.938	-0.935	-0.259	0.043	0.668	8.687
Sales growth	115,969	0.216	0.902	-0.948	-0.226	0.080	0.563	9.176
Cash flow volatility	89,108	0.137	0.168	0.020	0.038	0.090	0.258	1.463
Idiosyncratic volatility	117,274	0.988	0.886	0.284	0.404	0.735	1.755	6.511
Stock volatility	117,274	0.932	0.912	0.176	0.298	0.675	1.731	6.571
ROA	117,460	-0.052	0.498	-4.837	-0.300	0.064	0.179	0.334
Tobin's Q	116,692	2.666	6.465	-31.771	0.263	1.782	6.398	46.062
Target×100	144,751	0.158	3.974	0.000	0.000	0.000	0.000	100.000
Default×100	118,234	0.318	5.630	0.000	0.000	0.000	0.000	100.000
Bankrupt×100	118,234	0.251	5.006	0.000	0.000	0.000	0.000	100.000
Panel C: Loan-year level variables								
Spread	1,605,064	191.055	124.207	17.000	35.000	187.500	350.000	555.000

This table presents summary statistics. Panel A reports statistics for the state-year panel. The sample includes all completed leveraged buyouts from SDC Platinum M&A database, excluding partial buyouts, self-tenders, recapitalizations, and deals for which the targets' state of headquarters is located outside of the U.S. Panel B reports statistics for the firm-year panel. The sample includes all Compustat firms headquartered in the U.S. over the 1994-2004 period, excluding financial and utility firms, and observations with negative values for total assets or net sales. Panel C reports statistics for the loan-year panel. The sample contains all U.S. dollar-denominated loans made to U.S. firms from Dealscan for the 1982-2004 period. All variables are defined in the Appendix (Table A.1). All continuous variables are winsorized at the 1th and 99th percentiles.

Table 2: Constructive Fraud Provision (CFL) and state-level PE buyout activity

Dependent variable:	Deal number											
Acquirer type:	All				Financial acquirer				Strategic acquirer			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
CFL × Post	-1.305**		-1.374*		-0.947***		-0.908**		-0.357		-0.467	
	(0.537)		(0.746)		(0.325)		(0.398)		(0.240)		(0.373)	
CFL × $\mathbb{1}\{t=-5\}$		-0.744		-0.922		-0.235		-0.303		-0.509		-0.618
		(1.021)		(1.152)		(0.466)		(0.543)		(0.609)		(0.678)
CFL × $\mathbb{1}\{t=-4\}$		-0.595		-0.705		-0.172		-0.137		-0.423		-0.567
		(0.831)		(1.011)		(0.366)		(0.447)		(0.523)		(0.628)
CFL × $\mathbb{1}\{t=-3\}$		-0.241		-0.247		0.152		0.174		-0.392		-0.421
		(0.752)		(0.941)		(0.343)		(0.414)		(0.467)		(0.560)
CFL × $\mathbb{1}\{t=-2\}$		-0.755		-0.927		-0.387		-0.539		-0.369		-0.388
		(0.778)		(0.974)		(0.405)		(0.525)		(0.471)		(0.555)
CFL × $\mathbb{1}\{t=0\}$		-1.362**		-1.485		-0.701*		-0.768		-0.660		-0.717
		(0.630)		(0.903)		(0.359)		(0.472)		(0.396)		(0.583)
CFL × $\mathbb{1}\{t=1\}$		-1.356		-1.505		-0.812*		-0.728		-0.545		-0.777
		(0.828)		(1.182)		(0.449)		(0.559)		(0.499)		(0.669)
CFL × $\mathbb{1}\{t=2\}$		-2.073***		-2.443**		-1.270***		-1.436***		-0.803**		-1.007
		(0.670)		(1.033)		(0.402)		(0.518)		(0.396)		(0.616)
CFL × $\mathbb{1}\{t=3\}$		-2.344***		-2.584**		-1.514***		-1.576***		-0.830**		-1.008*
		(0.612)		(0.977)		(0.375)		(0.508)		(0.403)		(0.586)
CFL × $\mathbb{1}\{t=4\}$		-1.897***		-1.810*		-1.184***		-0.959*		-0.713*		-0.851
		(0.584)		(0.935)		(0.439)		(0.549)		(0.382)		(0.605)
CFL × $\mathbb{1}\{t=5\}$		-1.560*		-1.773		-0.959**		-0.935		-0.601		-0.838
		(0.824)		(1.138)		(0.472)		(0.613)		(0.513)		(0.675)
Event-specific year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-treatment controls (interacted)	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Observations	4,985	4,985	4,307	4,307	4,985	4,985	4,307	4,307	4,985	4,985	4,307	4,307
Adjusted R-squared	0.751	0.750	0.777	0.780	0.680	0.680	0.707	0.711	0.645	0.644	0.662	0.663

This table presents difference-in-differences estimates of the impact of the CFL on PE buyout activities at the state-level. The dependent variable is the number of PE buyout deals completed in state s in year t. The sample is divided into buyouts with financial acquirers (Columns 5 to 8) and strategic acquirers (Columns 9 to 12). *CFL* is an indicator that equals one if state s will be adopting the CFL in event k or has already adopted the CFL before the start of the sample period (i.e., before 1976). *Post* is an indicator that equals one if year t is in the year when event k occurred or in the post-event period. $\mathbb{1}\{t=\tau\}$ is an indicator for τ years relative to the adoption of the CFL. The sample includes state-year observations from 1976 to 2004. Standard errors are clustered at the firms' state of headquarters level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Table 3: Constructive Fraud Provision (CFL) and the likelihood of becoming a PE buyout target

Dependent variable:	Target	
	(1)	(2)
CFL × Post	-0.226***	-0.275***
	(0.050)	(0.065)
Event-specific year fixed effects	Yes	Yes
Event-specific state fixed effects	Yes	Yes
Event-specific firm fixed effects	Yes	Yes
Pre-treatment controls (interacted)	No	Yes
Observations	120,226	120,226
Adjusted R-squared	0.079	0.079

This table presents the difference-in-differences estimates of the impact of the CFL on the likelihood of becoming a PE buyout target. The dependent variable is *Target*, which is an indicator equal to one if a firm was acquired in a PE buyout in a given year. *CFL* is an indicator that equals one if state s will be adopting the CFL in event k or has already adopted the CFL before the start of the sample period (i.e., before 1976). *Post* is an indicator that equals one if year t is in the year when event k occurred or in the post-event period. The sample includes firm-year observations from 1994 to 2004. The coefficient estimates and standard errors are multiplied by 100 to improve readability. Standard errors are clustered at the firms' state of headquarters level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Table 4: Reallocation of PE buyout activity

Dependent variable:	Reallocation
	(1)
TreatedPE × Post	0.217**
	(0.097)
Event-specific year fixed effects	Yes
Event-specific firm fixed effects	Yes
Observations	126,216
Adjusted R-squared	0.002

This table presents difference-in-differences estimates of the impact of the CFL on PE buyout activity in states for which the law has not yet been adopted. Specifically, I estimate equation (4) on the sample of PE buyout activity in non-adopting states. The dependent variable, *Reallocation*, is an indicator that equals one if PE firm i undertook a buyout in a given state in a given year. *TreatedPE* is an indicator equal to one if the PE firm is identified as more likely to be affected by the CFL. *Post* is an indicator that equals one if year t is in the year when event k occurred or in the post-event period. The sample includes firm-year observations from 1976 to 2004. Standard errors are clustered at the event times firms' state of headquarters level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Table 5: Constructive Fraud Provision (CFL) and loan spreads

Dependent variable:	Spread (bps)		Spread (bps)	
Loan purpose:	PE buyout	Others	PE buyout	Others
	(1)	(2)	(3)	(4)
CFL × Post	55.105***	-13.853	57.616***	-20.746*
	(8.975)	(14.552)	(4.726)	(10.928)
Event-specific year fixed effects	Yes	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes	Yes
Event-specific lender fixed effects	Yes	Yes	Yes	Yes
Loan controls	No	No	Yes	Yes
Observations	127,768	1,477,296	127,768	1,477,296
Adjusted R-squared	0.350	0.333	0.412	0.590
F-test (F-stat., p-value)	(44.29, 0.000)		(47.04, 0.000)	

This table presents the difference-in-differences estimates of the impact of the CFL on loan spreads for the subsamples of loans that were used to finance PE buyouts and those for all other purposes. The sample includes loan-year observations from 1982 to 2004. The dependent variable, *Spread*, is the all-in spread drawn. *CFL* is an indicator that equals one if state s will be adopting the CFL in event k or has already adopted the CFL before the start of the sample period (i.e., before 1976). *Post* is an indicator that equals one if year t is in the year when event k occurred or in the post-event period. Using an F-test, I examine whether there is a statistical difference in the coefficient estimates of the CFL between the two subsamples (i.e., PE buyout loans versus all other loans). Standard errors are clustered at the firms' state of headquarters level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Table 6: Constructive Fraud Provision (CFL) and firm valuation, performance, growth, and risk

Panel A: Firm valuation and performance				
Dependent variable:	Tobin's Q		ROA	
	(1)	(2)	(3)	(4)
CFL × Post	-29.538*	-29.628**	3.185***	2.608***
	(15.642)	(14.351)	(0.758)	(0.692)
Event-specific year fixed effects	Yes	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes	Yes
Event-specific firm fixed effects	Yes	Yes	Yes	Yes
Pre-treatment controls (interacted)	No	Yes	No	Yes
Observations	116,692	116,692	117,460	117,460
Adjusted R-squared	0.309	0.312	0.653	0.654

Panel B: Firm growth						
Dependent variable:	Asset growth		PPE growth		Sales growth	
	(1)	(2)	(3)	(4)	(5)	(6)
CFL × Post	-16.389***	-19.072***	-15.136***	-17.138***	-0.286	-3.159*
	(1.498)	(1.892)	(4.432)	(4.120)	(1.865)	(1.624)
Event-specific year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Event-specific firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Pre-treatment controls (interacted)	No	Yes	No	Yes	No	Yes
Observations	117,578	117,578	115,969	115,969	115,696	115,696
Adjusted R-squared	0.231	0.232	0.248	0.249	0.265	0.266

Panel C: Firm risk						
Dependent variable:	Stock volatility		Idiosyncratic volatility		Cash flow volatility	
	(1)	(2)	(3)	(4)	(5)	(6)
CFL × Post	2.816	2.011	3.033	2.331	-2.234***	-2.344***
	(3.756)	(3.681)	(3.774)	(3.640)	(0.626)	(0.656)
Event-specific year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Event-specific firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Pre-treatment controls (interacted)	No	Yes	No	Yes	No	Yes
Observations	117,274	117,274	117,274	117,274	89,108	89,108
Adjusted R-squared	0.643	0.644	0.557	0.557	0.741	0.742

Panel D: Likelihood of default and bankruptcy				
Dependent variable:	Default		Bankruptcy	
	(1)	(2)	(3)	(4)
CFL × Post	-0.371***	-0.467***	-0.320***	-0.399***
	(0.125)	(0.121)	(0.088)	(0.084)
Event-specific year fixed effects	Yes	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes	Yes
Event-specific firm fixed effects	Yes	Yes	Yes	Yes
Pre-treatment controls (interacted)	No	Yes	No	Yes
Observations	118,234	118,234	118,234	118,234
Adjusted R-squared	0.032	0.032	0.046	0.046

This table presents the difference-in-differences estimates of the impact of the CFL on firms' valuation, performance, growth, and risk. *CFL* is an indicator that equals one if state s will be adopting the CFL in event k or has already adopted the CFL before the start of the sample period (i.e., before 1976). *Post* is an indicator that equals one if year t is in the year when event k occurred or in the post-event period. The sample includes firm-year observations from 1994 to 2004. The coefficient estimates and standard errors are multiplied by 100 to improve readability. Standard errors are clustered at the firms' state of headquarters level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. All variables are defined in the Appendix (Table A.1).

Table 7: Constructive Fraud Provision (CFL) and capital structure and financial policies

Panel A: Capital structure						
Dependent variable:	Book leverage		Market leverage			
	(1)	(2)	(3)	(4)		
CFL × Post	-0.595	0.246	2.675	2.623		
	(0.962)	(1.106)	(1.664)	(1.646)		
Event-specific year fixed effects	Yes	Yes	Yes	Yes		
Event-specific state fixed effects	Yes	Yes	Yes	Yes		
Event-specific firm fixed effects	Yes	Yes	Yes	Yes		
Pre-treatment controls (interacted)	No	Yes	No	Yes		
Observations	117,364	117,364	116,340	116,340		
Adjusted R-squared	0.676	0.678	0.717	0.718		
Panel B: Financial policies						
Dependent variable:	New financing		Net debt issuance		Net equity issuance	
	(1)	(2)	(3)	(4)	(5)	(6)
CFL × Post	-20.836***	-23.663***	-5.712***	-4.608***	-7.289***	-10.978***
	(6.474)	(6.951)	(1.980)	(1.842)	(2.543)	(3.007)
Event-specific year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Event-specific firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Pre-treatment controls (interacted)	No	Yes	No	Yes	No	Yes
Observations	104,248	104,248	104,248	104,248	104,248	104,248
Adjusted R-squared	0.327	0.328	0.235	0.237	0.337	0.341

This table presents the difference-in-differences estimates of the impact of the CFL on firms' capital structure and financial policies. *Book leverage* is the sum of total long-term debt and short-term debt divided by book value of total assets. *Market leverage* is the sum of total long-term debt and short-term debt divided by the market value of total assets. *New financing* is the sum of net equity issuance and net debt issuance. *New debt issuance* is the change in the sum of book value of long-term and short-term debt scaled by lagged book value of total assets. *New equity issuance* is sales of equity minus purchases of equity scaled by lagged book value of total assets. *CFL* is an indicator that equals one if state s will be adopting the CFL in event k or has already adopted the CFL before the start of the sample period (i.e., before 1976). *Post* is an indicator that equals one if year t is in the year when event k occurred or in the post-event period. The sample includes firm-year observations from 1994 to 2004. The coefficient estimates and standard errors are multiplied by 100 to improve readability. Standard errors are clustered at the firms' state of headquarters level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Table 8: Constructive Fraud Provision (CFL) and payout policies

Dependent variable:	Payout/Assets		Dividend/Assets		Repurchases/Assets	
	(1)	(2)	(3)	(4)	(5)	(6)
CFL × Post	-0.530***	-0.444***	-0.251*	-0.243*	0.017	0.086
	(0.108)	(0.110)	(0.134)	(0.133)	(0.260)	(0.270)
Event-specific year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Event-specific firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Pre-treatment controls (interacted)	No	Yes	No	Yes	No	Yes
Observations	106,991	106,991	106,991	106,991	106,991	106,991
Adjusted R-squared	0.514	0.514	0.839	0.839	0.417	0.417

This table presents the difference-in-differences estimates of the impact of the CFL on firms' payout policies. *Payout/Assets* is the sum of dividends and repurchases scaled by book value of total assets. *Dividend/Assets* is common dividend scaled by book value of total assets. *Repurchases/Assets* is the purchase of common and preferred stock scaled by book value of total assets. *CFL* is an indicator that equals one if state s will be adopting the CFL in event k or has already adopted the CFL before the start of the sample period (i.e., before 1976). *Post* is an indicator that equals one if year t is in the year when event k occurred or in the post-event period. The sample includes firm-year observations from 1994 to 2004. The coefficient estimates and standard errors are multiplied by 100 to improve readability. Standard errors are clustered at the firms' state of headquarters level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Table 9: Constructive Fraud Provision (CFL) and investment policies

Dependent variable:	Investment/Assets		Capex/Assets		R&D/Assets	
	(1)	(2)	(3)	(4)	(5)	(6)
CFL × Post	-1.971**	-1.939**	-1.759***	-1.643***	0.051	-0.035
	(0.741)	(0.756)	(0.590)	(0.580)	(0.191)	(0.170)
Event-specific year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Event-specific firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Pre-treatment controls (interacted)	No	Yes	No	Yes	No	Yes
Observations	115,788	115,788	115,788	115,788	115,788	115,788
Adjusted R-squared	0.700	0.701	0.636	0.637	0.776	0.776

This table presents the difference-in-differences estimates of the impact of the CFL on firms' investment policies. *Investment/Assets* is the sum of capital expenditures and R&D expenditures scaled by book value of total assets. *Capex/Assets* is capital expenditures scaled by book value of total assets. *R&D/Assets* is research and development expenditures scaled by book value of total assets. *CFL* is an indicator that equals one if state s will be adopting the CFL in event k or has already adopted the CFL before the start of the sample period (i.e., before 1976). *Post* is an indicator that equals one if year t is in the year when event k occurred or in the post-event period. The sample includes firm-year observations from 1994 to 2004. The coefficient estimates and standard errors are multiplied by 100 to improve readability. Standard errors are clustered at the firms' state of headquarters level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Table 10: Constructive Fraud Provision (CFL) and employment policies

Dependent variable:	Ln(Wage)		Ln(Wage per employee)		Ln(Employee)	
	(1)	(2)	(3)	(4)	(5)	(6)
CFL × Post	19.142***	18.090***	-1.834	-2.425	22.613***	22.176***
	(4.458)	(5.293)	(3.759)	(3.289)	(4.427)	(4.292)
Event-specific year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Event-specific firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Pre-treatment controls (interacted)	No	Yes	No	Yes	No	Yes
Observations	7,332	7,332	7,332	7,332	7,332	7,332
Adjusted R-squared	0.977	0.977	0.868	0.868	0.969	0.969

This table presents the difference-in-differences estimates of the impact of the CFL on firms' employment policies. *Ln(Wage)* is the natural logarithm of labor and related expenses. *Ln(Wage per employee)* is the natural logarithm of labor and related expenses divided by the number of employees. *Ln(Employee)* is the natural logarithm of the number of employees. *CFL* is an indicator that equals one if state s will be adopting the CFL in event k or has already adopted the CFL before the start of the sample period (i.e., before 1976). *Post* is an indicator that equals one if year t is in the year when event k occurred or in the post-event period. The coefficient estimates and standard errors are multiplied by 100 to improve readability. Standard errors are clustered at the firms' state of headquarters level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Appendix A - Variable definitions

Table A.1: Variable definitions

Variable	Definition
Asset growth	Book value of total assets (AT) divided by book value of total assets in the previous year minus one. Source: Compustat.
Bankrupt	Indicator that equals one if a firm filed for bankruptcy in a given year. Source: Moody's Default and Recovery Database.
Book leverage	The sum of book value of total long-term (DLTT) and short-term debt (DLC) divided by book value of total assets (AT). Source: Compustat.
Capex/Assets	Capital expenditures (CAPX) divided by book value of total assets (AT). Source: Compustat.
Cash flow/Assets	Operating income after depreciation (OIADP) minus accruals [(ACT_t − ACT_{t-1}) − (CHE_t − CHE_{t-1}) − (LCT_t − LCT_{t-1}) + (DLC_t − DLC_{t-1}) − DP_t] divided by lagged book value of total assets (AT). Source: Compustat.
Cash flow volatility	The annualized standard deviation of firm's quarterly ratio of Cash flow/Assets, where the estimation window is twelve quarters. Source: Compustat.
CFL	Indicator that equals one if state s will be adopting the CFL in event k or has already adopted the CFL before the start of the sample period (i.e., before 1976).
Covenant	Indicator that equals one if the loan has financial covenants. Source: Dealscan.
Deal number	Number of PE buyouts completed. Source: SDC Platinum.
Default	Indicator that equals one if a firm defaulted on its debt in a given year. Source: Moody's Default and Recovery Database.
Dividend/Assets	Common dividends (DVC) divided by book value of total assets (AT). Source: Compustat.
GDP growth	The growth of the value of the goods and services produced by labor and property. Source: U.S. Bureau of Economic Analysis.
HHI	Herfindahl index of sales (REVT) defined over the firm's four-digit SIC codes. Source: Compustat.
HighHHI	Indicator that equals one if the firm has above the sample median HHI.
HighOpinc	Indicator that equals one if the firm has above the sample median Operating Income/Assets.
HighTobinq	Indicator that equals one if the firm has above the sample median Tobin's Q.
Homeownership rate	The proportion of households that is owner-occupied. Source: U.S. Census Bureau.
Idiosyncratic volatility	The standard deviation of residuals from the market model for stock volatility. Source: CRSP.
Investment/Assets	The sum of capital expenditures (CAPX) and research and development expenditures (XRD) divided by book value of total assets (AT). Source: Compustat.

Variable	Definition
Ln(Amount)	The natural logarithm of the amount of loan facility in millions of dollars. Source: Dealscan.
Ln(Assets)	The natural logarithm of the book value of assets (AT). Source: Compustat.
Ln(Employee)	The natural logarithm of the number of employees (EMP) in millions. Source: Compustat.
Ln(Loan volume)	The natural logarithm of the amount of PE buyout loans in millions of dollars. Source: Dealscan.
Ln(Maturity)	The natural logarithm of loan maturity in months. Source: Dealscan.
Ln(Number of firms)	The natural logarithm of the number of firms. Source: U.S. Census Bureau.
Ln(Per capita income)	The natural logarithm of personal income of all residents divided by the resident population. Source: U.S. Bureau of Economic Analysis.
Ln(Population)	The natural logarithm of the number of total resident population. Source: U.S. Census Bureau.
Ln(Taxes)	The natural logarithm of state and local government tax revenue. Source: U.S. Census Bureau.
Ln(Wage)	The natural logarithm of labor and related expenses (XLR). Source: Compustat.
Ln(Wage per employee)	The natural logarithm of labor and related expenses (XLR) divided by the number of employees (EMP) in millions. Source: Compustat.
LowOpinc	Indicator that equals one if the firm has below the sample median Operating income/Assets.
LowTobinq	Indicator that equals one if the firm has below the sample median Tobin's Q.
Machinery indicator	Indicator that equals one if the standard industrial classification (SIC) code is between 3400 and 4000. Source: Compustat.
Market leverage	The sum of book value of long-term (DLTT) and short-term debt (DLC) divided by the market value of total assets (PRCC_F*CSHO+AT-CEQ). Source: Compustat.
Net debt issuance	The change in the sum of book value of total long-term (DLTT) and short-term debt (DLC) divided by lagged book value of total assets (AT_{t-1}). Source: Compustat.
Net equity issuance	Sales of equity (SSTK) minus purchases of equity (PRSTKC) divided by lagged book value of total assets (AT_{t-1}). Source: Compustat.
New financing	The sum of net debt issuance and net equity issuance.
Operating income/Assets	Earnings before interest, taxes, depreciation, and amortization (EBITDA) divided by the sum of book value of debt (LT) and the market value of equity (PRCC_F*CSHO). Source: Compustat.
Payout/Asset	The sum of common dividends (DVC) and the purchase of common and preferred stock (PRSTKC) divided by book value of total assets (AT). Source: Compustat.

Variable	Definition
Performance pricing	Indicator that equals one if the loan has performance pricing provisions and zero otherwise. Source: Dealscan.
Post	Indicator equal to one if year t is in the year when event k occurred or in the post-event period.
PPE growth	Net property, plant, and equipment (PPENT) divided by net PP&E in the previous year minus one. Source: Compustat.
R&D/Assets	Research and development expense (XRD) divided by book value of total assets (AT). Equal to zero if no R&D expenses are reported. Source: Compustat.
R&D/Sales	Research and development expenditures (XRD) divided by sales (REVT). Equal to zero if no R&D expenses are reported. Source: Compustat.
Reallocation	Indicator that equals one if a PE firm undertook a buyout in a given state in a given year.
Refinance	Indicator that equals one if the loan is to repay existing debt and zero otherwise. Source: Dealscan.
Repurchases/Assets	Purchase of common and preferred stock (PRSTKC) divided by book value of total assets (AT). Source: Compustat.
ROA	Earnings before interest and taxes (EBIT) divided by book value of total assets (AT). Source: Compustat.
Sales growth	Sales (REVT) divided by sales in the previous year minus one. Source: Compustat.
Secured	Indicator that equals one if the loan is secured and zero otherwise. Source: Dealscan.
Selling expenses/Sales	Selling expenses (XSGA) divided by sales (REVT). Equal to zero if no selling expenses are reported. Source: Compustat.
Senior	Indicator that equals one if the loan is senior. Source: Dealscan.
Sole lender	Indicator that equals one if the loan only has one lender. Source: Dealscan.
Spread	All-in spread drawn, which is the amount a borrower pays in basis points over LIBOR for each dollar drawn down. Source: Dealscan.
Stock volatility	The square root of the sum of squared daily returns multiplied by 252 and divided by the number of trading days. Source: CRSP.
Target	Indicator equal to one if a firm was acquired in a PE buyout in a given year. Source: SDC Platinum.
Tobin's Q	The sum of book value of debt (LT) and market value of equity (PRCC_F*CSHO) divided by book value of total assets (AT). Source: Compustat.
TreatedPE	Indicator equal to one if the PE firm is identified as more likely to be affected by the CFL.
Unemployment rate	The number of unemployed as a percentage of the labor force. Source: U.S. Bureau of Labor Statistics.
Unsecured debt/Assets	The sum of book value of total long-term (DLTT) and short-term debt (DLC) minus secured debt (DM) divided by book value of total assets (AT). Source: Compustat.
Unsecured debt/Debt	One minus secured debt (DM) divided by the sum of book value of total long-term (DLTT) and short-term debt (DLC). Source: Compustat.

Appendix B - Adoption of a constructive definition of fraud by state

Table B.1: Adoption of a constructive definition of fraud by state

State	NCCUSL Fraudulent Transfer Act - UFCA/UFTA/UVTA		Pre-existing statutory or case law
	Statutory citation (1)	Effective (2)	Effective (3)
AK	-	-	-
AL	UFTA (Code 1975, §§ 8-9A-1 to 8-9A-12)	1990	Before 1977
AR	UFTA (A.C.A. §§ 4-59-201 to 4-59-213)	1987	Before 1977
AZ	UFCA (A.R.S. §§ 44-1001 to 44-1013)	1919	-
CA	UFCA (Cal.Civ.Code §§ 34349 to 3439.12)	1939	-
CO	UFTA (C.R.S.A. §§ 38-8-101 to 38-8-112)	1991	-
CT	UFTA (C.G.S.A. §§ 52-552a to 52-552)	1991	Before 1977
DC	UFTA (D.C. Official Code, 2001 Ed. §§ 28-3101 to 28-3111)	1996	-
DE	UFCA (Del.C. §§ 1301 to 1312)	1919	-
FL	UFTA (West's F.S.A. §§ 726.101 to 726.112)	1988	Before 1977
GA	UFTA (Ga. Code Ann. §§ 18-2-70 to 18-2-81)	2002	Before 1977
HI	UFTA (HRS §§ 651C-1 to 651C-10)	1985	-
IA	UFTA (I.C.A. §§ 684.1 to 684.12)	1995	-
ID	UFCA (I.C. §§ 55-910 to 55-922)	1969	-
IL	UFTA (S.H.A. 740 ILCS §§ 160/1 to 160/12)	1990	Before 1977
IN	UFTA (West's A.I.C. §§ 32-2-7-1 to 32-2-7-21)	1994	Before 1977
KS	UFTA (K.S.A. §§ 33-201 to 33-212)	1999	-
KY	UVTA (K.R.S. §§ 378A.005 to 378A.140)	2016	Before 1977
LA	-	-	1985
MA	UFCA (M.G.L.A. c. 109A, §§ 1 to 13)	1924	-
MD	UFCA (Code, Com. Law, §§ 15-201 to 15-214)	1920	-
ME	UFTA (14 M.R.S.A. §§ 3571 to 3582)	1986	-
MI	UFCA (M.C.L.A. §§ 566.11 to 566.23)	1919	-
MN	UFCA (M.S.A. §§ 513.20 to 513.32)	1921	-
MO	UFTA (V.A.M.S. §§ 428.005 to 428.059)	1992	Before 1977
MS	UFTA (Code 1972, §§ 15-3-101 to 15-3-121)	2006	Before 1977
MT	UFCA (M.C.A. §§ 31-2-301 to 31-2-325)	1945	-
NC	UFTA (N.C.G.S.A. §§ 39-23.1 to 39-23.12)	1997	Before 1977
ND	UFCA (N.D. Cent. Code §§ 13-02-01 to 13-02-11)	1943	-
NE	UFCA (R.R.S.1943, §§ 36-601 to 36-613)	1980	-
NH	UFCA (R.S.A. §§ 545:1 to 545:12)	1919	-
NJ	UFCA (N.J.S.A. §§ 25:2-7 to 25:2-19)	1919	-
NM	UFCA (N.M.S.A. 1978, §§ 56-10-1 to 56-10-13)	1959	-
NV	UFCA (N.R.S. §§ 112.010 to 112.130)	1931	-
NY	UFCA (N.Y. Debt. & Cred. Law, §§ 270 to 281)	1925	-
OH	UFCA (R.C. §§ 1336.01 to 1336.12)	1961	-
OK	UFCA (24 Okl.St.Ann. §§ 101 to 111)	1965	-
OR	UFTA (O.R.S. §§ 95.200 to 95.310)	1986	-
PA	UFCA (39 P.S. §§ 351 to 363)	1921	-
RI	UFTA (Gen. Laws 1956, §§ 6-16-1 to 6-16-12)	1986	Before 1977
SC	-	-	Before 1977
SD	UFCA (S.D.C.L. §§ 54-8-5 to 54-8-19)	1919	-
TN	UFCA (T.C.A. §§ 66-3-301 to 66-3-325)	1993	-
TX	UFTA (V.T.C.A. Bus. & C. §§ 24.001 to 24.013)	1987	Before 1977
UT	UFCA (U.C.A. 1953, §§ 25-1-1 to 25-1-16)	1925	-
VT	UFTA (9 V.S.A. §§ 2285 to 2295)	1996	-
VA	-	-	Before 1977
WA	UFCA (West's R.C.W.A. §§ 19.40.010 to 19.40.130)	1945	-
WI	UFCA (W.S.A. §§ 242.01 to 242.13)	1919	-
WV	UFTA (Code, §§ 40-1A-1 to 40-1A-12)	1986	Before 1977
WY	UFCA (W.S.A. §§ 34-14-101 to 34-14-113)	1929	-

This table lists the adoption of a constructive definition of fraud by state. Columns (1) and (2) lists the earliest adoption of any version of the NCCUSL Fraudulent Transfer Act and its effective year for each state. Column (3) reports whether states adopt a constructive definition of fraud through statutory or case law prior to the earliest adoption of the NCCUSL acts.

Appendix C - Conceptual framework

In this section, I provide a simple framework to illustrate how the adoption of the CFL affects PE buyouts. The CFL gives original unsecured creditors of PE buyout targets the right to file a lawsuit to unwind the buyout deal when the target firm goes bankrupt and defaults on its debts. Let q denote the probability that the buyout deal is successfully challenged during a lawsuit. In the event of a successful lawsuit, the target's selling shareholders incurs a loss $L > 0$ for having to return the proceeds. Given that the original unsecured creditors will have priority for repayment, the lenders which financed the buyout deal incurs a cost of $c > 0$ for loosing its lien on the buyout target's assets.

Suppose it costs $I > 0$ to acquire a PE buyout target. There are two periods. In the first period, the acquirer decides to undertake the acquisition, where she finances the purchase of the target firm by borrowing I from lenders. The required rate of return for lenders is γ. The probability that the acquisition succeeds in generating a surplus is p. In the second period, the payoff of the project is realized. In case of success, the project has payoff $R > 0$. In case of failure, the payoff is $-L$ if the target's original creditors file a lawsuit, and zero otherwise.

The lender will agree to finance the buyout deal only if the following break-even constraint is satisfied

$$(\gamma - c)(1 - p)q + \gamma[1 - (1 - p)q] \geq 0. \tag{5}$$

The above condition can be rewritten as:

$$\gamma \geq cq(1 - p) = \gamma_{min}(c, p, q), \tag{6}$$

where $\gamma_{min}(c, p, q)$ is the minimum required rate of return for lenders. Differentiating $\gamma_{min}(c, p, q)$ with respect to q gives:

$$\frac{\partial \gamma_{min}(c, p, q)}{\partial q} = c(1 - p) > 0, \tag{7}$$

which implies that the lender's minimum required rate of return $\gamma_{min}(c, p, q)$ increases in the probability of a successful lawsuit q.

Prediction 1. The lender's required rate of return for financing a PE buyout increases following the adoption of the CFL.

Suppose an acquisition must be economically viable to occur. Therefore, the expected surplus S generated by an PE buyout deal should satisfy the following condition:

$$S = pR + (1 - p)q(-L) - \gamma_{min}(c, p, q)I > 0. \tag{8}$$

Differentiating S with respect to q gives

$$\frac{\partial S}{\partial q} = -(L + c)(1 - p) < 0, \tag{9}$$

which implies that the surplus S decreases in the probability of a successful lawsuit q. In other words, the set of viable deals decreases following the law change.

Prediction 2. PE buyout activity decreases following the adoption of the CFL.

Appendix D - Impact of the CFL by state

Using a difference-in-differences approach, this paper exploits the staggered adoption of the CFL by U.S. states to estimate the average treatment effect of the CFL. By estimating the average impact of the law adoption by multiple states, one can avoid arbitrarily selecting a state for analysis and increase statistical power. There is, however, concern regarding the large difference in the timing between the first and last adoption of the law, which may result in the law having a different impact depending on the prevailing economic conditions. To assess this possibility, I estimate the following regression for each of the ten states that adopted the CFL during the sample period 1976 to 2004:

$$Y_{s,t} = \beta CFL_s \times Post_t + \alpha_s + \lambda_t + \varepsilon_{s,t}, \tag{10}$$

where s indexes states and t indexes years. $Y_{s,t}$ is the number of PE buyouts completed in state s in year t. α_s and λ_t are state and year fixed effects. Standard errors are clustered by state. All other variables are defined as before.

The results are presented in Table D.1, and illustrated graphically in Figure D.1. In general, the adoption of the CFL has a negative impact on PE buyout activity in most of the states. The exceptions are the District of Columbia, where there is no evidence that the law has had any impact on buyout activity, and Colorado, where the law appears to have had a positive impact. I therefore exclude Colorado's adoption of the CFL from the sample and re-estimate the impact of the CFL on firm behavior. Table E.6 shows that my findings remain robust. This eliminates the potential concern that my findings could be biased by differences in the impact of the CFL across states.

Table D.1: Constructive Fraud Provision (CFL) and PE buyout activity by state

Dependent variable: deal number				
Adoption state:	Coefficient	Standard error	Observations	Adjusted R^2
	(1)	(2)	(3)	(4)
Nebraska	-0.636***	0.213	508	0.345
Hawaii	-3.478***	0.753	529	0.594
Louisiana	-2.778***	0.753	529	0.594
Maine	-2.492***	0.687	516	0.656
Oregon	-2.225***	0.687	516	0.655
Colorado	0.793**	0.311	502	0.840
Iowa	-0.818***	0.179	483	0.836
District of Columbia	0.323	0.192	470	0.856
Vermont	-0.010	0.192	470	0.856
Kansas	-1.637***	0.227	462	0.815

This table presents the difference-in-differences estimates of the impact of the CFL on PE buyout activity for each of the states that adopted the law during the sample period 1976 to 2004. The dependent variable is the number of PE buyouts completed in state s in year t. Column (1) shows the estimated β coefficients from equation (10). Standard errors are clustered at the firms' state of headquarters level and reported in Column (2). Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

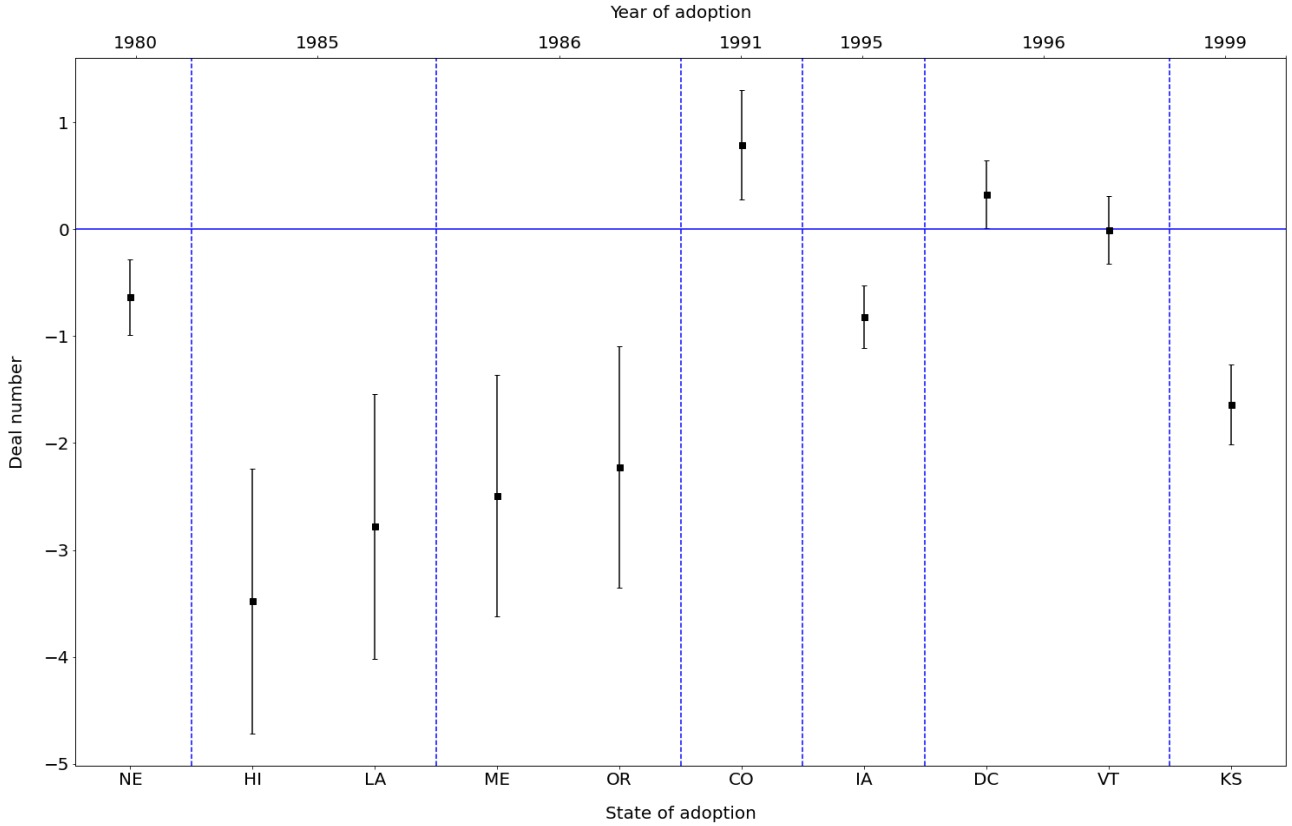

Figure D.1: Constructive Fraud Provision (CFL) and PE buyout activity by state

This figure shows the impact of the CFL on PE buyout activity for each of the states that adopted the law during the sample period 1976 to 2004. Specifically, for each state, the figure plots the estimated β coefficient and 90% confidence interval from the following regression: $Y_{s,t} = \beta CFL_s \times Post_t + \alpha_s + \lambda_t + \varepsilon_{s,t}$, where s indexes states and t indexes years. $Y_{s,t}$ is the number of PE buyouts completed in state s in year t. α_s and λ_t are state and year fixed effects.

Appendix E - Robustness tests and additional analyses

Table E.1: Robustness to the exclusion of already-treated states

Dependent variable:	Deal number			
	(1)	(2)	(3)	(4)
CFL × Post	-0.592**		-0.730**	
	(0.265)		(0.317)	
CFL × $\mathbb{1}\{t = -3\}$		-0.161		0.185
		(0.381)		(0.284)
CFL × $\mathbb{1}\{t = -2\}$		-0.711		-0.522
		(0.483)		(0.872)
CFL × $\mathbb{1}\{t = 0\}$		-0.554		-0.222
		(0.514)		(0.386)
CFL × $\mathbb{1}\{t = 1\}$		-0.055		-0.973
		(0.677)		(0.789)
CFL × $\mathbb{1}\{t = 2\}$		-1.558***		-1.982***
		(0.496)		(0.694)
CFL × $\mathbb{1}\{t = 3\}$		-1.365**		-1.443
		(0.524)		(1.033)
Event-specific year fixed effects	Yes	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes	Yes
Pre-treatment controls (interacted)	No	No	Yes	Yes
Observations	392	392	273	273
Adjusted R-squared	0.106	0.131	0.029	0.205

This table presents difference-in-differences estimates of the impact of the CFL on PE buyout activities at the state-level. The dependent variable is the number of PE buyout deals completed in state s in year t. *CFL* is an indicator that equals one if state s will be adopting the CFL in event k or has already adopted the CFL before the start of the sample period (i.e., before 1976). $\mathbb{1}\{t = \tau\}$ is an indicator for τ years relative to the adoption of the CFL. The sample includes state-year observations from 1977 to 2002, and excludes observations from already-treated states. Standard errors are clustered at the event times firms' state of headquarters level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Table E.2: Constructive fraud law (CFL) and cumulative abnormal stock returns

Event window:	(-1,+1)	(-3,+3)	(-5,+5)	(-10,+10)
Market adjusted return, equally weighted index				
Precision weighted CAAR	0.05%	-0.23%	-1.19%**	-1.06%**

This table presents the precision weighted CAAR around the effective date of the CFL. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Table E.3: Do firms change their composition of debt?

Dependent variable:	Unsecured debt/Debt	Unsecured debt/Assets
	(1)	(2)
CFL × Post	0.429	-1.089
	(2.334)	(0.871)
Event-specific year fixed effects	Yes	Yes
Event specific state fixed effects	Yes	Yes
Event-specific firm fixed effects	Yes	Yes
Pre-treatment controls (interacted)	Yes	Yes
Observations	92,489	108,430
Adjusted R-squared	0.594	0.542

This table presents the difference-in-differences estimates of the impact of the CFL on firms' debt composition. *Unsecured debt/Debt* is one minus secured debt divided by the sum of book value of total long-term and short-term debt. *Unsecured debt/Assets* is the sum of book value of total long-term and short-term debt minus secured debt divided by book value of total assets. *CFL* is an indicator that equals one if state s will be adopting the CFL in event k or has already adopted the CFL before the start of the sample period (i.e., before 1976). $Post_{t,k}$ is an indicator that equals one if year t is in the year when event k occurred or in the post-event period. The sample includes firm-year observations from 1994 to 2004. The coefficient estimates and standard errors are multiplied by 100 to improve readability. Standard errors are clustered at the firms' state of headquarters level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Table E.4: State-level macroeconomic dynamics prior to the adoption of Constructive Fraud Provision (CFL)

Dependent variable:	GDP growth (percent)	Log(Per capita income)	Log(Taxes)	Log(Number of firms)	Log(Population)	Unemployment rate (percent)	Homeownership rate (percent)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CFL $\times \mathbb{1}\{t = -1\}$	-0.214	-0.010	-0.021	0.008	0.007	0.169	-0.284
	(0.418)	(0.010)	(0.046)	(0.018)	(0.012)	(0.223)	(0.601)
CFL $\times \mathbb{1}\{t = -2\}$	0.729	-0.006	-0.027	0.015	0.012	0.016	-0.907
	(0.576)	(0.012)	(0.043)	(0.021)	(0.013)	(0.323)	(0.759)
CFL $\times \mathbb{1}\{t = -3\}$		0.001	0.012	0.014	0.019	-0.226	-1.103
		(0.015)	(0.039)	(0.024)	(0.016)	(0.417)	(0.921)
CFL $\times \mathbb{1}\{t = -4\}$		0.002	0.027	0.018	0.024	-0.265	-1.525
		(0.016)	(0.048)	(0.022)	(0.018)	(0.453)	(1.110)
CFL $\times \mathbb{1}\{t = -5\}$		0.001	0.007	0.024	0.028	0.242	-0.381
		(0.017)	(0.049)	(0.021)	(0.020)	(0.573)	(1.258)
Event-specific year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	930	4,985	4,904	4,776	4,985	4,985	4,146
Adjusted R-squared	0.224	0.994	0.994	0.999	0.999	0.820	0.944

This table presents state-level macroeconomic dynamics prior to the adoption of the CFL. *CFL* is an indicator that equals one if state s will be adopting the CFL in event k or has already adopted the CFL before the start of the sample period (i.e., before 1976). $\mathbb{1}\{t = \tau\}$ is an indicator for τ years relative to the adoption of the CFL. The sample includes state-year observations from 1976 to 2004. Standard errors are clustered at the firms' state of headquarters level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. All variables are defined in the Appendix (Table A.1).

Table E.5: Robustness to the exclusion of firms that relocate their state of headquarters

Panel A: Firm valuation, performance, and growth					
Dependent variable:	Tobin's Q	ROA	Asset growth	PPE growth	Sales growth
CFL × Post	-35.604**	2.196***	-22.674***	-18.382***	-6.870***
	(13.306)	(0.652)	(2.424)	(4.259)	(1.671)
Event-specific year fixed effects	Yes	Yes	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes	Yes	Yes
Event-specific firm fixed effects	Yes	Yes	Yes	Yes	Yes
Pre-treatment controls (interacted)	Yes	Yes	Yes	Yes	Yes
Observations	115,946	116,716	116,828	115,248	114,986
Adjusted R-squared	0.314	0.654	0.233	0.249	0.267
Panel B: Firm risk and the likelihood of default and bankruptcy					
Dependent variable:	Cash flow volatility	Idiosyncratic volatility	Stock volatility	Default	Bankruptcy
CFL × Post	-2.230***	1.958	1.480	-0.456***	-0.384***
	(0.658)	(3.439)	(3.465)	(0.122)	(0.084)
Event-specific year fixed effects	Yes	Yes	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes	Yes	Yes
Event-specific firm fixed effects	Yes	Yes	Yes	Yes	Yes
Pre-treatment controls (interacted)	Yes	Yes	Yes	Yes	Yes
Observations	88,607	116,516	116,516	117,391	117,391
Adjusted R-squared	0.743	0.639	0.644	0.034	0.048
Panel C: Firm policies					
Dependent variable:	New financing	Payout/Assets	Investment/Assets	Ln(Employee)	
CFL × Post	-25.301***	-0.357***	-1.732**	13.257***	
	(7.209)	(0.103)	(0.738)	(2.731)	
Event-specific year fixed effects	Yes	Yes	Yes	Yes	
Event-specific state fixed effects	Yes	Yes	Yes	Yes	
Event-specific firm fixed effects	Yes	Yes	Yes	Yes	
Pre-treatment controls (interacted)	Yes	Yes	Yes	Yes	
Observations	103,634	106,187	115,048	113,484	
Adjusted R-squared	0.329	0.506	0.701	0.961	

This table presents the difference-in-differences estimates of the impact of the CFL on firms' valuation, performance, growth, risk, and firm policies. *CFL* is an indicator that equals one if state s will be adopting the CFL in event k or has already adopted the CFL before the start of the sample period (i.e., before 1976). *Post* is an indicator that equals one if year t is in the year when event k occurred or in the post-event period. The sample includes firm-year observations from 1994 to 2004, and excludes firms that relocate their state of headquarters to another state in the five years before the CFL was adopted in their original state of headquarters. The coefficient estimates and standard errors are multiplied by 100 to improve readability. Standard errors are clustered at the firms' state of headquarters level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. All variables are defined in the Appendix (Table A.1).

Table E.6: Robustness to the exclusion of Colorado's adoption of the CFL

Panel A: Firm valuation, performance, and growth					
Dependent variable:	Tobin's Q	ROA	Asset growth	PPE growth	Sales growth
CFL × Post	-29.628**	2.608***	-19.072***	-17.138***	-3.159*
	(14.249)	(0.687)	(1.879)	(4.090)	(1.613)
Event-specific year fixed effects	Yes	Yes	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes	Yes	Yes
Event-specific firm fixed effects	Yes	Yes	Yes	Yes	Yes
Pre-treatment controls (interacted)	Yes	Yes	Yes	Yes	Yes
Observations	110,744	111,416	111,525	109,977	109,705
Adjusted R-squared	0.303	0.651	0.225	0.238	0.258
Panel B: Firm risk and the likelihood of default and bankruptcy					
Dependent variable:	Cash flow volatility	Idiosyncratic volatility	Stock volatility	Default	Bankruptcy
CFL × Post	-2.344***	2.336	2.011	-0.467***	-0.399***
	(0.652)	(3.616)	(3.654)	(0.121)	(0.083)
Event-specific year fixed effects	Yes	Yes	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes	Yes	Yes
Event-specific firm fixed effects	Yes	Yes	Yes	Yes	Yes
Pre-treatment controls (interacted)	Yes	Yes	Yes	Yes	Yes
Observations	84,497	111,290	111,290	112,161	112,161
Adjusted R-squared	0.737	0.633	0.638	0.036	0.049
Panel C: Firm policies					
Dependent variable:	New financing	Payout/Assets	Investment/Assets	Ln(Employee)	
CFL × Post	-23.663***	-0.464***	-1.939**	12.872***	
	(6.900)	(0.110)	(0.750)	(2.776)	
Event-specific year fixed effects	Yes	Yes	Yes	Yes	
Event-specific state fixed effects	Yes	Yes	Yes	Yes	
Event-specific firm fixed effects	Yes	Yes	Yes	Yes	
Pre-treatment controls (interacted)	Yes	Yes	Yes	Yes	
Observations	98,773	101,159	109,846	108,337	
Adjusted R-squared	0.322	0.494	0.696	0.959	

This table presents the difference-in-differences estimates of the impact of the CFL on firms' valuation, performance, growth, risk, and firm policies. *CFL* is an indicator that equals one if state s will be adopting the CFL in event k or has already adopted the CFL before the start of the sample period (i.e., before 1976). *Post* is an indicator that equals one if year t is in the year when event k occurred or in the post-event period. The sample includes firm-year observations from 1994 to 2004, and excludes the event in which Colorado adopted the CFL. The coefficient estimates and standard errors are multiplied by 100 to improve readability. Standard errors are clustered at the firms' state of headquarters level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively. All variables are defined in the Appendix (Table A.1).