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 reduce investments, cherry-pick positive NPV projects with low risk, and become less likely to default on their debt or go bankrupt, indicative of managers enjoying a quiet life and reducing risk-taking. Further analyses show that firms are less likely to relocate, suggesting that the law adoption positively impacts firm retention.

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1. Introduction

What are the real effects of private equity (hereafter, PE) buyouts? This question has been and remains of critical importance in light of the historical booms and busts and the recent record-high levels of PE buyout activity. Much of the literature has investigated this question with respect to the outcomes of firms acquired in PE buyouts, as well as their stakeholders.[1] It remains unclear, however, how PE buyouts may affect potential target firms. In this paper, I investigate how firms respond to changes in the likelihood of becoming PE buyout targets.

PE buyouts refer to acquisitions in which PE firms acquire target firms using a large amount of debt secured by the targets' assets. As a result, the target firms' ownership structure changes after the buyouts as the firms' equity is replaced with debt. The buyouts can have both upsides and downsides for the target firms' managers. For instance, managers must exert more effort to repay the buyout debt. However, the highly leveraged capital structure following a buyout suggests that managers' shares of equity ownership increase, which implies higher financial returns should they work towards improving the firms' value. Whether managers respond positively or negatively towards a potential buyout may thus depend on whether their private benefits from the buyout outweigh their private costs.

Considering that the PE firms' goal is to ultimately exit the target firm at a profit, buyouts are more likely to be initiated if the PE firms believe that they can increase the target's value. This follows that, when the likelihood of firms becoming buyout targets declines, managers seeking to have their firm bought out will take actions to reduce their firms' value to increase the likelihood of a takeover. As for managers who do not wish their firm to be bought out, their optimal level of entrenchment increases as the likelihood of a takeover decreases (e.g., Manne, 1965). Therefore, the prediction is that, regardless of how managers perceive a potential buyout, managers will become more entrenched in response to a reduced likelihood of a buyout.

To test this prediction, I exploit the staggered adoption of constructive fraud provisions (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

[1] Empirical work investigating changes in target firms' performance following PE buyouts include, for instance, Kaplan (1989), Lichtenberg and Siegel (1990), Smith (1990), Boucly, Sraer, and Thesmar (2011), Guo, Hotchkiss, and Song (2011), Lerner, Sorensen, and Stromberg (2011), and Bernstein and Sheen (2016). Empirical work investigating the impact of PE buyouts on the target firms' stakeholders include, for instance, Davis et al. (2014), Agrawal and Tambe (2016), and Cohn, Nestpriak, and Wardlaw (2021).

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 may respond positively to PE buyouts, as was the case in 99% of the PE buyouts examined in this paper. Thus, firms are less likely 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. Specifically, to test the prediction that the law adoption leads to increased costs of PE buyouts, I investigate whether the lending banks account for the risk of losing their lien on the targets' assets and charge higher interest rates for the buyout loans following the law adoption. I find that the yield spread of PE buyout loans increases following the adoption of the law. In particular, the loan spreads increase by 51 basis points on average, representing a 19% increase relative to the pre-treatment sample mean. By contrast, the spreads of loans used to finance other types of M&A deals (i.e., non-PE buyout M&A deals) 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 decrease by 42% relative to the pre-treatment sample mean. Consistent with this result, I also find that firms are around less likely to become PE buyout targets following the law change. Further analysis shows that PE firms

respond to the increased buyout costs by shifting their investments from law-adopting states to non-adopting states.

The second part of my analysis investigates whether and how firms respond to the reduced likelihood of being targeted in PE buyouts. Although the prediction is that the firms' managers will become more entrenched, the manner in which the mangers would act against shareholders' interests is theoretically ambiguous. 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), and exerting less effort to enjoy a quiet life (e.g., Grossman and Hart, 1983; Bertrand and Mullainathan, 2003). 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 cut back their investments and raise less capital. Second, firm valuation, as measured by Tobin's Q, decreases. Third, while firms become more profitable and operational risks decrease, firm growth appears to decline. Together, these results suggest that the firms' managers cherry-pick positive NPV projects with low risk, consistent with managers exerting less effort and enjoying a quiet life. Further, 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 investments in response to increased financial constraints. This is unlikely, however, since I find no evidence of firms adjusting their debt composition in terms of unsecured and secured debt. Second, since PE buyouts can be a means for firms to obtain capital (e.g., Boucly, Sraer, and Thesmar, 2011), the reduced likelihood of firms becoming PE buyout targets could potentially explain their reduction in investments. However, since public firms have access to public capital markets, they are less likely to experience financial constraints absent a buyout. 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 to the literature on the corporate governance role of takeovers. The empirical literature provides mixed evidence regarding how the likelihood of a takeover affects potential target firms' behavior. Evidence based on the passage of Business Combination laws suggests that the firms' managers respond to reduced hostile takeover threats by exerting less effort and avoiding costly decisions (Bertrand and Mullainathan, 2003), or by taking risk-reducing actions even though they are detrimental to the firms' value (Gormley and Matsa, 2016). Low (2009) finds that Delaware-incorporated firms reduce their risk-taking after the state increased takeover protections. Exploiting the passage of a French protectionist anti-takeover law, Frattaroli (2020) finds that the firms' board of directors increased the sensitivity of pay-for-performance for managers in response to weakened shareholder governance. In contrast to these papers, I exploit the passage of a law which was not designed specifically to restrict buyouts, thereby eliminating the potential concern that firms may have lobbied for the passage of the law. Further, I contribute to the literature by documenting that entrenched managers cherry-pick projects with a positive NPV and low risk, shedding light on an alternative approach taken by managers in pursuit of a quiet life.

This paper is also related to Ersahin, Irani, and Waldock's (2020) work, which investigates the impact of the CFL on entrepreneurial activity. Specifically, they show that, in response to the increased unsecured creditor rights following the adoption of the CFL, small businesses entry declines as entrepreneurs shun away from the higher costs associated with a potential failure of their business. In contrast, I show that following the adoption of the CFL, the secured lenders who financed the PE buyouts face a greater risk of losing their liens, thus resulting in a decline in PE buyout activity as buyout costs increase.

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

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

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 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 receive less than fair value in exchange, and (2) the debtors become insolvent following these transactions. A successful lawsuit against the debtors would result in the debtors' transactions being unwound and the creditors being repaid. With the CFL, creditors who have not been able to receive payments from their debtors are able to challenge their debtors' transactions more easily, as opposed to previously being limited to challenging their debtors' transactions under actual fraud provisions, which required them to prove their 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 and organization of the UFCA, and the substance of the law remained largely unchanged.

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

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 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.

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 raises debt through the shell company and uses the proceeds to acquire 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 pledged as collateral for the secured buyout loans and the target's future cash flows are used to service the loans.

Following a PE buyout, the target may be more at risk of going bankrupt due to its highly

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

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 target's original creditors may argue that the newly incurred debt obligations impaired the target's financial condition, which subsequently led to its bankruptcy. Second, 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. Due to the fact that the target received nothing in exchange for pledging its assets as collateral and repaying the loan, together with the target's inability to repay its creditors, the buyout deal is deemed a fraudulent transfer.[6]

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 lose their 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 increased 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 declines.

[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. 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.

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

3.2. Firms' responses to reduced PE buyout activity

PE buyouts result in a change in the ownership structure of the target firms since equity is replaced with debt. The main implications for the target firms' managers from this change in ownership structure is as follows. First, the new ownership structure is characterised by a higher level of debt, which requires managers to exert additional effort toward debt repayment. Yet, the managers' equity ownership share also increase (e.g., Kaplan and Stromberg, 2009; Gompers, Kaplan, and Mukharlyamov, 2016), which implies that they can expect greater financial returns should they work toward increasing firm value. Second, PE firms may threaten to replace the targets' managers should they perform poorly. Thus, for managers, the probability of their firm becoming a buyout target also implies a threat of job loss.

A manager's perception of a potential buyout depends largely on whether the benefits of their firm becoming a buyout target outweigh the costs to the manager, which can be affected by the manager's private costs. For instance, all else equal, a high-skilled manager may respond more positively to a potential buyout than a low-skilled manager. Specifically, the level of effort required to repay the buyout debt or to increase the firm's value in order to benefit from the increased financial rewards is lower for high-skilled managers than for low-skilled managers.

Acquirers are more likely to initiate buyouts if they believe they can further increase the target firms' value. Given the increased cost of undertaking buyouts following the CFL's adoption, managers who respond positively towards a potential buyout will take actions to reduce the value of their firm in order to increase the likelihood of a buyout. In the case of managers who respond negatively towards a potential buyout, they can increase their level of entrenchment until their firm value is reduced to the point where the likelihood of their firm being targeted is the same as before the law was adopted. Thus, regardless of the managers' perception towards a potential buyout, their level of entrenchment increases following the law change.

Prediction 3. Managers' level of entrenchment increase as the likelihood of their firms becoming buyout targets decrease.

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 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 the following two sources: (1) Bill McDonald's Augmented 10-X Header Data,[9] which was extracted and compiled from 10-K and 10-Q filings on EDGAR dating back to 1994, and (2) for the years prior to 1994, I use data from Bai, Fairhurst, and Serfling (2020), which contains information derived from WRDS SEC Analytics Suite, supplemented by data collected manually from Mergent. Finally, given that a potential concern is that firms may choose whether or not to be subjected to the CFL by relocating their state of headquarters, I exclude firms that moved their headquarters to another state within five years before the CFL was adopted

[8] Data coverage for loans with U.S. borrowers begins in 1982.
[9] https://sraf.nd.edu/data/augmented-10-x-header-data

in their original state of headquarters.[10]

Table 1 presents the summary statistics for the main variables in the pre-treatment period. 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. The average number of completed PE buyouts is 4.1. Panel B reports statistics for the sample of Compustat firms, including measures relating to firm policies, performance, valuation, and risk. On average, firms invest 11.7% of their total assets, pay their shareholders 1.8% of their total assets, and raise capital that account for 15.8% of their total assets. On average, firms' annual growth rate is around 21.6% to 24.2%, ROA is 1.3%, Tobin's Q is 2.1, and annualized stock return volatility is 0.7. 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. On average, the spread for PE buyout loans is 262 basis points, and the spread for all other M&A loans (i.e., non-PE buyout M&A loans) is 220 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.[11] The CFL applies to the state in which the plaintiff (i.e., the debtor being sued) resides; for a lawsuit challenging a PE buyout as constructively fraudulent, the applicable state law is the state in which the target firm is headquartered.

5.1. Stacked difference-in-differences approach

To investigate 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). Throughout the sample period of 1976 to 2004, ten states adopted the CFL, in which I refer to each adoption of

[10] In untabulated tests, I find that the results are robust to the inclusion of firms that relocate their headquarters.

[11] 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, Irani, and Waldock (2020).

the law as an "event." For each event, I create a panel 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 has not yet taken effect during the period of inclusion (i.e., never-treated states and eventually-treated states but whose laws have not yet been implemented), as well as firms headquartered in states where the law was already adopted before the beginning of the sample period (i.e., already-treated states that adopted the law before 1976). I then stack the ten panels together to form the final panel for analysis.

A concern with including already-treated states in the control group is that, if the treatment effects take more than one period to fully materialize, the difference between the treatment and control groups may not be constant in the pre-treatment period, leading to biased difference-in-differences estimates (e.g., Goodman-Bacon, 2021; Baker, Larcker, and Wang, 2022). I provide the following discussion and analysis to suggest that biases induced by dynamic treatment effects are unlikely to be present in my analysis. First, there are often contracts between the limited and general partners in PE funds that stipulate restrictions on the investments, such as limiting the investments to certain regions or industries. With the adoption of the CFL, there may be a need to adjust the investments due to the increased cost of buyouts in law-adopting states, which will likely necessitate contract renegotation. In view of the potential lengthy renegotiations, the impact of law adoption on PE buyout activity may not take full effect within a single period of time. However, since the law had already been in place in the already-treated states before PE buyouts first gained prominence in the 1980s, it is unlikely that there would have been contractual renegotiations due to the lack of contracts at the time the law was adopted. The law adoption is thus expected to have a stable impact on buyout activity in the already-treated states in the post-treatment period. Second, the fact that the majority of the already-treated states have had the law in place for decades suggests that the impact of the law adoption has likely taken full effect in these states during the sample period. Third, further supporting this claim is the fact that, in a dynamic difference-in-differences estimation, I find no evidence of a pre-treatment trend regardless of whether the control group includes or excludes already-treated states.[12]

[12]Table 3 presents results on the dynamic impact of the law adoption on PE buyout activity with the control group consisting of eventually-treated, never-treated, and already-treated states. Column (4) in Panel A of Table 13 presents the results with the control group excluding already-treated states.

I then discuss the potential benefit of using already-treated states as the control group. Given that the adoption of the law increases the cost of undertaking PE buyouts, PE firms may seek to reallocate their investments to states where the law is not yet in effect. This suggests that the difference-in-differences estimates would be overstated if the control group includes states that have not yet adopted the law. Limiting the control group to only already-treated states can therefore reduce the bias associated with spillovers from the treatment states, since such states have already adopted the law and are less likely to experience buyout activity inflows.

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

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

where s indexes states, t indexes years, and k indexes events (i.e., states' adoption of the law). A total of ten states adopted the law during the 1976 to 2004 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 is in the treatment group, that is, the state in which the law will be adopted in event k. For each event k, $Post_{t,k}$ is an indicator that equals one if year t is in the year of the law's adoption or in the period following its adoption. The estimated coefficient β reflects the average treatment effect of the law across the k events. $\alpha_{s,k}$ and $\lambda_{t,k}$ denote event-specific state fixed effects and event-specific year fixed effects. $\theta_s \times t$ denote state-specific linear time trends, which allow for differential trends between the treatment and control states. $X_{s,k}$ 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, Duflo, and Mullainathan, 2004).

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

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

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. $\omega_{i,k}$ and $\lambda_{t,k}$ denote event-specific firm fixed effects and event-specific year fixed effects. $\alpha_{s,k}$ are event-specific state fixed effects. I include event-specific state fixed effects instead of state-specific linear time trends because firms may relocate their headquarters from one state to another, and therefore may be in the treatment or control group at different points in time. $X_{i,k}$ 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 errors are clustered at the state level, as before.

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

$$Y_{i,s,t,k} = \beta CFL_{s,k} \times Post_{t,k} + \alpha_{s,k} + \lambda_{t,k} + \gamma' X_{i,k} + \varepsilon_{i,s,t,k}, \tag{3}$$

where i indexes loans, s indexes states, t indexes years, and k indexes events (i.e., states' adoption of the law). $Y_{i,s,t,k}$ refers to the outcome of interest for loan i issued to a borrower headquartered in state s in year t. $X_{i,k}$ is a vector of control variables, where I use contemporaneous values for the variables since many of the borrowers may not necessarily borrow every year, and may therefore lack pre-treatment characteristics. The control variables include *Ln(Amount)*, *Ln(Maturity)*, *Senior*, *Covenant*, *Sole lender*, *Refinance*, and *Performance pricing*, as well as borrower-industry fixed effects and lender fixed effects. I define all other variables as before, and cluster the standard errors at the state level.

5.2. Structural break in PE buyout financing: junk bond market crash in the late 1980s

Considering the relevance of the CFL to bankruptcy litigations that involve fraudulent transfers, the CFL may not only affect PE buyouts, but may have other implications as well. In this case, it can be challenging to determine whether the observed changes in firm outcomes following the law adoption are indeed reflecting firms' response to a reduced likelihood of becoming buyout targets. I address this concern by drawing upon a structural break that led to changes in the

financing of PE buyouts.

The emergence of junk bonds in the 1980s sparked the first wave of PE buyout activity. As a result of the boom in the junk bond market, banks reduced their positions in PE buyout financing as secured loans were pushed aside by junk bonds. Additionally, buyouts became more highly leveraged and aggressively priced. In an analysis of buyouts in the 1980s, Kaplan and Stein (1993) find that the banks responded by imposing faster repayment schedules, which forced the sale of the buyout targets' assets and shifted the risk onto more junior creditors. In the late 1980s, the collapse of the junk bond market brought an end to this buyout wave. Guo, Hotchkiss, and Song (2011) show that buyouts thereafter became more conservatively priced, less leveraged, and less reliant on asset sales to repay the buyout debt.

Consequently, I predict that the CFL will have a greater impact on PE buyouts in the 1990s than in the 1980s. The idea is that, due to the more conservative buyout pricings in the 1990s, bank repayment schedules are presumably longer, which will expose bank debt to the risk of CFL lawsuits for longer periods of time. My main analyses thus focus on the period 1991 to 2004, the period following the crash of the junk bond market.

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. The law adoption increases the risk of PE buyouts being unwound, leading to the targets' selling shareholders and the secured lenders asking for higher takeover premiums and higher interest rates for the buyout loans, which in turn results in a reduction in PE buyout activity. My analysis focuses on whether secured lenders charge higher interest rates on PE buyout loans following the adoption of the CFL, since data on takeover premiums are available for only a fraction of the sample.

Following the CFL's adoption, the risk of debt-financed M&A deals being unwound increases if the target firms' assets were pledged as collateral for the acquisition loan. In PE buyouts, the PE firms use shell companies as vehicles for the buyouts, thus requiring the use of the target firms' assets to pledge as collateral for the loan. By contrast, the acquirers in non-PE buyout

M&A deals are often existing businesses with assets of their own, and are therefore less likely to have pledged the targets' assets as collateral for the acquisition loan, since such a move would increase the risk of the deal being unwound. I thus expect the law adoption to have a greater impact on PE buyout loans than for all other M&A loans. To test this, I estimate equation (3) separately for PE buyout loans and for all other M&A loans (i.e., non-PE buyout M&A loans), and then compare the coefficients on $CFL \times Post$ across the two regressions.

Table 2 presents the results. Column (1) shows that the spread of PE buyout loans increases by 51 basis points after the law's adoption, which corresponds to an increase of 19% relative to the pre-treatment sample mean. This is consistent with the prediction that PE buyout financing costs increase following the adoption of the CFL. In contrast, Column (2) shows that the law has no statistically significant impact on the spread of all other M&A loans (i.e., non-PE buyout M&A loans). In Columns (3) and (4), I show that the results are robust to controlling for loan characteristics. Further, the result from an F-test suggests that the difference between the coefficient estimates on $CFL \times Post$ for the two samples of loans (i.e., PE buyout loans and all other M&A loans) is statistically significant at the 1% level.

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 begin by investigating the impact of the CFL adoption on PE buyout activity at the state level. Specifically, I estimate equation (1), in which buyout activity is measured by the number of completed buyout deals. [13]

Table 3 presents the results. Column (1) shows that the coefficient estimate on $CFL \times Post$ is negative and statistically significant at the 1% level. The point estimate suggests that the number of PE buyouts decline by 2.9, representing a 71% decrease relative to the pre-treatment sample mean. Column (3) reports similar estimates when controlling for state-level characteristics. These findings are consistent with the prediction that the law adoption leads to a decline in PE buyout activity.

The difference-in-differences framework assumes that the outcomes of interest for the treat-

[13] The size of the buyout deal can also be used as a measure of PE buyout activity. However, since most deals are proprietary, the data on deal size is limited to a small subsample. A second alternative would be to use the dollar amount of PE buyout loans as a measure of PE buyout activity. However, this measure has a drawback: a reduction in PE buyout loan amounts could indicate a decline in PE buyout activity; it could also suggest that the acquirers are using less leverage in 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.

ment and control groups would trend in parallel prior to the treatment. The presence of a pre-treatment trend could thus 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 (1):

$$Y_{s,t,k} = \sum_{\tau=-5,\tau\neq-1}^{\tau=5} \beta_\tau \times CFL_{s,k} \times \mathbb{1}\{t=\tau\} + \alpha_{s,k} + \lambda_{t,k} + \theta_s \times t + \sum_{\tau=-5,\tau\neq-1}^{\tau=5} \gamma'_\tau \times X_{s,k} \times \mathbb{1}\{t=\tau\} + \varepsilon_{s,t,k} \quad (4)$$

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 adoption ($\tau = -1$) serves as the reference point and is thus excluded.

Columns (2) and (4) of Table 3 examine the dynamic impact of the CFL on the number of PE buyouts. In all periods before the law adoption (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 adoption. This finding suggest that 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, alleviating the concern that differential pre-treatment trends are driving the results. Figure 1 displays this finding graphically by plotting the point estimates and the 90% confidence intervals.

6.1.3. Firms' likelihood of becoming a PE buyout target

Thus 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).[14] The dependent variable, *Target*, is an indicator that equals one if firm i was acquired in a PE buyout in year t.

Table 4 presents the results. To improve readability, the coefficient estimates and standard

[14]One may question whether a linear probability model is an appropriate regression specification, as it could yield predicted probabilities that fall outside the range of zero and one. This issue does not arise if the independent variables are bounded by zero and one, as when equation (2) is estimated without including controls in Columns (1) and (3). As Table 4 shows that the results are robust to the inclusion of controls, the use of a linear probability model is unlikely to pose a problem. Further, untabulated tests indicate that the results are robust to estimating either a logit model or a hazard model.

errors are multiplied by 100. Column (1) and (2) control for industry fixed effects. The coefficient estimate on *CFL*×*Post* in Column (1) suggests that firms are 1.24% less likely to be targeted in a PE buyout following the law change. In Column (2), 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*×*LowTobinQ*, *LowOpinc*×*HighTobinQ*, and *HighHHI*×*LowTobinQ*. The coefficient estimate on *CFL*×*Post* in Column (2) is -1.37 and statistically significant at the 1% level, suggesting that the results are robust to including controls. Columns (3) and (4) replace industry fixed effects with firm fixed effects. Given that the unconditional probability of becoming a buyout target is 1.07%, the coefficient estimates on *CFL*×*Post* in Columns (3) and (4) imply a 68% to 70% reduction in the firms' likelihood of becoming a buyout target. Overall, 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

As states adopt the CFL, the cost of undertaking PE buyouts increases. Consequently, PE firms planning to invest in law-adopting states may respond to this increased cost by shifting their buyout activities from law-adopting states, where buyout costs are higher, to non-adopting states in pursuit of lower costs. 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 not possible to identify 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) or (2) have historically invested in eventually-treated states. 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, Kumar, and Sulaeman, 2015; Ellis, Madureira, and Underwood, 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.

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:

$$Y_{i,j,t,k} = \beta TreatedPE_{i,k} \times Post_{t,k} + \omega_{i,k} + \lambda_{t,k} + \varepsilon_{i,t,k}, \tag{5}$$

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 a reallocation of buyout activity to non-adopting states.

Table 5 presents the results. The coefficient estimate on the interaction term $TreatedPE \times Post$ is positive and statistically significant at the 5% level. The point estimate implies a 120% increase relative to the pre-treatment 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 magnitude of the estimated impact of the law in Tables 3 and 4 are overstated, since the difference-in-differences estimates capture the differences between increased activity in non-adopting states and reduced activity in law-adopting states.

6.1.5. Magnitude of the estimated impact of the CFL

To reduce biases resulting from the reallocation of buyout activity from states where the law is currently in effect to states where the law is not yet in effect, I restrict the control group to

include only already-treated states. The idea behind this approach is that the law is currently in effect in already-treated states, and therefore these states are unlikely to experience positive spillovers of buyout activity from the treatment states.

With the control group consisting only of already-treated states, the magnitude of the estimated impact of the law on buyout activity is expected to be smaller than what was previously estimated. The results of this re-estimation are presented in Table 6. Panel A presents the pre-treatment summary statistics for the sample under estimation. Column (1) in Panel B shows the impact of the law adoption on PE buyout activity, and Column (2) further controls for state-level characteristics. The coefficient estimates on $CFL \times Post$ in Columns (1) and (2) are both negative and statistically significant at the 5% level. The point estimates suggest that the number of PE buyouts decline by around 1.8, which represents a 42% decrease relative to the pre-treatment sample mean. Compared to Table 3, the magnitude of the estimated impact of the CFL presented here is significantly lower. Panel C shows the impact of the law adoption on firms' likelihood of becoming a buyout target, where the results are presented without firm-level controls in Column (1) and with controls in Column (2). In both columns, the coefficient estimates on $CFL \times Post$ are negative and statistically significant at the 5% level. The point estimates suggest that the firms are 0.7% less likely to become buyout targets, which represents a 69% decrease relative to the pre-treatment sample mean. The magnitude of the estimated impact of the CFL presented here is lower than that of Table 4, although not significantly so. Overall, these analyses provide a less biased estimate of the impact of the CFL. Further, the fact that the estimated impact of the law is smaller in this analyses confirms my previous finding that PE firms reallocate their investments in response to the adoption of the law.

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 investigate whether and how potential target firms respond to the reduction in PE buyout activity. Although the prediction is that the firms' managers will become more entrenched, the manner in which they would act against shareholders' interests is theoretically ambiguous.

Entrenched managers could behave in two main ways: First, they may take on projects with a negative NPV. 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. Another example is that managers could undertake acquisitions to reduce firm risk, even though they could lead to value destruction for their firms (e.g., Gormley and Matsa, 2016). Second, entrenched managers may not undertake all projects with a positive NPV. For instance, 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.

6.2.1. Firm investments, financing, and payouts

A key distinction between the two forms of managerial entrenchment behavior discussed above is the managers' investment decisions. In particular, entrenched managers may either take on projects with negative NPV, which implies a higher investment level, or they may not undertake all positive NPV projects, which implies a lower investment level. I thus begin by investigating how the firms alter their investment decisions in response to the law change. To do so, I estimate equation (2) and measure firm investments as the sum of capital expenditures and R&D expenses scaled by total assets. Column (1) in Panel A of Table 7 presents the result. The coefficient estimate on $CFL \times Post$ is negative and statistically significant. Although this finding indicates that the firms are making fewer investments, this does not necessarily imply that the managers are entrenched given the lack of information regarding the types of investments being cut back. Section 6.2.2 continues this investigation by exploring the valuation, performance, and risk implications of the law.

Since the firms appear to be making fewer investments following the law change, this could suggest that the firms need not raise as much capital as before. I thus investigate how the firms' financial policies have changed. Specifically, I examine the impact of the law change on the firms' new financing activity, which I define as the sum of net debt and equity issuances. The results are presented in Column (2) in Panel A of Table 7. The coefficient estimate on $CFL \times Post$ is negative and statistically significant, indicating that the firms are raising less capital. This finding also suggests that managers may refrain from raising capital if possible in order to avoid being monitored by capital markets when seeking external financing.

I next examine whether and how the firms' payout policies have changed as a result of the law change. Theories predict that entrenched managers reduce payouts to their shareholders. This tendency can be explained by the fact that payouts reduce firms' free cash flow, thereby limiting managers' ability to engage in wasteful spending (e.g., Jensen, 1986), or forcing them to raise external capital, which places them under increased scrutiny by capital markets (e.g., Rozeff, 1982; Easterbrook, 1984). Column (3) in Panel A of Table 7 reports the estimated impact of the law on firms' payouts, where payouts are defined as the sum of common dividends and share repurchases scaled by total assets. As the coefficient estimate on $CFL \times Post$ is statistically insignificant, there is neither evidence to support nor contradict the prediction that payouts reduce as managers become more entrenched.

6.2.2. Valuation, performance, and risk implications

The next step is to explore the valuation, performance, and risk implications of the law to determine whether the finding that firms are making less investments reflects the fact that managers are becoming more entrenched. As prior literature indicates that managerial entrenchment is negatively associated with firm value (e.g., Gompers, Ishii, and Metrick, 2003; Bebchuk, Cohen, and Ferrell, 2009), I begin by investigating whether firm value declines following the law adoption. I use Tobin's Q as a measure for firm value, where Tobin's Q is calculated as the market value of assets divided by the book value of assets. Column (1) in Panel B of Table 7 shows that Tobin's Q declines following the adoption of the CFL. In addition, an analysis of the cumulative abnormal returns around the effective date of the law, in Table 15, shows that the returns are negative on average. Both findings indicate that the law has a detrimental impact on firm valuation, suggesting a greater degree of managerial entrenchment as a result of the law change.

In Columns (2) and (3) in Panel B of Table 7, I examine the impact of the CFL on firm growth. Column (2) examines asset growth, which is measured as the year-over-year growth in firms' total assets. As shown, the coefficient estimates on $CFL \times Post$ is negative and statistically significant at the 1% level. Column (3) examine sales growth, which is measured as the year-over-year growth in firms' total revenue. The result shows that the coefficient estimates on $CFL \times Post$ is negative and statistically significant at the 5% level. 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.

I next examine the impact of the CFL on firm performance, using ROA as a measure of firm performance. Column (4) in Panel B of Table 7 shows that the coefficient estimate on *CFL*×*Post* is positive and statistically significant at the 1% level. The finding of an increase in ROA following the law change contrasts with Bertrand and Mullainathan's (2003), who show that firms become less profitable as managers enjoy a quiet life when the threat of the firm being taken over reduces. Yet, the finding of improved firm performance is not necessarily in contradiction with the quiet life hypothesis, 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 is also indicative of entrenched managers enjoying a quiet life.

To investigate the changes in managers' risk-taking behavior, I also examine the impact of the law on firm risk, using two measures to capture the different aspects of firm risk. Columns (1) and (2) in Panel C of Table 7 presents the results. Column (1) examines stock return volatility, which is a summary measure of firm risk that reflects the financial and non-financial aspects of risk. The result indicates that there is no evidence that the overall risk of firms have changed. Column (2) examines operational risk, which is measured by cash flow volatility (e.g., Gormley and Matsa, 2016). As shown, the coefficient estimates on *CFL*×*Post* is negative and statistically significant at the 5% 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 suggest that managers are exerting less effort, consistent with managers enjoying a quiet life.

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, Francis et al., 2010), which adversely affects debtholders by increasing the agency costs of debt. Since the CFL induces managerial entrenchment, the law 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. Columns (3) and (4) in Panel C of Table 7 presents the results. Column (3) shows that firms are around 0.3% less likely to default on their debt, with a statistical significance at the 5% level. Similarly, the coefficient estimate on *CFL*×*Post* in Column (4) reveals that firms are around 0.3% less likely to file for bankruptcy. Overall, these findings are consistent with the view that managerial entrenchment reduces risk-shifting from shareholders to debtholders.

6.2.3. Firms' likelihood of relocating their state of headquarters

Thus far, the evidence suggests that the CFL reduces the likelihood of firms becoming buyout targets, thereby resulting in the firms' managers slacking off. I next investigate whether the states' adoption of the CFL affects firms decision regarding the location of their headquarters. Specifically, I examine whether firms are more or less likely to move to non-law adopting states after the states in which they are headquartered in adopts the law. To do so, I estimate equation (2), where the dependent variable is *Move-to-nonlaw*. *Move-to-nonlaw* is an indicator that equals one if firm i relocated their headquarters to a non-law adopting state in a given year t.

Table 8 presents the results. To improve readability, the coefficient estimates and standard errors are multiplied by 100. The coefficient estimate on *CFL*×*Post* in Column (1) is -1.8, and the estimate is statistically significant at the 1% level. Column (2) reports similar estimates when controlling for firm-level characteristics. These findings suggest that, after the firms' state of headquarters adopt the law, firms are less likely to relocate their headquarters to another state. Firms' decision to remain within law-adopting states potentially reflects their managers' desire to slack off.

6.3. Robustness tests

6.3.1. Alternative explanation: increased financial constraints

In addition to a reduced likelihood of becoming buyout targets, there may be alternative explanations for the firms' reduced investments following the adoption of the law. A potential explanation may be that the law adoption resulted in the firms becoming more financially constrained. Specifically, the CFL may increase the firms' financial constraints in two ways. First, because PE buyouts may be a way for firms to obtain capital and alleviate credit constraints

(Boucly, Sraer, and Thesmar, 2011), the reduced likelihood of becoming a buyout target following the adoption of the law may have resulted in firms becoming more financially constrained. Second, 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. As a result, managers may be less inclined to take on unsecured debt due to the increased litigation risk posed by unsecured creditors. Firms' reduction in investments after the law adoption may therefore reflect the firms' increased financial constraints caused by managers' avoidance of unsecured debt.

I provide the following discussion and analyses to alleviate these concerns. First, to reduce the possibility that my findings are driven by the firms becoming financially constrained, I restrict my analyses to public firms since they are less likely to be financially constrained than private firms (e.g., Saunders and Steffen, 2011; Fleckenstein, Longstaff, and Strebulaev, 2020). In addition, as public firms have access to public capital markets, PE buyouts typically serve to restructure them towards a more efficient corporate structure, rather than providing them with capital as is often the case with private firms. Thus, restricting the analyses to public firms also alleviates concerns regarding the possibility that the lack of buyouts may have resulted in increased financial constraints among the sample firms.

Second, I investigate whether the firms adjust their use of unsecured debt in response to stronger rights for unsecured creditors. Specifically, I examine the impact of the CFL on firms' debt composition in terms of unsecured and secured debt. Following Benmelech, Kumar, and Rajan (2020) and Giambona, Golec, and Lopez-de Silanes (2021), I compute the share of unsecured debt in total debt as one minus secured debt, divided by the sum of book value of total long-term and short-term debt. For robustness, I also measure firms' debt composition as unsecured debt scaled by total assets. The results, presented in Table 10, suggest that there is no evidence of a change in firms' debt composition, alleviating the concern firms' reduced use of unsecured debt are driving my results.

Finally, I conduct a placebo test in which my analyses are based on the period preceding the collapse of the junk bond market. As discussed in Section 5.2, the impact of the law adoption on PE buyouts is predicted to be more pronounced in the period following the collapse of the junk bond market than in the period preceding it. The absence of such findings could indicate that my results are driven by factors other than an increase in buyout costs. Panel A of Table

11 presents the results of the impact of the CFL on PE buyout activity at the state level. Since the coefficient estimate on $CFL \times Post$ is statistically insignificant, there is no evidence to suggest that the law adoption had an impact on buyout activity. Panel B of Table 11 presents the results of the impact of the CFL on firms' likelihood of becoming buyout targets, where Column (1) controls for industry fixed effects and Column (2) controls for firm fixed effects. The coefficient estimates on $CFL \times Post$ are statistically insignificant in both columns, indicating that the law adoption had no significant impact on firms' likelihood of being bought out. It is reassuring to see that neither the state-level nor the firm-level results in Table 11 suggest that the law adoption had a greater impact on PE buyouts prior to the junk bond market crash. I then investigate how firms respond to the adoption of the CFL in the period prior to the junk bond market crash. The results in Table 12 suggest no evidence of a significant impact of the law adoption on firm outcomes. This result, along with the fact that there is no evidence that the law adoption had a significant impact on buyout activity during the pre-junk bond market crash period, suggest that my findings are unlikely to have captured firms' responses to factors other than the reduction in their likelihood of becoming buyout targets.

6.3.2. Robustness to alternative samples and specifications

In this section, I show that my results are robust to alternative samples and specifications. In my main analysis, firms that relocated their headquarters to another state in the five years before the CFL was adopted in their original state of headquarters were excluded from the sample. The purpose of this is to address the concern that firms' endogenous choice of their state of headquarters could potentially confound my analysis. However, this approach has the drawback of being unable to identify which of the firms choose to remain in their state of headquarters because they consider the state's law to be favorable. Therefore, it remains possible that my findings could be confounded by firms' decision of where to locate their state of headquarters. Addressing this issue is challenging. Yet, the fact that the law does not appear to have an impact on buyout activity before its adoption reduces the possibility that firms may have anticipated the law adoption and accordingly made their decision regarding the location of their headquarters. In untabulated tests, I find that my results remain robust when I include firms that relocated their state of headquarters in my sample.

Panel A of Table 13 shows that my state-level analysis is robust to alternative samples and specifications. In particular, the results in Columns (1) and (2) suggest that my results are

robust to excluding the period following the recession in the early 2000s. Columns (3) and (4) suggest that my results are robust to the control group excluding already-treated states. Columns (5) and (6) suggest that my results remain robust when the dependent variable is log-transformed. Similarly, Panel B of Table 13 shows that my firm-level analysis remains robust when the period following the recession in the early 2000s is excluded, as well as when already-treated states are excluded from the control group.

6.3.3. Endogenous to state-specific economic conditions?

A potential concern is that the adoption of the CFL may be 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 slowdown in the economy may have contributed in part to the reduction in PE buyout activity. To address 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 14 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.

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 the reduced likelihood of becoming buyout targets. I find that the firms cherry-pick positive NPV projects with low risk, consistent with the firms' managers enjoying a quiet life. I also find that the firms become less likely to default on their debt or go bankrupt, consistent with a lower risk-taking by quiet-life managers. Further analyses suggest that firms are less likely to relocate their state of headquarters after the the law is adopted by the state in which they are based. Together, an implication of these findings is that, while the law adoption may have benefited states in terms of retaining firms, the firms' managers in such states slack off and enjoy a quiet life.

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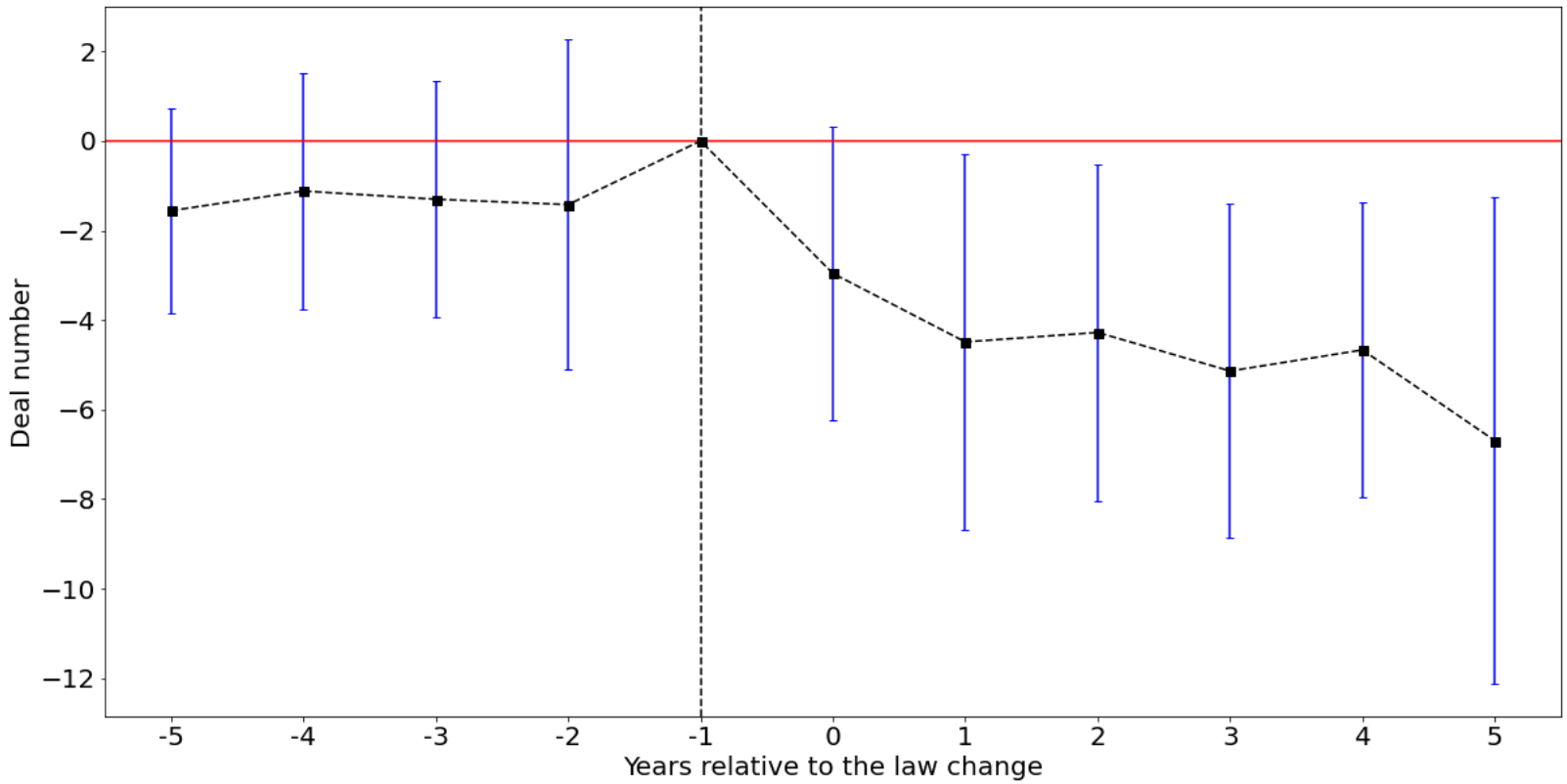

Figure 1: Timing of changes in PE buyout activity around the adoption of the CFL

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: $Y_{s,t,k} = \sum_{\tau=-5,\tau\neq-1}^{\tau=5} \beta_\tau \times CFL_{s,k} \times \mathbb{1}\{t=\tau\} + \alpha_{s,k} + \lambda_{t,k} + \theta_s \times t + \sum_{\tau=-5,\tau\neq-1}^{\tau=5} \gamma_\tau \times X'_{s,k} \times \mathbb{1}\{t=\tau\} + \varepsilon_{s,t,k}$, 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 that equals one if state s is in the treatment group, that is, the state in which the law will be adopted in event k. 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:	Obs.	Mean	SD	Min	p10	p50	p90	Max
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: State-year level variables								
Deal number	820	4.059	6.098	0.000	0.000	2.000	11.000	43.000
Panel B: Firm-year level variables								
Investments	77,300	0.117	0.126	0.000	0.016	0.079	0.262	1.138
Payouts	73,334	0.018	0.041	0.000	0.000	0.000	0.052	0.417
New financing	71,873	0.158	0.606	-0.429	-0.083	0.012	0.441	11.705
Asset growth	77,525	0.216	0.725	-0.727	-0.185	0.070	0.625	11.524
Sales growth	76,981	0.242	0.756	-0.969	-0.169	0.097	0.626	8.859
Tobin's Q	76,128	2.103	1.939	0.527	0.904	1.480	3.874	24.361
ROA	76,307	0.013	0.110	-0.911	-0.112	0.046	0.098	0.176
Stock volatility	66,901	0.711	0.457	0.155	0.273	0.610	1.246	3.165
Cash flow volatility	52,879	0.124	0.117	0.018	0.038	0.088	0.242	1.033
Reallocation × 100	21,681	0.074	2.716	0.000	0.000	0.000	0.000	100.000
Target × 100	78,136	1.065	10.264	0.000	0.000	0.000	0.000	100.000
Default × 100	78,136	0.283	5.311	0.000	0.000	0.000	0.000	100.000
Bankruptcy × 100	78,136	0.166	4.076	0.000	0.000	0.000	0.000	100.000
Move-to-nonlaw × 100	78,136	0.020	1.431	0.000	0.000	0.000	0.000	100.000
Panel C: Loan-year level variables								
Spread (PE buyouts)	12,644	261.688	58.202	87.500	200.000	250.000	300.000	555.000
Spread (Other M&A)	66,527	220.245	82.353	17.000	125.000	225.000	275.000	555.000

This table presents summary statistics for the main variables in the pre-treatment period. 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 1991-2004 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 U.S. dollar-denominated loans made to U.S. firms from Dealscan for the 1991-2004 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 loan spreads

Dependent variable:	Spread (bps)		Spread (bps)	
Loan purpose:	PE buyout	Other M&A	PE buyout	Other M&A
	(1)	(2)	(3)	(4)
CFL × Post	50.823***	17.849	96.074***	8.683
	(10.060)	(15.427)	(15.088)	(12.456)
Event-specific year fixed effects	Yes	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes	Yes
Loan controls	No	No	Yes	Yes
Observations	89,814	356,672	89,435	355,475
Adjusted R^2	0.304	0.266	0.541	0.536
F-test (F-stat., p-value)	(3.28, 0.077)		(17.32, 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 M&A deals. The sample includes loan-year observations from 1991 to 2004. The dependent variable, *Spread*, is the all-in spread drawn. CFL is an indicator that equals one if state s is in the treatment group, that is, the state in which the law will be adopted in event k. *Post* is an indicator that equals one if year t is in the year of the law's adoption or in the period following its adoption. An F-test is used to determine whether there is a statistical difference between the coefficient estimates of the CFL between the two subsamples (i.e., PE buyout loans versus all other M&A 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 3: Constructive Fraud Provision (CFL) and state-level PE buyout activity

Dependent variable:	Deal number			
	(1)	(2)	(3)	(4)
CFL × Post	-2.863***		-2.993***	
	(1.047)		(1.068)	
CFL × $\mathbb{1}\{t = -5\}$		-0.923		-1.558
		(1.536)		(1.386)
CFL × $\mathbb{1}\{t = -4\}$		-1.335		-1.119
		(1.176)		(1.603)
CFL × $\mathbb{1}\{t = -3\}$		-1.282		-1.303
		(1.302)		(1.605)
CFL × $\mathbb{1}\{t = -2\}$		-1.323		-1.423
		(2.101)		(2.243)
CFL × $\mathbb{1}\{t = 0\}$		-2.769		-2.959
		(1.725)		(1.989)
CFL × $\mathbb{1}\{t = 1\}$		-4.495**		-4.489*
		(1.955)		(2.549)
CFL × $\mathbb{1}\{t = 2\}$		-4.056**		-4.278*
		(1.935)		(2.285)
CFL × $\mathbb{1}\{t = 3\}$		-4.839**		-5.138**
		(1.936)		(2.265)
CFL × $\mathbb{1}\{t = 4\}$		-4.878***		-4.668**
		(1.432)		(2.004)
CFL × $\mathbb{1}\{t = 5\}$		-6.701**		-6.694**
		(2.728)		(3.306)
Event-specific year fixed effects	Yes	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes	Yes
State-specific time trend	Yes	Yes	Yes	Yes
Pre-treatment controls (interacted)	No	No	Yes	Yes
Observations	2,130	2,130	2,130	2,130
Adjusted R^2	0.882	0.882	0.855	0.864

This table presents difference-in-differences estimates of the impact of the CFL on PE buyout activities at the state-level. The sample includes state-year observations from 1991 to 2004. 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 is in the treatment group, that is, the state in which the law will be adopted in event k. *Post* is an indicator that equals one if year t is in the year of the law's adoption or in the period following its adoption. $\mathbb{1}\{t = \tau\}$ is an indicator for τ years relative to the adoption of the CFL. 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: Constructive Fraud Provision (CFL) and firms' likelihood of becoming a PE buyout target

Dependent variable:	Target			
	(1)	(2)	(3)	(4)
CFL × Post	-1.242***	-1.373***	-0.728**	-0.749**
	(0.460)	(0.415)	(0.330)	(0.323)
Event-specific year fixed effects	Yes	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes	Yes
Event-specific industry fixed effects	Yes	Yes	No	No
Event-specific firm fixed effects	No	No	Yes	Yes
Pre-treatment controls (interacted)	No	Yes	No	Yes
Observations	193,556	193,556	187,648	187,648
Adjusted R^2	0.004	0.009	0.045	0.045

This table presents the difference-in-differences estimates of the impact of the CFL on firms' likelihood of becoming a PE buyout target. The sample includes firm-year observations from 1991 to 2004. 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 is in the treatment group, that is, the state in which the law will be adopted in event k. *Post* is an indicator that equals one if year t is in the year of the law's adoption or in the period following its adoption. 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 5: Reallocation of PE buyout activity

Dependent variable:	Reallocation
	(1)
TreatPE × Post	0.206**
	(0.084)
Event-specific year fixed effects	Yes
Event-specific firm fixed effects	Yes
Observations	58,667
Adjusted R^2	0.067

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 (5) on the sample of PE buyout activity in non-adopting states. The sample includes firm-year observations from 1991 to 2004. 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. *TreatPE* 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 of the law's adoption or in the period following its adoption. The coefficient estimates and standard errors are multiplied by 100 to improve readability. 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 6: Control group consists only of already-treated states

Panel A: Summary statistics (Pre-treatment)						
Variable:	Obs.	Mean	SD	Min	p50	Max
	(1)	(2)	(3)	(4)	(5)	(6)
Deal number	779	4.261	6.190	0.000	2.000	43.000
Target × 100	77,774	1.066	10.269	0.000	0.000	100.000

Panel B: CFL and state-level PE buyout activity		
Dependent variable:	Deal number	
	(1)	(2)
CFL × Post	-1.796**	-1.873**
	(0.871)	(0.902)
Event-specific year fixed effects	Yes	Yes
Event-specific state fixed effects	Yes	Yes
State-specific time trend	Yes	Yes
Pre-treatment controls (interacted)	No	Yes
Observations	2,029	2,029
Adjusted R^2	0.881	0.855

Panel C: CFL and firm's likelihood of becoming a PE buyout target		
Dependent variable:	Target	
	(1)	(2)
CFL × Post	-0.717**	-0.738**
	(0.328)	(0.322)
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	186,759	186,759
Adjusted R^2	0.046	0.046

This table presents difference-in-differences estimates of the impact of the CFL on PE buyout activity, with the control group consists only of already-treated states. Panel A presents summary statistics for the dependent variables in the pre-treatment period. Panel B presents the difference-in-differences estimates of the impact of the CFL on PE buyout activity at the state level. The sample includes state-year observations from 1991 to 2004. The dependent variable is the number of PE buyout deals completed in state s in year t. Panel C presents the difference-in-differences estimates of the impact of the CFL on the likelihood of becoming a PE buyout target. The sample includes firm-year observations from 1991 to 2004. 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. The coefficient estimates and standard errors are multiplied by 100 to improve readability. For Panels B and C, CFL is an indicator that equals one if state s is in the treatment group, that is, the state in which the law will be adopted in event k. *Post* is an indicator that equals one if year t is in the year of the law's adoption or in the period following its adoption. 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 7: Constructive Fraud Provision (CFL) and firm-level outcomes

Panel A: Firm policies				
Dependent variable:	Investments	New financing	Payouts	
	(1)	(2)	(3)	
CFL × Post	-2.162*	-9.595*	0.429	
	(1.203)	(5.160)	(0.991)	
Event-specific year fixed effects	Yes	Yes	Yes	
Event-specific state fixed effects	Yes	Yes	Yes	
Event-specific firm fixed effects	Yes	Yes	Yes	
Pre-treatment controls (interacted)	Yes	Yes	Yes	
Observations	152,230	139,242	128,423	
Adjusted R^2	0.649	0.181	0.387	
Panel B: Firm growth, valuation, and performance				
Dependent variable:	Tobin's Q	Asset growth	Sales growth	ROA
	(1)	(2)	(3)	(4)
CFL × Post	-15.851*	-6.370**	-4.959**	1.177***
	(8.541)	(3.013)	(2.458)	(0.427)
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	153,097	153,711	152,787	152,797
Adjusted R^2	0.414	0.091	0.118	0.475
Panel C: Firm risk and the likelihood of default or bankruptcy				
Dependent variable:	Stock volat.	Cash flow volat.	Default	Bankruptcy
	(1)	(2)	(3)	(4)
CFL × Post	1.236	-1.995**	-0.305*	-0.296***
	(1.475)	(0.781)	(0.152)	(0.099)
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	147,852	114,224	153,973	153,973
Adjusted R^2	0.596	0.516	0.052	0.049

This table presents the difference-in-differences estimates of the impact of the CFL on firms-level outcomes. The sample includes firm-year observations from 1991 to 2004. CFL is an indicator that equals one if state s is in the treatment group, that is, the state in which the law will be adopted in event k. *Post* is an indicator that equals one if year t is in the year of the law's adoption or in the period following its adoption. 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 8: Firms' likelihood of relocating their state of headquarters

Dependent variable:	Move to non-law	
	(1)	(2)
CFL × Post	-1.751***	-1.748***
	(0.303)	(0.301)
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	160,593	160,593
Adjusted R^2	0.121	0.121

This table presents the difference-in-differences estimates of the impact of the CFL on firms' likelihood of relocating their state of headquarters. The sample includes firm-year observations from 1991 to 2004. The dependent variable is *Move-to-nonlaw*, which is an indicator equal to one if a firm relocated their headquarters to a non-law adopting state in a given year. CFL is an indicator that equals one if state s is in the treatment group, that is, the state in which the law will be adopted in event k. *Post* is an indicator that equals one if year t is in the year of the law's adoption or in the period following its adoption. 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: Do firms change their composition of debt?

Dependent variable:	Unsecured debt/Debt	Unsecured debt/Assets
	(1)	(2)
CFL × Post	5.057	1.107
	(3.518)	(1.127)
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	124,448	145,284
Adjusted R^2	0.537	0.454

This table presents the difference-in-differences estimates of the impact of the CFL on firms' debt composition. The sample includes firm-year observations from 1991 to 2004. *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 is in the treatment group, that is, the state in which the law will be adopted in event k. *Post* is an indicator that equals one if year t is in the year of the law's adoption or in the period following its adoption. 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: Placebo tests: Sample period 1976-1990

Panel A: CFL and state-level PE buyout activity		
Dependent variable:	Deal number	
	(1)	(2)
CFL × Post	-0.084	
	(0.941)	
CFL × $\mathbb{1}\{t = -5\}$		-1.476
		(1.154)
CFL × $\mathbb{1}\{t = -4\}$		-0.864
		(0.897)
CFL × $\mathbb{1}\{t = -3\}$		0.150
		(0.681)
CFL × $\mathbb{1}\{t = -2\}$		-0.982
		(1.073)
CFL × $\mathbb{1}\{t = 0\}$		0.049
		(1.116)
CFL × $\mathbb{1}\{t = 1\}$		1.904**
		(0.931)
CFL × $\mathbb{1}\{t = 2\}$		-0.448
		(0.888)
CFL × $\mathbb{1}\{t = 3\}$		0.393
		(0.899)
CFL × $\mathbb{1}\{t = 4\}$		2.421*
		(1.295)
CFL × $\mathbb{1}\{t = 5\}$		6.091***
		(2.009)
Event-specific year fixed effects	Yes	Yes
Event-specific state fixed effects	Yes	Yes
State-specific time trend	Yes	Yes
Pre-treatment controls (interacted)	Yes	Yes
Observations	2,177	2,177
Adjusted R^2	0.840	0.869
Panel B: CFL and firms' likelihood of becoming a PE buyout target		
Dependent variable:	Target	
	(1)	(2)
CFL × Post	-0.546	-0.946
	(0.919)	(1.038)
Event-specific year fixed effects	Yes	Yes
Event-specific state fixed effects	Yes	Yes
Event-specific industry fixed effects	Yes	No
Event-specific firm fixed effects	No	Yes
Pre-treatment controls (interacted)	Yes	Yes
Observations	174,035	169,362
Adjusted R^2	0.024	0.084

This table presents difference-in-differences estimates of the impact of the CFL on PE buyout activity, where the sample period is 1976 to 1990. CFL is an indicator that equals one if state s is in the treatment group, that is, the state in which the law will be adopted in event k. *Post* is an indicator that equals one if year t is in the year of the law's adoption or in the period following its adoption. $\mathbb{1}\{t = \tau\}$ is an indicator for τ years relative to the adoption of the CFL. 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 11: Placebo tests: Sample period 1976-1990

Panel A: Firm policies			
Dependent variable:	Investments	Payouts	New financing
	(1)	(2)	(3)
CFL × Post	-0.086	-0.461	-0.627
	(2.453)	(0.338)	(1.627)
Event-specific year fixed effects	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes
Event-specific firm fixed effects	Yes	Yes	Yes
Pre-treatment controls (interacted)	Yes	Yes	Yes
Observations	147,691	144,217	140,467
Adjusted R^2	0.584	0.349	0.167

Panel B: Firm growth, valuation, and performance				
Dependent variable:	Asset growth	Sales growth	Tobin's Q	ROA
	(1)	(2)	(3)	(4)
CFL × Post	0.693	0.232	-2.047	-0.649
	(2.606)	(2.780)	(5.020)	(1.037)
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	147,691	147,691	147,379	147,363
Adjusted R^2	0.133	0.180	0.634	0.530

Panel C: Firm risk and the likelihood of default or bankruptcy				
Dependent variable:	Stock volat.	Cash flow volat.	Default	Bankruptcy
	(1)	(2)	(3)	(4)
CFL × Post	-0.545	1.505	0.947	0.198
	(1.644)	(1.651)	(1.073)	(0.351)
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	146,905	78,413	147,691	147,691
Adjusted R^2	0.506	0.750	0.022	0.034

This table presents the difference-in-differences estimates of the impact of the CFL on firms-level outcomes, where the sample period is 1976 to 1990. CFL is an indicator that equals one if state s is in the treatment group, that is, the state in which the law will be adopted in event k. *Post* is an indicator that equals one if year t is in the year of the law's adoption or in the period following its adoption. 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 12: Robustness tests: Constructive Fraud Provision (CFL) and PE buyout activity

Panel A: CFL and state-level PE buyout activity						
Robustness:	1991-2000		Excl. already-treated		log(deal number)	
	(1)	(2)	(3)	(4)	(5)	(6)
CFL × Post	-2.047***		-2.038***		-1.003***	
	(0.496)		(0.455)		(0.176)	
CFL × $\mathbb{1}\{t=-5\}$		-0.098		-1.346		-0.602
		(0.908)		(0.940)		(-0.448)
CFL × $\mathbb{1}\{t=-4\}$		-0.222		-0.726		-0.260
		(1.039)		(0.912)		(0.361)
CFL × $\mathbb{1}\{t=-3\}$		-0.092		-1.032		-0.272
		(0.714)		(1.009)		(0.388)
CFL × $\mathbb{1}\{t=-2\}$		-0.814		-0.731		-0.208
		(1.283)		(1.230)		(0.669)
CFL × $\mathbb{1}\{t=0\}$		-2.213***		-2.111**		-0.861*
		(0.974)		(0.962)		(0.472)
CFL × $\mathbb{1}\{t=1\}$		-2.950**		-2.787**		-1.346**
		(1.107)		(1.044)		(0.527)
CFL × $\mathbb{1}\{t=2\}$		-3.055***		-3.487**		-1.459**
		(1.026)		(1.375)		(0.643)
CFL × $\mathbb{1}\{t=3\}$		-3.717***		-4.176***		-1.714***
		(0.962)		(1.491)		(0.634)
CFL × $\mathbb{1}\{t=4\}$		-3.285***		-2.848**		-1.226***
		(1.130)		(1.130)		(0.425)
CFL × $\mathbb{1}\{t=5\}$		-5.084***		-4.464***		-1.816***
		(1.396)		(1.120)		(0.604)
Event-specific year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Event-specific state fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State-specific time trend	Yes	Yes	Yes	Yes	Yes	Yes
Pre-treatment controls (interacted)	Yes	Yes	No	No	Yes	Yes
Observations	1,797	1,797	164	164	2,130	2,130
Adjusted R^2	0.824	0.842	0.614	0.669	0.874	0.883

Panel B: CFL and firms' likelihood of becoming a PE buyout target		
Robustness:	1991-2000	Excl. already-treated
	(1)	(2)
CFL × Post	-0.955***	-2.744*
	(0.293)	(1.522)
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	167,252	2,005
Adjusted R^2	0.041	0.183

This table presents the difference-in-differences estimates of the impact of the CFL on PE buyout activity. CFL is an indicator that equals one if state s is in the treatment group, that is, the state in which the law will be adopted in event k. *Post* is an indicator that equals one if year t is in the year of the law's adoption or in the period following its adoption. 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 13: 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^2	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. The sample includes state-year observations from 1976 to 2004. CFL is an indicator that equals one if state s is in the treatment group, that is, the state in which the law will be adopted in event k. $\mathbb{1}\{t = \tau\}$ is an indicator for τ years relative to the adoption of the CFL. 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 14: Constructive Fraud Provision (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.

Appendix

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.
Cash flow/Assets	Operating income after depreciation (OIADP) minus accruals [$(\text{ACT}_t - \text{ACT}_{t-1}) - (\text{CHE}_t - \text{CHE}_{t-1}) - (\text{LCT}_t - \text{LCT}_{t-1}) + (\text{DLC}_t - \text{DLC}_{t-1}) - \text{DP}_t$] 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 is in the treatment group, that is, the state in which the law will be adopted in event k.
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.
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.
Investment/Assets	The sum of capital expenditures (CAPX) and research and development expenditures (XRD) divided by book value of total assets (AT). Source: Compustat.
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(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.

Variable	Definition
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.
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.
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.
Performance pricing	Indicator that equals one if the loan has performance pricing provisions and zero otherwise. Source: Dealscan.
Post	Indicator that equals one if year t is in the year of the law's adoption or in the period following its adoption.
PPE growth	Net property, plant, and equipment (PPENT) divided by net PP&E in the previous year minus one. 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.
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.

Variable	Definition
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 secured 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

$$(\gamma - c)(1 - p)q + \gamma[1 - (1 - p)q] \geq 0. \tag{6}$$

The above condition can be rewritten as:

$$\gamma \geq cq(1 - p) = \gamma_{min}(c, p, q), \tag{7}$$

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:

$$\frac{\partial \gamma_{min}(c, p, q)}{\partial q} = c(1 - p) > 0, \tag{8}$$

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:

$$S = pR + q(1-p)(-L) - \gamma_{min}(c,p,q)I > 0. \tag{9}$$

Differentiating S with respect to q gives

$$\frac{\partial S}{\partial q} = -(L+c)(1-p) < 0, \tag{10}$$

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 declines following the adoption of the CFL.