How Do Firms Respond to Reduced Private Equity Buyout Activity?*

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Abstract

I investigate how firms respond to reduced private equity buyout activity in their home states by exploiting the state-by-state adoption of laws that increase the cost of buyouts. I find evidence suggestive of reduced firm efficiency. The firms also become less likely to go bankrupt or to relocate to other states. These findings suggest that the firms' managers may be starting to enjoy the quiet life. I also find a reduction in layoffs. Overall, the results indicate a trade-off: Whereas reduced private equity buyout activity increases managerial entrenchment, it also lowers firm exits and strengthens employees' job security.

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"Out of state, private equity firms have shut down Wisconsin manufacturing plants and stores and laid off our workers in Janesville, Waukesha, and Green Bay. We need to rip up the predatory playbook that these private equity firms are using to leave workers with nothing but pink slips."

—U.S. Senator Tammy Baldwin, 2021

1 Introduction

With private equity (PE) expanding into new business sectors and gaining market share, concerns about PE firms profiting at the expense of their targets and their targets' stakeholders have resurfaced. Recent evidence from the healthcare sector, for example, suggests that PE acquisitions of nursing homes may adversely affect patients' health and increase mortality rates, possibly due to reduced nursing staff as PE firms attempt to cut costs (e.g., Gupta et al. 2024).

In response to these trends, policymakers seek to regulate PE buyouts. Senator Elizabeth Warren, for instance, introduced the Stop Wall Street Looting Act, which, if implemented, could make PE buyouts less appealing to PE investors and potentially lead to a decline in buyout activity.[1]

This paper studies the potential consequences of PE buyout activity. It does so by exploiting state-level variation in PE buyout activity in a difference-in-differences framework. The findings suggest a potential trade-off in restricting PE buyout activity: Whereas lower PE buyout activity reduces firm exits and strengthens employees' job security, it also increases managerial entrenchment and leads to lower investment.

The variation in PE buyout activity that I exploit stems from the state-by-state adoption of constructive fraud provisions (CFP) in fraudulent transfer laws. The CFP was introduced to help creditors recover defaulted debt repayments in cases where debtors remove assets from their possession shortly before default or bankruptcy. Specifically, if debtors transfer their assets to other parties without receiving a reasonably equivalent payment in return and later default on their debts, the unpaid creditors may have legal grounds to challenge these transfers as fraudulent. In the case an

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[1]The Stop Wall Street Looting Act has been introduced twice in Congress to date. For details, see S.2155 at https://www.congress.gov/bill/116th-congress/senate-bill/2155 and H.R.5648 at https://www.congress.gov/bill/117th-congress/house-bill/5648.

asset transfer is deemed fraudulent, a court may order the recovery of the transferred assets from the transferees to force repayment to the creditors.

While PE buyouts are typically not seen as fraudulent, they may still meet the criteria for fraudulent asset transfers under the CFP if the buyout target later goes bankrupt.[2] The reason is as follows: In a PE buyout, the PE firm raises debt through a shell company to acquire the target's shares. Thereafter, the shell company merges with the target, and the target's assets are pledged as collateral for the loans obtained by the shell company to finance the buyout. The buyout thus boils down to the target taking on debt to buy back its own shares. These shares, however, are considered to have no value to the target in a legal sense, and the target is thus deemed to have received nothing in exchange for its newly incurred buyout debt. If the target later defaults on debts owed to its initial unsecured creditors (i.e., those whose claims were established before the buyout), these creditors may then challenge the buyout as a fraudulent asset transfer under the CFP.

In the case the buyout is indeed ruled fraudulent, repayments of debt will be ordered to the target's initial unsecured creditors. These repayments are made possible by recovering the funds paid in the buyout deal to the target's selling shareholders and by removing the lien placed on the target's assets by the secured lenders who financed the buyout. Hence, as the target's selling shareholders and the secured lenders of the buyout loans may incur losses ex post if a buyout is subsequently deemed fraudulent, they are likely to demand a higher takeover premium and a higher interest rate on the buyout loan ex ante. The cost of undertaking PE buyouts thus increases with the adoption of the CFP, and PE buyout activity is predicted to decline.[3]

My analyses proceed in two steps. In the first part of the paper, I begin by examining whether the adoption of the CFP indeed increases the cost of PE buyouts. Using

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[2]In the Internet Appendix, Section IA, I discuss a leading example of a PE buyout deemed fraudulent under the CFP, the *Gleneagles* case, and also provide several additional examples. In Section 2.2, I discuss in detail why PE buyouts may be deemed fraudulent under the CFP and also explain how the CFP affects various types of creditors involved in PE buyouts and how these effects lead to an increase in the cost of buyouts. In Section 2.3, I present evidence suggesting that the CFP indeed pose a significant risk of buyouts being deemed fraudulent transfers.

[3]Empirical evidence in support of this prediction is presented in Section 5.

a difference-in-differences approach, I find that the yield spread of secured PE buyout loans increases by about one percentage point following CFP adoption. The magnitude of this impact is economically significant, equivalent to almost one-and-a-half times the sample standard deviation. In contrast, the yield spread of secured loans used to finance other M&A deals (i.e., non-PE buyout deals) does not change.

Next, I examine the impact of the CFP on PE buyout activity itself. At the state level, I estimate that the adoption of the CFP leads to a decline of about one buyout deal per ten thousand firms, consistent with the CFP increasing the cost of a buyout. The magnitude of this decline is economically significant, approximately twice the sample standard deviation. At the firm level, I find that firms in states that adopt the CFP become less likely to be targeted for PE buyouts, with the magnitude of this decline being around one-tenth of the sample standard deviation.

I find that two additional factors may contribute to the estimated decline in buyout activity. First, there appear to be spillovers of buyout activity from CFP-adopting states to non-adopting states as PE firms relocate their buyout investments to states that have not yet adopted the CFP. Indeed, when limiting the control group to states that are less likely to be affected by such spillovers, the estimated effect of the CFP on firms' probability of becoming a PE buyout target declines to less than one-tenth of the sample standard deviation.

The other potential factor is reduced firm creation. Ersahin, Irani, and Waldock (2020) show that, for small businesses, the CFP may increase the costs associated with business failure, which could reduce entrepreneurs' incentives to start new businesses and, consequently, lead to fewer potential targets for PE buyouts. Consistent with this idea, I find that the magnitude of the estimated decline in buyout activity becomes smaller when focusing on a sample that is less likely to be affected by reduced firm creation—specifically, when focusing on buyouts of public targets.

In the second part of the paper, I examine the potential consequences of PE buyout activity by addressing two main questions: How do firms respond to a reduced likelihood of becoming a PE buyout target? And what are the implications of reduced PE

buyout activity for employment?

I document three sets of findings. First, I find that firms invest less in response to a reduced likelihood of becoming a PE buyout target. Further, I estimate that a 1% reduction in investment is associated with a 0.4% decrease in cash flow volatility and a 0.2% decrease in the likelihood of bankruptcy. These findings suggest that the firms may be forgoing risky investment projects.

I also find evidence of an increase in firms' profitability, as measured by ROA and ROE. Further analyses suggest that the higher ROA and ROE are likely due to the average investment project becoming more profitable and an increasing trend in share repurchases. Collectively, these results suggest that the firms may be cherry-picking investment projects with a positive NPV and low risk while using the additional cash from investing less to repurchase shares. This seems to be inefficient, however, as indicated by a decline in the firms' market-to-book ratio. These findings thus echo concerns from the press and policymakers about self-interested managers repurchasing shares instead of investing.[4]

Second, estimates from a Cox (1972) proportional-hazards model suggest that the firms become less likely to relocate to other states when their likelihood of becoming a PE buyout target declines in their home states. This suggests that, on average, firms prefer not to be acquired by PE firms, consistent with the possibility of a PE acquisition being a threat to the firms. A further implication of this finding is that states may be able to retain firms by reducing PE buyout activity within their borders.

Third, I find that job destruction declines at the state level when buyout activity decreases in those states. This reduction in layoffs is likely due to fewer firms going bankrupt and fewer firms relocating to other states, which is consistent with the evidence at the firm level.

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[4]For instance, an article published by Reuters, entitled "Stock buybacks enrich the bosses even when business sags," (Dec 10, 2015) highlights concerns about the misuse of share buybacks to inflate stock prices, potentially benefiting management at the expense of firms' long-term growth. Responding to this concern, U.S. President Joe Biden introduced an additional 1% tax on share repurchases as part of the Inflation Reduction Act of 2022. As Biden stated, *"...they [the firms] used those record profits to buy back their own stock, rewarding their CEOs and shareholders. ...I propose that we quadruple the tax on corporate stock buybacks to encourage long term investments instead."*

Overall, these findings suggest that reduced PE buyout activity leads to fewer firm exits and increases employees' job security. However, there seems to be a trade-off: Managers may also start enjoying the quiet life, where they increasingly cherry-pick investment projects with a positive NPV and low risk, leading to lower investment.

The advantage to using states' adoption of the CFP as a source of variation in PE buyout activity is that these adoptions are unlikely to be an endogenous response to factors that also explain variations in PE buyout activity. Specifically, the CFP applies to all types of bankruptcy proceedings that involve potentially fraudulent asset transfers, not just PE buyouts. This makes it less likely that states adopted the CFP specifically to reduce PE buyouts. In fact, their adoption was likely a response to prompts from legislators and the legal profession promoting consistency in dealing with bankruptcy proceedings across states.[5]

A potential concern is that due to the CFP's applicability to fraudulent transfer cases in general, determining whether the observed changes in firm behaviour following the CFP's adoption are indeed the result of reduced PE buyout activity is challenging. For instance, firms might cut back on their use of unsecured debt after the CFP's adoption due to the increased risk of being sued by unsecured creditors for fraudulent transfers in the event of a default. This could, in turn, increase firms' financial constraints and impact their financing and investment decisions.

To address this concern, I restrict my sample to public firms. The rationale is as follows. There are two main types of asset transfers that may qualify as fraudulent under the CFP: (1) transfers made specifically to defraud creditors, such as firms transferring their assets away shortly before bankruptcy to keep those assets out of creditors' reach, and (2) transfers that qualify as fraudulent under the CFP but are not generally regarded as fraudulent in other situations, such as PE buyouts in which the buyout

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[5]The New York State Bar Association (NYSBA) and the New York City Bar Association (NYCBA), for example, urged the state of New York to adopt the latest version of the fraudulent transfer law to ensure that the state's legislation on bankruptcy proceedings is up-to-date and consistent with that of other states. For details, see the NYSBA's Memorandum in Support (May 2019) at http://www.nysba.org/WorkArea/DownloadAsset.aspx?id=93738 and the NYCBA's letter to the governor recommending the law's adoption at http://documents.nycbar.org/files/20073132-UVTA_Cuomo_FINAL_7.30.19.pdf.

targets later go bankrupt. Public firms, subject to financial reporting requirements and governed by boards that scrutinize their managers, are less likely to commit the former type of "intentional" fraud compared to private firms (Ersahin, Irani, and Waldock 2020). This suggests that if the CFP affect public firms, it is likely through its impact on the latter type of "unintentionally" fraudulent transactions. To my knowledge, the legal profession has only identified PE buyouts of targets that subsequently go bankrupt as transactions of the latter type. Hence, it seems reasonable to suggest that the CFP impact public firms only through its effect on PE buyout activity.

To further alleviate the concern, I focus on a period when the CFP is likely to have a greater impact on the secured lenders that provide PE buyout financing (i.e., the mechanism through which the CFP increases buyout costs): the period following the junk bond market crash at the end of 1989. The idea is the following. In the 1980s, Drexel Burnham's introduction of junk bonds sparked a surge in PE buyout activity (see Figure 1), with junk bonds providing a substantial portion of the debt financing for these buyouts (e.g., Kaplan and Stein 1993; Axelson, Jenkinson, Strömberg, and Weisbach 2013). Secured debt, in contrast, accounted for a much smaller share of the buyout financing. Consequently, if the buyout targets were to go bankrupt, the amount of secured debt to be repaid in the bankruptcy proceedings tended to be relatively modest. This meant that the buyout targets' initial unsecured creditors, who stood to be repaid after the secured creditors, were less likely to have litigated and challenged the buyout deals as fraudulent under the CFP. In particular, such litigation would have been impractical given the associated legal costs. Even if the buyout targets' initial creditors did pursue litigation, the dollar amount of loans held by secured creditors that were exposed to the risk posed by the CFP (i.e., the risk of having their liens removed) was small. Hence, the secured creditors were less likely to have perceived the CFP as a significant risk that would lead them to respond by charging a higher interest rate for the buyout loans. Indeed, consistent with this conjecture, I find no evidence that the CFP's adoption reduced buyout activity before the junk bond market crashed.

I also examine the plausibility of alternative explanations for the observed changes

in firm outcomes and find that they are unlikely to explain my findings.[6] In particular, the reduction in firm creation following the CFP's adoption, documented by Ersahin, Irani, and Waldock (2020), does not seem to explain the decline in firms' investments. Further, this decline is unlikely to result from firms avoiding unsecured debt due to the increased risk of lawsuits from unsecured creditors. My findings also remain robust when controlling for whether firms are protected from hostile takeovers by laws that were introduced during my sample period.

This paper contributes to the ongoing debate over recent efforts to regulate PE buyouts. The introduction of the Stop Wall Street Looting Act, for example, was driven by concerns about the potential negative impacts of PE buyouts on acquired firms.[7] The implications of PE buyouts, however, may extend beyond the acquired firms. For instance, firms that have not yet been acquired by PE firms may also be affected by the mere possibility of becoming a buyout target. These firms may perceive the potential change in ownership in the case of a PE acquisition as either an opportunity for restructuring or a threat to their management, and may, therefore, respond to changes in their likelihood of becoming a PE buyout target. Hence, when assessing the consequences of regulating PE buyouts, it is insufficient to consider only the actually acquired firms. My contribution is to study how firms (specifically, those that have not yet been acquired by PE firms) respond to a reduced likelihood of becoming a PE buyout target.

The paper thereby contributes to the literature on how the likelihood of a takeover affects firm behavior. Existing work shows that, in response to a reduced threat of hostile takeovers, managers become more entrenched and run their firms less efficiently (e.g., Bertrand and Mullainathan 2003; Low 2009; Giroud and Mueller 2010; Gormley and Matsa 2016). In contrast, my paper studies PE buyouts and finds that a reduced

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[6]I discuss these alternative explanations and the corresponding empirical tests in Section 5.4.3.

[7]There is a strand of literature examining how PE buyouts affect acquired firms, with evidence supporting one of the two opposing views: that PE buyouts create value for the targets or that PE firms profit at the expense of their buyout targets. See, for instance, Kaplan (1989), Smith (1990), Lichtenberg and Siegel (1990), Chevalier (1995), Boucly, Sraer, and Thesmar (2011), Lerner, Sorensen, and Strömberg (2011), Davis, Haltiwanger, Handley, Jarmin, Lerner, and Miranda (2014), Bloom, Sadun, and Van Reenen (2015), Agrawal and Tambe (2016), Bernstein and Sheen (2016), Eaton, Howell, and Yannelis (2020), Cohn, Nestpriak, and Wardlaw (2021), Fracassi, Previtero, and Sheen (2022), Kirti and Sarin (2024), and Gupta et al. (2024).

likelihood of becoming a PE buyout target in a firm's home state not only leads to lower firm efficiency but may also affect the firm's decision to relocate to other states, in particular, making such relocations less likely.

More broadly, this paper relates to the literature on managers' incentives to extract private benefits (e.g., Bertrand and Mullainathan 2003; Yermack 2006; Edgerton 2012; Gormley and Matsa 2016; Décaire and Sosyura 2024). Within this literature, most closely related are the papers on entrenched managers seeking a quiet life. Bertrand and Mullainathan (2003), for example, show that managers have the incentive to avoid the difficult decisions and costly efforts associated with shutting down old plants and starting new ones. Giroud and Mueller (2010) find that such managers are also less likely to reduce overhead costs. In contrast, I show that these quiet-life managers may also cherry-pick investment projects with a positive NPV and low risk.

Finally, my paper is related to Ersahin, Irani, and Waldock's (2020) work, which also examines the implications of increased unsecured creditor rights under the CFP. They investigate how the higher costs associated with a business failure—due to the stronger unsecured creditor rights introduced by the CFP—affect small business entry. In contrast, I study how the CFP's strengthening of unsecured creditor rights affects secured lenders who finance PE buyouts and how this, in turn, affects PE buyout activity.

2 Institutional Details

2.1 Fraudulent Transfer Law: The Constructive Fraud Provisions

In 1571, the British Parliament passed the Statute of 13 Elizabeth, prohibiting debtors from making asset transfers intended to "delay, hinder, or defraud creditors." The statute was introduced to help creditors recover debt repayments in cases where the debtors had given up possession of some of their assets shortly before default or bankruptcy to evade repayment. Creditors, however, had to provide evidence that the debtors intended to defraud them, and this was difficult (Ayer, Bernstein, and

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Friedland 2004). In response, the English courts developed a set of criteria known as "badges of fraud," which served as presumptive evidence of a debtor's fraudulent intent. The landmark case was *Twyne's Case* (76 Eng. Rep. 809, Star Chamber, 1601), where an English farmer (Pierce) attempted to defraud his creditors by transferring ownership of his sheep to his brother (Twyne) while continuing to shear and sell the sheep as if they were still his own. Several factors in this case were identified as badges of fraud, for example, "the debtor's continued possession of the property."

The Statute of 13 Elizabeth and the badges of fraud were later adopted by the U.S. legal system. However, jurisdictions differed in their weighting of the badges of fraud and in their criteria for determining whether a transaction was fraudulent. Hence, 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).[8]

The act established a number of objective criteria for determining whether a transfer was fraudulent. These criteria, known as the constructive fraud provisions (i.e., CFP), make it easier for unpaid creditors to seek repayment in court. Specifically, creditors are no longer required to prove that their debtors intended to defraud them. Instead, they can challenge their debtors' prior asset transfers as fraudulent if the following two criteria are met: (1) The debtors received less than fair value for their transferred assets, and (2) the debtors became insolvent following the transfers. If the court rules that the asset transfer was fraudulent, the transferees must return the assets to the debtor so that the creditors can be repaid.

In general, only creditors with claims against a debtor that were established before the debtor's asset transfer are entitled to challenge the transfer as fraudulent and seek repayment. These creditors can argue that, because the debtor did not receive a reasonably equivalent payment for the asset, the transfer may have weakened the

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[8]The NCCUSL made two amendments to the UFCA after its initial enactment in 1918. As a result of these amendments, the UFCA was renamed the Uniform Fraudulent Transfer Act (UFTA) and, subsequently, the Uniform Voidable Transactions Act (UVTA). Both amendments sought to reduce ambiguity in the courts' interpretation of the act during bankruptcy proceedings, while retaining the UFCA's structure, organization, and substance.

debtor's financial condition compared to when they initially extended credit, which may have contributed to why they are now unpaid. In contrast, creditors who extended credit to the debtor after the asset transfer may lack grounds for a lawsuit, as they did so with knowledge of the potentially fraudulent transfer.

The CFP applies to all types of bankruptcy proceedings that involve potentially fraudulent asset transfers, which fall into two main categories: The first consists of transfers that are executed with the intent to defraud creditors, for example, if a firm transfers its assets to another party shortly before bankruptcy, to shield these assets from creditors. The second category involves transfers that inadvertently meet the criteria for fraud under the CFP. These transfers, while considered constructively fraudulent in a legal sense, are typically not considered fraudulent in other contexts. Cases where firms go bankrupt after a PE buyout fall into this second category.

2.2 The Constructive Fraud Provisions and Private Equity Buyouts

Here, I discuss how PE buyouts, which are generally not considered fraudulent transfers, may still qualify as fraudulent under the CFP. Section IA in the Internet Appendix discusses a leading case where a PE buyout was ruled fraudulent under the CFP—the *Gleneagles* case. It also provides examples of subsequent cases where PE buyouts were similarly ruled fraudulent.

I begin by describing how a PE buyout typically works, which is crucial to understanding how such a buyout can be deemed fraudulent under the CFP. In a PE buyout, a PE firm acquires a target firm or its division using a large amount of debt that is secured by the target's assets. The process begins with the PE firm forming a shell company with a small amount of capital. Through this shell company, the PE firm raises debt to purchase the target's shares from the target's selling shareholders. The shell company then merges with the target, with the target surviving the merger. Thereafter, the target's assets become collateral for the secured loans that the shell company took out to finance the buyout.

The defining feature that puts a buyout at risk of being deemed fraudulent under

the CFP is the pledge of the target's assets to secure the buyout loan. In doing so, the target is effectively taking on debt to repurchase its own shares.[9] However, courts have ruled that shares bought back by the target and held in treasury are considered to have no value to the target.[10] Consequently, the target is deemed to have received nothing of value in exchange for its newly incurred buyout debt. If the target later goes bankrupt, the buyout may then qualify as a fraudulent transfer under the CFP.

Only the target's initial creditors (i.e., those who already were creditors before the buyout and continue to hold claims afterwards) can challenge the buyout as fraudulent under the CFP and seek repayment.[11] This is because only creditors with claims that were established prior to a given asset transfer have the legal grounds to bring a fraudulent transfer charge. Among these initial creditors, it is typically the unsecured creditors who would pursue such charges, as they tend to receive little or no payment if the target goes bankrupt.[12] In contrast, the creditors who financed the buyout generally lack the legal basis to challenge the buyout as fraudulent since they extended credit with knowledge of the buyout deal. In fact, these creditors might even be adversely affected if the buyout is deemed fraudulent, as I will explain below.

If the target's initial unsecured creditors challenge the buyout as a fraudulent transfer and succeed, the court will order the repayment of debts to these creditors. This repayment is achieved by recovering the funds paid to the target's selling shareholders in the buyout deal and by removing the lien placed on the target's assets by the secured lenders that financed the buyout. This can be costly for the targets' selling shareholders because, even if they can recover their shares by returning their sale pro-

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[9]In a PE buyout, the target firm undergoes two main changes: First, after the shell company acquires the target's shares from the target's selling shareholders and merges into the target, the target ends up owning those shares (i.e., its own bought-back shares). Second, the target's assets are pledged as collateral for the loans that the shell company used to finance the buyout. In sum, these changes mean that the target effectively takes on debt to repurchase its own shares.

[10]Courts have ruled that when firms buy back their own shares from their shareholders and hold them in treasury, as in buyouts, these shares are deemed to have no value to the firms. In addition, any indirect benefits that the target receives from the buyout do not count towards the value received in exchange for incurring the buyout debt. See, for instance, *Robinson v. Wangemann*, 75 F.2d 756.

[11]For example, the court ruling in *Credit Managers Ass'n of Southern California v. Federal Co.* states: "…if leveraged buyouts are to be susceptible to attack on fraudulent conveyance grounds, only those who were creditors at the time of the transaction should have a right to attack the transaction."

[12]This is because their claims are not backed by the target's assets and are repaid only after the secured creditors, which now include the creditors of the target's newly incurred buyout loans.

ceeds, they will now own shares of a bankrupt firm. The secured lenders that financed the buyout will also be adversely affected, as they will be positioned behind the target's initial unsecured creditors during the bankruptcy proceeding, losing priority in claiming the target's assets.[13]

In sum, because the target's selling shareholders and the secured lenders that finance the buyout may incur losses if the buyout is ex post ruled fraudulent, they may demand higher takeover premiums and higher interest rates on the buyout loans ex ante. This increase in the cost of undertaking buyouts could then render some buyouts economically infeasible and, consequently, lead to a decline in buyout activity. I provide empirical evidence of the increasing costs of undertaking buyouts following the CFP's adoption and the resulting decline in buyout activity in Section 5.

2.3 Discussion: Does the CFP Pose a Sufficiently High Risk to Deter PE Buyouts?

One might ask whether the risk of PE buyout deals being deemed a fraudulent transfer under the CFP is sufficiently large to lead secured lenders to charge a higher interest rate on buyout loans and for the target's selling shareholders to demand a higher takeover premium.

Given the extensive legal literature and commentary on the risk of buyouts being deemed fraudulent under the CFP, the relevance of the CFP to buyouts seems unquestionable.[14] Nonetheless, there has been considerable debate about whether the CFP should be applied to PE buyouts. Critics argue that the CFP was initially designed to prevent "genuinely" fraudulent transfers—like the "Elizabethan deadbeat who sells his sheep to his brother for a pittance"—and that buyouts, despite meeting the CFP's criteria for fraudulent transfers, do not resemble such transfers (e.g., Baird and Jackson 1985). In light of these debates, it is important to examine whether courts have

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[13]The unsecured lenders who financed the buyout, in contrast, are not directly affected by the court's orders to recover funds, as they do not hold a lien on the target's assets. However, they may be impacted by the shift in debt repayment priority. Specifically, the target's initial unsecured creditors, who were previously in a similar position to them, will now have priority for debt repayment.

[14]See, for instance, the seminal work of Baird and Jackson (1985), which is frequently cited in court cases addressing buyouts under fraudulent transfer charges. Subsequent notable works, such as Murdoch, Sartin, and Zadek (1987) and Blackwood (1992), are similarly referenced in these court cases.

upheld the application of the CFP to PE buyouts. A review of court cases in Section IB of the Internet Appendix shows that the majority of courts have applied the CFP to buyouts, with only two notable exceptions.

I also examine whether buyouts during my sample period are likely to meet the criteria for a fraudulent transfer under the CFP and thus are at risk of a fraudulent transfer charge. Specifically, the buyout targets must have a sufficiently high likelihood of going bankrupt after the acquisitions, and the time between the completion of the buyouts and the targets' bankruptcy must fall within the CFP's four- to six-year look-back period. Evidence from prior work shows that buyouts of public firms between 1990 and 2008 faced a considerable risk of meeting the CFP's criteria for fraudulent transfers—16% of these firms went bankrupt post-buyout, with an average time to bankruptcy of 4.6 years (e.g., Ayash and Schütt 2016).

One may be concerned that certain buyouts might be exempt from fraudulent transfer charges due to the Bankruptcy Code's safe harbor provision. In the landmark case *Tribune*, which involved the buyout of a publicly traded media giant that later went bankrupt, the court ruled that the buyout target's selling shareholders were protected from fraudulent transfer charges under Section 546(e) of the Bankruptcy Code.[15] Consequently, the USD 8.3 billion in payments to these shareholders were not subject to a clawback. This raises the question of whether selling shareholders in buyouts of public firms are typically exempt from fraudulent transfer charges and whether such exemptions apply to buyouts within my sample period.

To examine how courts have applied Section 546(e) of the Bankruptcy Code to payments made to selling shareholders in buyouts of public firms, I review several court cases within and around my sample period. This review, shown in Section IC of the Internet Appendix, reveals a lack of consensus among courts, and this inconsistency is also reflected in the legal literature. This ambiguity suggests that selling shareholders in buyouts of public firms may still face the risk of fraudulent transfer charges. Furthermore, even if selling shareholders are exempt from these charges, this exemp-

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[15]See, for instance, 946 F.3d 66 (2d Cir. 2019) for details of the case.

tion does not extend to the secured lenders who finance the buyouts. Hence, secured lenders are still likely to charge a higher interest rate for buyout loans to compensate for the risk of losing their liens on the buyout targets' assets.

3 How Might Firms Respond to Reduced Buyout Activity?

I now discuss how firms might respond to a reduced likelihood of becoming a PE buyout target. A useful starting point is to consider the rationale behind PE buyouts and their implications for the targeted firms. PE firms typically conduct buyouts to restructure underperforming or undervalued firms and later sell them at a profit.

To achieve these profits, PE firms may implement changes to the targeted firms' capital structure and governance mechanisms (e.g., Jensen 1986; Jensen 1989). Increasing leverage, for instance, incentivizes the targeted firms' managers to improve efficiency and cut back on investments that yield returns below the cost of capital. PE firms may also introduce operational changes, such as cost reductions, product market expansions, or shifting the focus of the targeted firms' business operations (e.g., Acharya, Gottschalg, Hahn, and Kehoe 2013; Bernstein and Sheen 2016; Fracassi, Previtero, and Sheen 2022; Gupta et al. 2024).

For firms that have not yet been acquired (i.e., potential PE targets), the anticipated changes in the case of a PE buyout may either be seen as an opportunity for restructuring or as a threat to their management (e.g., Halpern, Kieschnick, and Rotenberg 1999; Bharath and Dittmar 2010). For instance, buyouts may appeal to managers seeking to consolidate control. They may also be attractive to public firms looking to transition to private ownership. However, some managers may find buyouts undesirable due to the additional effort required to service and repay the buyout debts.

If firms perceive buyouts as a threat, the possibility of becoming a PE target can act as a disciplining device that keeps the firms' managers on their toes, incentivizing them to maximize their firms' value and deter potential PE acquisitions (e.g., Manne 1965; Grossman and Hart 1980; Scharfstein 1988). Hence, when the likelihood of a buyout decreases, managers may run their firms less efficiently than before. In this case,

managers may live the quiet life and forgo some investment projects with a positive NPV (e.g., Bertrand and Mullainathan 2003). Managers may also invest in projects with a negative NPV, possibly for the perks that come with these investments (e.g., Edgerton 2012), the additional power and compensation they gain from managing larger firms (e.g., Baumol 1959; Marris 1964; Williamson 1964; Jensen 1986), or to secure their positions (e.g., Shleifer and Vishny 1989; Gormley and Matsa 2016).

On the other hand, firms that see buyouts as an opportunity to restructure may want to increase their attractiveness as potential buyout targets when the likelihood of a buyout decreases. In that case, the firms' managers may take actions that reduce the firms' current value (or forego actions that would increase their value), thereby creating an opportunity for PE firms to step in. This may involve deferring investments in projects with a positive NPV.

In sum, regardless of whether firms perceive the possibility of being acquired by a PE firm as a threat or an opportunity, they are expected to become less efficient in response to a reduced likelihood of a buyout.

4 Empirical Framework and Data

To investigate how firms respond to a reduced likelihood of becoming a PE buyout target in their home states, I exploit the state-by-state adoption of the CFP as a source of variation in PE buyout activity in a difference-in-differences framework. To address potential identification concerns—specifically, that the CFP may affect firms through mechanisms other than its impact on PE buyout activity—I focus on public firms and the period following the junk bond market crash at the end of 1989, as discussed in the introduction. My sample thus begins in 1991, the year after the crash.

The CFP applies to the state where the defendant resides (i.e., where the debtor being sued resides). For a lawsuit challenging a PE buyout, the relevant state is thus where the buyout target is headquartered.[16] I classify states as having adopted the

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[16]Examples of cases challenging buyouts as fraudulent transfers, discussed in the Internet Appendix, Section I, illustrate this application.

CFP if they have enacted any version of the fraudulent transfer act put forward by the NCCUSL (i.e., UFCA, UFTA, or UVTA) or if they have otherwise introduced the constructive definition of fraud into their statutory or case law.[17]

4.1 Stacked Difference-in-Differences Approach

Given the staggered timing of the CFP's adoption across states, I use a difference-in-differences framework with a stacked regression approach (e.g., Gormley and Matsa 2011; Cengiz, Dube, Lindner, and Zipperer 2019).

I construct my sample for analysis as follows. I refer to each adoption of the CFP 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 in which the CFP has not yet taken effect (i.e., never-treated states and eventually-treated states),[18] as well as firms headquartered in states in which the CFP had already been in effect for a long time (i.e., already-treated states). Specifically, the already-treated states in my sample include only those that had implemented the CFP at least ten years before the start of my sample period, 1991, with most of those states having done so decades before. I then stack the panels to form the final panel for analysis.

A concern with including already-treated states in the control group is that if the treatment effect varies over time, then the time trends of the newly-treated (i.e., treatment) group and the already-treated (i.e., control) group may differ, even if the newly-treated group was not treated (i.e., in the absence of treatment) (e.g., Goodman-Bacon 2021; Baker, Larcker, and Wang 2022). The parallel trend assumption could thus be violated, leading to biased difference-in-differences estimates.

Biases induced by dynamic treatment effects may be less of a concern in my analy-

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[17]Table IA.1 in the Internet Appendix lists the earliest adoption dates of any version of the NCCUSL fraudulent transfer acts for each state and indicates whether a state adopted a constructive definition of fraud through statutory or case law before the enactment of the NCCUSL fraudulent transfer acts. Figure 2 shows the states that adopted the CFP between 1976, around the beginning of the first buyout wave, and 2004, the end of my sample period.

[18]The eventually-treated states are excluded from the control group once the CFP becomes effective in those states.

sis, however, for the following reasons. First, the fact that most of the already-treated states adopted the CFP decades before the start of my sample period (and all adopted the CFP at least ten years before) makes it likely that the impact of the CFP has already been fully realized (i.e., that there is no remaining time-variation in the treatment effect). Second, in a dynamic difference-in-differences estimation, I find no evidence of differential trends between the control group and the newly-treated group before treatment, regardless of whether the control group includes or does not include already-treated states.[19]

There are also benefits of including already-treated states in the control group. Given that the adoption of the CFP increases the cost of undertaking PE buyouts, PE firms may seek to relocate their buyout investments to states where the CFP is not yet in effect. If so, then the CFP's adoption would result in a reduction in buyout activity in the treatment group, as well as an increase in buyout activity in the eventually-treated and never-treated groups (i.e., in the control group). An increase in buyout activity in the control group in the post-treatment period, however, would violate the assumption that the control group is not affected by the treatment. In that case, the difference between the treatment group and the control group in the post-treatment period would be larger than it would have been if the control group had not been affected. This would imply an overestimation of the treatment effect. By including already-treated states in the control group, one can reduce the potential bias associated with spillovers from the treatment states. The reason for this is that states that have already adopted the CFP are less likely to experience buyout activity inflows after the adoption of the CFP in other states.

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[19]Table 3 estimates the CFP's impact on PE buyout activity at the state level, where the control group consists of never-treated, eventually-treated, and already-treated states. Columns (2) and (4) show the dynamic difference-in-differences estimates. Table IA.4 in the Internet Appendix repeats this analysis but excludes already-treated states from the control group. In both cases, there is no evidence of a pre-treatment trend.

4.2 Regression Specification

The baseline specifications are as follows. To estimate the CFP's impact on state-level outcomes, such as PE buyout activity and layoffs, I estimate the following differences-in-differences regressions at the state-year level:

$$Y_{s,t,k} = \beta \times CFP_{s,k} \times Post_{t,k} + \alpha_{s,k} + \lambda_{t,k} + \theta_s \times t + \gamma' X_{s,k} \times Post_{t,k} + \varepsilon_{s,t,k}, \quad (1)$$

where s indexes states, t indexes years, and k indexes events (i.e., states' adoption of the CFP). $Y_{s,t,k}$ is the outcome of interest for state s in year t for event k. $CFP_{s,k}$ is an indicator that equals one if state s is in the treatment group, that is, if state s adopted the CFP in event k. For each event k, $Post_{t,k}$ is an indicator that equals one if year t is the year of the CFP's adoption or in the period following the adoption. The estimated coefficient β reflects the average treatment effect of the CFP 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 time trends, which allow for differential linear trends between the treatment and control states. $X_{s,k}$ is a vector of state-level characteristics, consisting of *Ln(Per Capita Personal Income)*, *Ln(Number of Firms)*, *Ln(Population)*, *Unemployment Rate*, and *Home Ownership Rate*. I use the pre-treatment values of these control variables, measured as the average over the five years before the CFP's adoption. The reason for using pre-treatment values is that the contemporaneous values of the variables could be endogenous to the CFP's adoption (e.g., Angrist and Pischke 2009). The standard errors are clustered by state, the level at which the CFP is implemented (e.g., Bertrand, Duflo, and Mullainathan 2004).

To examine firms' responses to the CFP's adoption, I estimate the following difference-in-differences regressions at the firm-year level:

$$Y_{i,s,t,k} = \beta \times CFP_{i,s,k} \times Post_{t,k} + \omega_{i,k} + \lambda_{t,k} + \alpha_{s,t} + \gamma' X_{i,k} \times Post_{t,k} + \varepsilon_{i,s,t,k}, \quad (2)$$

where i indexes firms, s indexes states, t indexes years, and k indexes events (i.e., states' adoption of the CFP). $Y_{i,s,t,k}$ is the outcome of interest for firm i headquartered in state s in year t for event k. $CFP_{i,s,k}$ is an indicator that equals one if firm i is in the

treatment group, that is, if firm i is headquartered in the state that adopted the CFP in event k. $Post_{t,k}$ is 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 that control for the time-invariant differences across states since firms may relocate their headquarters to other states. $X_{i,k}$ is a vector of the pre-treatment values of firm-level characteristics, measured as the averages of the firm-level characteristics over the five years before the CFP's adoption. The vector of firm-level characteristics in my main analysis consists of *Ln(Assets)*, *Leverage*, *Cash-to-Assets*, *Return on Assets*, *Cash Flow Volatility*, and *Asset Growth*. The standard errors are clustered at the state level, as before.

4.3 Data

This section provides a brief description of the data used in the paper. A more detailed description is available in the Internet Appendix. Table 1 presents summary statistics.

The sample of PE buyouts that I analyze comes from Refinitiv's Security Data Company (SDC) Platinum M&A database. Information on the PE firms and their buyout investments, including the locations of their headquarters, funds, and portfolio companies, as well as the dates of each investment round, is obtained from VentureXpert. Data on secured loans that were used to finance acquisitions and buyouts come from Refinitiv's Dealscan database.

Data on U.S. public firms come from Compustat. Historical data on the states in which the firms were headquartered come from Bill McDonald's Augmented 10-X Header Data and Bai, Fairhurst, and Serfling (2020).[20] Information on bankruptcy filings is retrieved from Moody's Default and Recovery Database.

State-level macroeconomic data come from various sources: Information on the resident population, the home-ownership rate, the number of firms, and job destruction is from the U.S. Census Bureau. Per capita personal income data are obtained from the U.S. Bureau of Economic Analysis. Data on the unemployment rate come

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[20]Bill McDonald's Augmented 10-X Header Data can be accessed at https://sraf.nd.edu/sec-edgar-data/.

from the U.S. Bureau of Labor Statistics.

5 Results

5.1 Evidence on the Mechanism: CFP and Secured Private Equity Buyout Loans

In this section, I provide evidence that secured lenders charge a higher interest rate on PE buyout loans when the borrower's state (i.e., the state where the PE buyout target is headquartered) adopts the CFP, which is an underlying mechanism through which the CFP increases the cost of a buyout, as discussed in Section 2.2.

To do so, I study a sample of secured PE buyout loans from Dealscan and show that the yield spread on loans originated after the CFP's adoption increases for borrowers in treatment states compared to those in control states. I estimate the following difference-in-differences regression at the loan level:

$$Spread_{i,s,t,k} = \beta \times CFP_{i,s,k} \times Post_{t,k} + \alpha_{s,k} + \lambda_{t,k} + \omega_{i,k} + \delta_{j,k} + \gamma'X_{i,k} + \varepsilon_{i,s,t,k}, \quad (3)$$

where i indexes loans, s indexes states, t indexes years, and k indexes events (i.e., states' adoption of the CFP). $Spread_{i,s,t,k}$ is the yield spread of loan i issued to a borrower headquartered in state s in year t. $CFP_{i,s,k}$ is an indicator that equals one if loan i is in the treatment group, that is, if the borrower of loan i is headquartered in the state that adopted the CFP in event k. $\omega_{i,k}$ denote event-specific borrower-industry fixed effects, and $\delta_{j,k}$ denote event-specific lender fixed effects. $X_{i,k}$ is a vector of control variables. Here, I use contemporaneous values for the variables since borrowers may not necessarily borrow every year and may, therefore, lack pre-treatment characteristics. The control variables are *Ln(Amount)*, *Ln(Maturity)*, *Senior*, *Covenant*, *Sole Lender*, *Refinance*, and *Performance Pricing*. I define all other variables as before and cluster the standard errors at the state level.

Columns (1) and (3) of Table 2 present the results. All specifications include state fixed effects, year fixed effects, borrower-industry fixed effects, and lender fixed effects, each interacted with event fixed effects. Column (3) further controls for loan

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characteristics. The results show that the yield spread of secured PE buyout loans increases by about one percentage point after the borrower's state adopts the CFP. This estimate is robust across specifications and the magnitude of this increase is economically significant, about one-and-a-half times the sample standard deviation.

I next examine the CFP's impact on the yield spread of secured loans used to finance other types of M&A deals (i.e., non-PE buyout M&A deals). Since the CFP affects secured M&A loans only when the targets' assets are pledged as collateral, it should affect only loans in which the targets are the borrowers. Therefore, I do not expect to find an increase in the yield spread of secured loans for non-PE buyout deals, as the borrowers in these cases are typically the acquirers.

The results, presented in columns (2) and (4) of Table 2, show that the CFP has no statistically significant impact on the yield spread of non-PE buyout secured loans. This analysis serves as a useful placebo test, as it suggests an absence of confounding factors that may affect the yield spread on all types of secured acquisition loans, which could otherwise explain my findings for the PE buyout loan sample (i.e., columns (1) and (3)). Further, the results from an F-test show that the difference between the coefficient estimates on *CFP×Post* for the two samples of loans (i.e., PE buyout loans versus non-PE buyout loans) is statistically significant.

5.2 CFP and Private Equity Buyout Activity: State-Level Evidence

5.2.1 Private Equity Buyout Activity

Next, I estimate the impact of the CFP's adoption on PE buyout activity at the state level. To do so, I estimate equation (1) using the PE buyout rate as the dependent variable. The PE buyout rate is measured as the number of completed buyout deals scaled by the average number of firms (i.e., potential targets) in the pre-treatment period. This approach controls for potential differences in economic activity between states. My results are robust to measuring buyout activity simply using the number of buyout deals (i.e., without scaling by the number of firms), as shown in Table IA.3 in the Internet Appendix.

Table 3 presents the results. Consistent with the prediction that the CFP's adoption reduces PE buyout activity, column (1) shows that the coefficient estimate on $CFP \times Post$ is negative and statistically significant at the 1% level. Column (3) shows that the result is robust to controlling for state characteristics. The point estimates indicate a decline of 0.7 basis points in the PE buyout rate, about twice the sample standard deviation. This magnitude is large, and I examine two possible explanations: Section 5.2.2 examines spillover effects from treated to untreated states, and Section 5.2.3 examines reduced firm creation as a possible explanation. Section 5.3 then discusses how to address the possibility of overestimation due to spillovers.

I also examine the dynamic impact of the CFP on the PE buyout rate by introducing lead-lag terms into equation (1):

$$Y_{s,t,k} = \sum_{\tau=-5,\tau\neq-1}^{\tau=5} \beta_\tau \times CFP_{s,k} \times \mathbb{1}\{t=\tau\} + \alpha_{s,k} + \lambda_{t,k} + \theta_s \times t + \gamma' X_{i,k} \times Post_{t,k} + \varepsilon_{s,t,k},$$

(4)

where $\mathbb{1}\{t = \tau\}$ is an indicator for τ years relative to the adoption of the CFP. All other variables are defined as before. The period right before the CFP's adoption (i.e., $\tau = -1$) serves as the reference point and is thus excluded.

Columns (2) and (4) of Table 3 present the results. Figure 3 displays the findings graphically. In all periods before the CFP's adoption (i.e., $\tau < 0$), the coefficient estimates on the interactions between CFP and $\mathbb{1}\{t = \tau\}$ are statistically insignificant. In contrast, the coefficient estimates are negative and statistically significant in the years following the CFP's adoption. These findings suggest that the reduction in PE buyout activity coincides with the CFP's adoption and that the CFP did not have any impact on PE buyout activity before its adoption. This alleviates concerns about differential pre-treatment trends.

One may be concerned that if states' decisions to adopt the CFP were a response to economic downturns, then the downturns themselves might explain the reduction in PE buyout activity. While there are reasons to believe that states' decisions to adopt the CFP are more likely the result of prompts from the legal profession seeking consistency

in dealing with bankruptcy proceedings across states, the concern remains if these prompts coincided with economic downturns.

To alleviate this concern, I examine the dynamics of state-level macroeconomic factors leading up to the adoption of the CFP. Specifically, I examine GDP growth, per capita personal income, total taxes, number of firms, population, unemployment rate, and home-ownership rate. Table IA.10 in the Internet Appendix shows that the impact of the CFP is not present in the five years prior to its adoption, suggesting that state-level macroeconomic conditions are unlikely to have contributed to states' decisions to adopt the CFP.

5.2.2   Relocation of Private Equity Buyout Investments

I now examine spillover effects from treated to untreated states as a possible explanation for the large estimates in Table 3. As states adopt the CFP, the cost of undertaking PE buyouts increases. To reduce costs, PE firms may thus move their buyout investments away from states where the CFP is in effect (i.e., law-adopting states) and into states where the CFP has not yet been adopted (i.e., non-adopting states). If that is the case, then the magnitude of the estimated reduction in PE buyout activity following the CFP's adoption is likely overstated in Table 3. Specifically, the shift in buyout activity to non-adopting states following the CFP's adoption would indicate that the difference in buyout activity between the treatment and the control group in the post-adoption period represents a reduction in the treatment group *and* an increase in the control group. This would imply an overestimation of the treatment effect.

To investigate whether PE firms may be relocating their buyout investments to non-adopting states, I examine whether PE firms that are most likely to be affected by the CFP increase their buyout investments in non-adopting states more than those that are least likely to be affected. To do so, I construct a panel of each PE firm's buyout investments in non-adopting states for each year. I then classify PE firms as either being most likely or least likely to be affected by the CFP's adoption. Specifically, PE firms most likely to be affected are those that likely had plans to invest in states where

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the CFP is adopted (i.e., the treatment state). I classify a PE firm as such if (1) it is headquartered in the treatment state (*PE Headquarters* = 1), or (2) it has local offices in the treatment state that manage buyout funds (*PE Local Offices* = 1). The idea is that investors tend to exhibit familiarity bias when making investment decisions and may therefore invest locally—possibly due to the belief that they have an informational advantage (e.g., Coval and Moskowitz 1999; Bernile, Kumar, and Sulaeman 2015; Ellis, Madureira, and Underwood 2020).

The regression I estimate takes the following form:[21]

$$Relocation_{i,j,s,t,k} = \beta \times CFP_{s,k} \times Post_{t,k} \times PE\ Headquarters_{i,k} + \omega_{s,k} + \lambda_{t,k} + \varepsilon_{i,j,s,t,k}, \quad (5)$$

where i indexes PE firms, j indexes PE firms' investment states, s indexes PE firms' headquarters states, t indexes years, and k indexes events (i.e., states' adoption of the CFP). The dependent variable, $Relocation_{i,j,s,t,k}$, is an indicator that equals one if PE firm i, headquartered in state s, undertook a buyout in non-adopting state j, in year t, in event k. *PE Headquarters$_{i,k}$* is an indicator that equals one if PE firm i is headquartered in the treatment state (i.e., the state that adopted the CFP in event k). The standard errors are clustered at the state level. All other variables are defined as before.

Panel A of Table 4 presents the results. Columns (1) and (2) show that the coefficient estimates on *CFP×Post×PE Headquarters* and *CFP×Post×PE Local Offices* are positive and statistically significant at the 5% level. These results indicate that PE firms headquartered or with local offices in the treatment states are more likely to invest in non-adopting states after the CFP's adoption. Overall, these findings are consistent with the prediction that PE firms would move their investments into non-adopting states in search of lower buyout costs. The findings also imply that the magnitude of the estimated impact of the CFP on buyout activity in Table 3 is indeed overstated. I discuss how to address this overestimation in Section 5.3.

I also investigate whether PE firms relocate their buyout investments to already-treated states. If PE firms are indeed moving their investments away from law-

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[21]The specification illustrates the case where PE firms are classified based on their state of headquarters.

adopting states to reduce costs, then I should not find evidence of the PE firms increasing their investments in already-treated states. To test this, I construct a panel of each PE firm's buyout investments in already-treated states for each year and estimate equation (5). The dependent variable, $Relocation_{i,j,s,t,k}$, is now an indicator that equals one if PE firm i, headquartered in state s, undertook a buyout in already-treated state j, in year t, in event k. The results in Panel B of Table 4 show no evidence of PE firms increasing their investments in already-treated states.

5.2.3 Public versus Private Targets

I now examine reduced firm creation as a possible explanation for the large estimates in Table 3. The mechanism through which the CFP's adoption may lead to reduced firm creation is that the CFP might weaken entrepreneurs' incentives to start new businesses by increasing the costs associated with a potential business failure for small businesses (Ersahin, Irani, and Waldock 2020).

To investigate whether reduced firm creation contributes to the large estimates, I split my sample of PE buyout deals into those with public targets versus those with private targets and examine whether the CFP's impact on PE buyout activity differs between the two samples. The idea is that firms typically start as private firms when initially established. Therefore, if reduced firm creation indeed contributes to the large estimated impact, then I should find the CFP's impact to be more pronounced among buyouts of private firms.

Table 5 shows the estimated impact of the CFP on buyout activity with public targets in columns (1) and (2) and with private targets in columns (3) and (4). The coefficient estimates on $CFP \times Post$ are all negative and statistically significant at the 1% level. For the sample of buyouts with public targets, the point estimate of -0.17 suggests a reduction in buyout activity corresponding to only half of the sample standard deviation. In contrast, for the sample of buyouts with private targets, the point estimate of -0.50 suggests a reduction corresponding to about one-and-a-half times the sample standard deviation, a larger impact than for buyouts of public targets.

Overall, the findings are consistent with reduced firm creation being a possible explanation for the large estimated impact in Table 3. These findings are also consistent with the concern that the CFP may affect private firms through mechanisms beyond its impact on PE buyout activity, as discussed in the introduction. My subsequent analyses will thus focus on public firms to address this identification concern.

5.3 CFP and Firms' Likelihood of Becoming a Private Equity Buyout Target

The evidence thus far indicates that the CFP's adoption reduces PE buyout activity. I now turn to a linear probability model to estimate the CFP's impact on firms' likelihood of becoming a PE buyout target.[22] The dependent variable is *Target*, an indicator that equals one if a firm is a PE buyout target in a given year. The estimation sample now consists only of public firms from Compustat.

Table 6 presents the results. The regressions in columns (1) and (2) include year fixed effects, state fixed effects, and firm fixed effects, all of which are interacted with event fixed effects. Column (2) adds a set of control variables considered in Opler and Titman (1993) and known to affect a firm's likelihood of being acquired in a PE buyout. Specifically, the vector of controls consists of *Operating Income-to-Assets*, *Tobin's Q*, *Machinery Indicator*, *R&D-to-Sales*, *Selling Expenses-to-Sales*, *Ln(Assets)*, *HHI*, *HighOpinc×LowTobinQ*, *LowOpinc×HighTobinQ*, and *HighHHI×LowTobinQ*. The coefficient estimates and standard errors are multiplied by 100 to improve readability.

Column (1) shows that the coefficient estimate on *CFP×Post* is negative and statistically significant at the 1% level. This finding indicates that the firms are less likely to become PE buyout targets following the CFP's adoption, consistent with the evidence at the state level that the CFP reduces PE buyout activity. Column (2) shows that adding controls has little effect on the coefficient estimate on *CFP×Post*. The magnitude of the estimates is economically significant: The point estimates of -0.39 and -0.46 in columns (1) and (2) are about one-tenth of the sample standard deviation.

I address a number of potential identification concerns in the Internet Appendix.

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[22]Untabulated tests indicate that the results are robust to estimating either a logit model or a Cox proportional-hazards model instead.

First, I consider the possibility of an overestimation. As discussed in Section 5.2, there appear to be spillovers of buyout activity from newly-treated states (i.e., the treatment group) to states that have not yet adopted the CFP (i.e., never-treated in the control group), which could lead to overestimation of the treatment effect. To address this possibility of overestimation, I now restrict the control group to include only already-treated states. The idea is that since the CFP is already in effect in already-treated states, these states are less likely to experience positive spillovers of buyout activity from the treatment states.

Columns (1) and (2) of Table IA.5 present the results. The point estimates on *CFP×Post* correspond to less than one-tenth of the sample standard deviation, indicating a smaller magnitude of the estimated impact compared to that of Panel A, although not significantly so. In columns (3) and (4), I exclude already-treated states from the control group to include only never-treated and eventually-treated states, that is, only the untreated states are in the control group. The results indicate that the magnitude of the estimated impact is larger than that of Panel A. Overall, these results are consistent with the idea that buyout activity spills over from treated to untreated states after the CFP's adoption.

Second, I restrict the sample to a period when the CFP's impact on the mechanism through which buyout costs increase—and consequently on PE buyout activity—is likely to be limited: the junk bond market boom in the 1980s.[23] The results, presented in columns (5) and (6), show that the CFP's impact on firms' likelihood of becoming a buyout target is statistically insignificant. These findings serve as a useful placebo test as they suggest an absence of confounding factors that could otherwise explain my findings that the CFP reduces buyout activity.

Third, the introduction of business combination (BC) laws during my sample period, which protects firms from hostile takeovers, might explain firms' reduced likelihood of becoming a PE buyout target. Columns (7) and (8), however, show that my results remain robust even when controlling for whether the firms' state of incorpora-

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[23]The rationale for why the CFP's impact on PE buyout activity is likely limited during this period is discussed in the introduction.

tion had adopted BC laws.

5.4 How Reduced Likelihood of a Private Equity Buyout Affects Firm Efficiency

The evidence thus far suggests that states that adopt the CFP see a decline in PE buyout activity and that firms in these states become less likely to be targeted for PE buyouts. I now investigate how firms respond to this reduced likelihood of being acquired. Section 3 predicts that managers may become more entrenched and run their firms less efficiently when the likelihood of a PE buyout declines. This reduced efficiency may stem from managers forgoing positive NPV investment projects or even undertaking negative NPV investment projects.

In this section, I examine how firms' investments change as the likelihood of a PE buyout decreases. I then investigate whether these changes are consistent with reduced efficiency and how managers' actions may have led to these changes.

5.4.1 Firm Efficiency

Column (1) of Table 7 reports the estimate of the CFP's impact on firm investments. The dependent variable is the capital-expenditure-to-assets ratio. The result shows a 4.1 percentage point decrease in this ratio, indicating that firms undertake fewer investments in response to a reduced likelihood of becoming a PE buyout target. The magnitude of this reduction is economically significant, corresponding to about one-third of the sample standard deviation.

I next investigate whether the reduction in investments is associated with a decrease in firm efficiency. If firms are becoming less efficient as they cut back on investments, it should be the case that they are forgoing investment projects with a positive NPV. However, providing direct evidence of this is challenging due to the difficulty of observing the set of investment opportunities available to firms. I thus adopt an alternative approach by examining whether managerial actions may be driving these changes. Specifically, managers may forgo investments to reduce the firms' risk-taking or to avoid the costly efforts associated with these investments (e.g., Hicks

1935; Bertrand and Mullainathan 2003). As a first test of these possibilities, I examine whether there is evidence consistent with a reduced risk-taking by estimating the CFP's impact on firms' cash flow volatility and the likelihood of bankruptcy.

Columns (2) and (3) of Table 7 present the results. Column (2) shows that cash flow volatility decreases by 11.7 percentage points. The magnitude of this reduction corresponds to about one-tenth of the sample standard deviation. Column (3) shows that firms' likelihood of a bankruptcy declines by 0.3 percentage points. The magnitude of this reduction corresponds to about one-twentieth of the sample standard deviation. A back-of-the-envelope calculation suggests that a 1% reduction in the firms' investments is associated with a 0.2% reduction in cash flow volatility and a 0.4% reduction in the likelihood of bankruptcy.

The findings thus far suggest that the firms may be reducing risky investment projects, but this does not necessarily imply a decrease in firm efficiency. Specifically, firms might prioritize short-term results if they view the possibility of a buyout as a threat (e.g., Stein 1988; Asker, Farre-Mensa, and Ljungqvist 2015). And since it is theoretically ambiguous whether firms over- or under-invest when they focus on the short term, reduced investment in response to a lower likelihood of a buyout could, in this case, be efficient. I thus examine whether there is also evidence of a decline in firm valuation, as reduced efficiency typically leads to lower valuations. I use the market-to-book ratio of assets as a measure of firm value. Column (4) of Table 7 shows that the market-to-book ratio declines following the CFP's adoption.

Overall, the results in Table 7 suggest that firms become less efficient as the likelihood of a PE buyout decreases. This decline in efficiency appears to be driven by the firms' increasing tendency to forgo risky investments, consistent with the idea that the firms' managers may be starting to enjoy the quiet life.

5.4.2 Implications for Profitability

I now examine the implications of firms forgoing risky investment projects on their profitability. A priori, the impact on profitability is not obvious, as the decision to forgo

risky investments may stem from managers' incentives to reduce risk, exert less effort, or both. A decrease in managerial effort, for instance, can affect profitability in several ways: If reduced effort takes the form of managers undertaking fewer investments and also making less effort in negotiating with suppliers, this may increase the costs of their investment projects and potentially lower profitability (Giroud and Mueller 2010). In contrast, if managers take on fewer investment projects, cherry-picking only those with high returns, but maintain their negotiating efforts with suppliers, then profitability is likely to increase.

Table 8 reports estimates of the CFP's impact on the firms' ROA and ROE. Column (1) shows that ROA, measured as net income divided by total assets, increases by 9.1 percentage points following the CFP's adoption. Column (2) shows that ROE, measured as net income divided by equity, increases by 16.7 percentage points following the CFP's adoption. In terms of magnitude, these increases in ROA and ROE correspond to approximately one-tenth of the sample standard deviation.

I examine two possible explanations for the higher ROA and ROE. First, firms might be using their assets more efficiently to generate income. To test this, I examine the firms' free cash flow margin, defined as cash flow from operations minus capital expenditures, divided by revenue. Column (1) of Table 9 shows an increase in the free cash flow margin following the CFP's adoption, suggesting that the firms' investment projects are yielding a higher cash flow per dollar invested. This finding, together with evidence of reduced firm efficiency, suggests that the firms may be cherry-picking investment projects with a high NPV.

Second, the higher ROA and ROE might result from the firms reducing their assets and equity. For example, the firms may be using the cash available after reducing their investments to return capital to shareholders, which potentially reduces their assets and equity, thereby increasing ROA and ROE. To investigate this possibility, I examine changes in the firms' share repurchases and dividend payouts following the CFP's adoption. The results, presented in columns (2) and (3), show an increase in share repurchases after the CFP's adoption, while changes in dividend payouts are

statistically insignificant.

Overall, these findings suggest that the firms' managers may be cherry-picking investment projects with a positive NPV and low risk, consistent with the firms' managers enjoying the quiet life. Further, the managers appear to be increasingly repurchasing shares with the additional cash from making fewer investments. These findings are consistent with concerns from the press and policymakers that entrenched managers may repurchase shares to exploit the temporary price increase for personal gain instead of investing the cash for their firms' growth.

5.4.3 Alternative Explanations and Robustness Tests

This section examines the plausibility of alternative explanations for my main findings. First, perhaps the estimated impact of the CFP on firms' behavior reflects their response to changes in product market competition, rather than changes in the likelihood of becoming a PE buyout target. Specifically, the CFP may reduce firm creation in the treated states (Ersahin, Irani, and Waldock 2020), which could lower the competitive threat from new entrants in these states and potentially explain the decline in firm efficiency. This is unlikely to explain my findings, however, as my analyses focus on public firms, which typically operate across multiple states and face nationwide competition, not just local competition within their own states. Hence, if the CFP affects firm creation in a treated state, it affects product market competition not only for the treated firms but also for the control firms in untreated states—and if product market competition changes in both the treatment and the control group, then this change does not affect the difference-in-differences estimates.

Second, firms might avoid taking on unsecured debt to prevent potential lawsuits from unsecured creditors, who under the CFP have the right to legally challenge the firms' prior asset transfers as fraudulent if debts are not repaid. This could increase the firms' financial constraints. To investigate this possibility, I examine whether the CFP's adoption affects firms' debt composition in terms of unsecured versus secured debt. I compute the share of unsecured debt in total debt as one minus secured debt,

divided by the sum of the book value of total long-term and short-term debt (e.g., Giambona, Golec, and Lopez-de Silanes 2021; Benmelech, Kumar, and Rajan 2024). For robustness, I also measure firms' debt composition as unsecured debt scaled by total assets. The results, presented in Table IA.6, show no evidence of a change in firms' debt composition, alleviating the concern that the firms avoid unsecured debt and thereby become more financially constrained.

Third, prior work indicates that PE buyouts provide firms with access to capital (e.g., Boucly, Sraer, and Thesmar 2011). In that case, reduced PE buyout activity might lead to firms becoming more financially constrained, which could explain their reduced investments. However, this is unlikely to explain my findings since I focus on public firms. Because public firms can access capital through public markets, PE buyouts are more likely to serve as a means of restructuring and mitigating agency problems rather than a source of capital (e.g., Jensen 1986; Fracassi, Previtero, and Sheen 2022).

Fourth, the introduction of business combination (BC) laws during my sample period, which protects firms from hostile takeovers, might explain the decrease in firm efficiency (e.g., Bertrand and Mullainathan 2003). Table IA.8 in the Internet Appendix, however, shows that my results remain robust even when controlling for whether firms' states of incorporation had adopted BC laws.

Finally, firms that moved their headquarters and thereby changed whether they were subject to the CFP are excluded from my sample. The purpose was to address the concern that firms' endogenous choice of their state of headquarters could potentially confound my analysis. It remains possible, however, that firms may decide to stay in a state because they find the state's law favorable. Therefore, my findings could still be confounded by firms' choice of state in which to locate their headquarters. Addressing this issue is challenging. Yet, the fact that the CFP does not appear to have an impact on buyout activity before its adoption, as shown in Table 3, reduces the possibility that firms may have anticipated the CFP's adoption and made their decision regarding the location of their headquarters accordingly. I also find that my results are robust to

including firms that move their headquarters to another state, as shown in Table IA.9 in the Internet Appendix.

5.5 Relocation of Headquarters

I now investigate how a reduced likelihood of a PE acquisition in firms' home states affects their decisions to relocate their headquarters. If firms perceive the possibility of a PE acquisition as a threat, a lower likelihood of becoming a PE target in their home states may be appealing, making them less likely to relocate to other states.

To test this idea, I employ a Cox (1972) proportional-hazards model. The sample construction is akin to the approach used for the stacked difference-in-differences sample described in Section 4.1. For each event (i.e., states' adoption of the CFP), I create a panel where each firm has one observation in the pre-treatment period and one in the post-treatment period that indicates how long the firm stays in its original state of headquarters. To construct this panel, I use five years of observations on firms' headquarters states before and after the CFP's adoption and track, separately for the pre-treatment and post-treatment periods, the number of years firms remain in their original states from the beginning of the estimation.[24] I then stack these panels to form the sample for analysis.

The regression that I estimate is the following:

$$\lambda(\tau | CFP_{i,j,s,k} \times Post_{t,k}, \alpha_{s,k}, \omega_{j,k}, \lambda_{t,k}, X_{i,k} \times Post_{t,k})$$

$$= \lambda_0(\tau) \times exp(\beta \times CFP_{i,j,s,k} \times Post_{t,k} + \alpha_{s,k} + \omega_{j,k} + \lambda_{t,k} + \gamma' X_{i,k} \times Post_{t,k}), \quad (6)$$

where i indexes firms, j indexes industries, s indexes states, t indexes time, k indexes events (states' adoption of the CFP).[25] λ is the hazard rate and λ_0 is the baseline hazard rate. τ represents the number of years from the beginning of the estimation until firm i relocates out of its original headquarters state. For instance, in the post-treatment period, firms that relocate one year after the beginning of the estimation (i.e., the year

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[24]For pre-treatment observations, the beginning of the estimation is five years before the CFP's adoption. For post-treatment observations, the beginning of the estimation is the year of the CFP's adoption.

[25]Here, t indexes whether the observation is from the pre-treatment or post-treatment period, rather than specific years, since each firm has only one observation in each period.

in which the CFP is adopted) are assigned a τ of 1, whereas firms that relocate five years after the beginning of the estimation are assigned a τ of 5. If a firm does not relocate during the estimation period, it is assigned a τ of 6. For each event k, $CFP_{i,j,s,k}$ is an indicator equal to one if firm i, in industry j, headquartered in state s, is in the treatment group, that is, if the firm is headquartered in the state that adopted the CFP in event k. $Post_{t,k}$ is an indicator that equals one if t is in the post-treatment period. $\alpha_{s,k}$ are event-specific state fixed effects, $\omega_{j,k}$ are event-specific industry fixed effects, and $\lambda_{t,k}$ are event-specific time fixed effects. $X_{i,k}$ is a vector of the pre-treatment values of firm-level characteristics, which are the same as those in equation (2). The standard errors are clustered at the state level.

Figure 4 displays the findings graphically. It shows the survival curve for firms headquartered in treatment states versus those in control states. The vertical axis represents the survival rates, derived from the Cox proportional-hazards model, with all covariates set at their mean values. That is, it shows the fraction of firms that remain in their original headquarters states over the estimation period. The horizontal axis represents the number of years that firms remain in their original headquarters states from the start of the estimation period.

The survival curves show that the proportion of firms that remain in their original headquarters states over the five-year period is consistently higher in treatment states compared to those in control states. Put differently, firms become less likely to relocate to other states after their home states adopt the CFP. This finding has two implications. First, it suggests that, on average, firms perceive the possibility of a PE acquisition as a threat, as reduced buyout activity in their home states appears to incentivize the firms to stay. Second, states may be able to retain firms by reducing PE buyout activity within their borders.

In terms of the magnitude of the estimated impact, about 5% of firms in the treatment states relocated to other states within five years after the CFP's adoption, compared to 8% in the control states. This indicates that the CFP's adoption reduces the likelihood of relocation by around 37.5%. For a more formal assessment of the magni-

tude of the estimated impact, I turn to the estimation results from the hazards model presented in Table 10. Across all specifications, the coefficient estimates on *CFP×Post* are negative and statistically significant at the 5% level. These results indicate that the adoption of the CFP reduces the likelihood of firms relocating their headquarters to other states. Specifically, the implied hazard ratio is 0.63, indicating that the CFP reduces the likelihood of firms relocating their headquarters by 37% over the five years following its adoption. This finding is consistent with the estimates from the graphical analysis.

5.6 Implications of Reduced Private Equity Buyout Activity for Employment

The concern driving policymakers to regulate PE buyouts is that PE firms may exploit these buyouts to strip their targets for profit, potentially leading to the targets' bankruptcy and, consequently, layoffs of their employees. In this section, I examine how reduced PE buyout activity affects employment. To do so, I estimate the CFP's impact on job destructions at the state level using equation (1) and employment data from the U.S. Census Bureau's Business Dynamic Statistics Database. The dependent variable is the number of employment losses at existing and closing establishments, scaled by the average number of firms in the pre-treatment period. This approach accounts for potential differences in economic activity between states.

The results in Table 11 suggest that layoffs decline in states with reduced PE buyout activity. This decline is consistent with the finding that firms (i.e., potential PE targets) reduce their risk-taking in response to lower buyout activity in their home states, which leads to fewer bankruptcies and, consequently, fewer layoffs.

6 Conclusion

How do firms respond to a reduced likelihood of being acquired by PE firms? And what are the implications of reduced PE buyout activity for employment? This paper addresses these questions by exploiting state-level variation in PE buyout activity, which stems from the state-by-state adoption of constructive fraud provisions in

fraudulent transfer laws.

I document three sets of findings. First, in response to a reduced likelihood of becoming a PE buyout target, firms appear to increase the cherry-picking of positive NPV investment projects with low risk and use the additional cash from making fewer investments to repurchase shares. This seems inefficient, as indicated by a decline in firms' value. Second, firms become less likely to relocate to other states when their likelihood of becoming a PE buyout target declines in their home states, consistent with the possibility of a PE acquisition being perceived by the firms as something negative. Third, fewer layoffs occur at the state level when PE buyout activity declines in those states, likely due to reduced bankruptcy rates and fewer firms relocating to other states.

Overall, the findings suggest a potential trade-off in reducing PE buyout activity: Whereas reduced PE buyout activity leads to fewer firm exits and strengthens workers' job stability, it also increases managerial entrenchment and lowers investment.

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Figure 1: Private Equity Buyout Activity Between 1976 and 2004
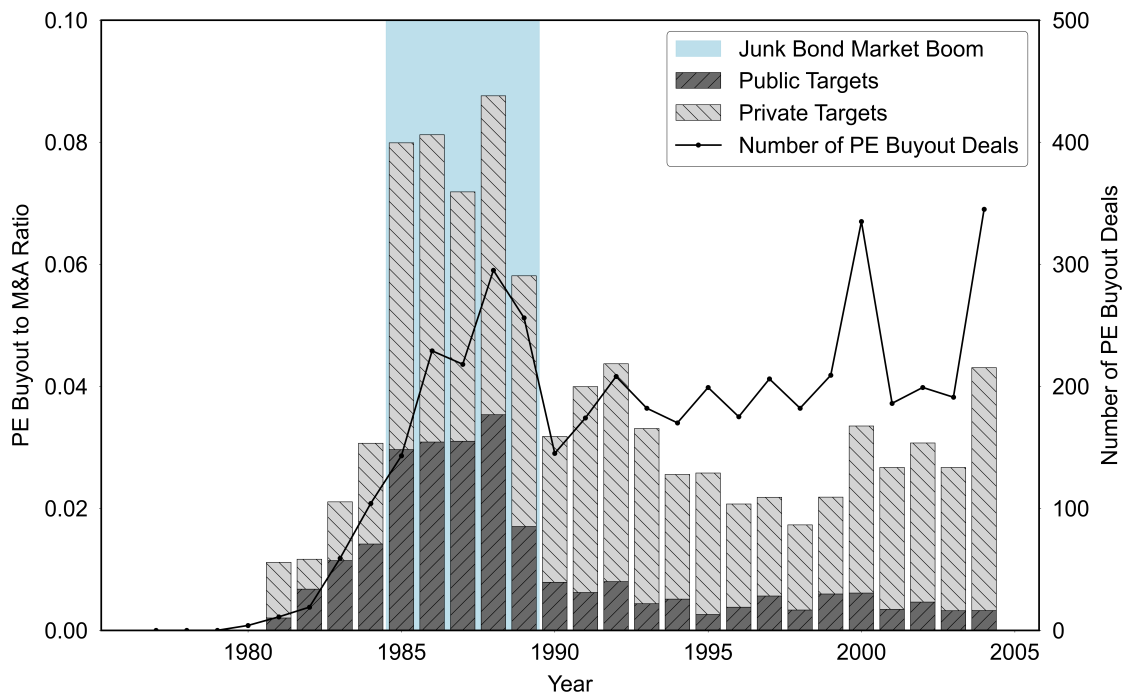
This figure shows the private equity (PE) buyout activity in the U.S. from 1976 to 2004. The data come from SDC Platinum. The vertical axis on the left shows the ratio of PE buyout deals to all M&A deals. The vertical axis on the right shows the total number of PE buyout deals. The shaded area highlights the period of the junk bond market boom (1985 to 1989). The stacked bars represent the ratio of PE buyout deals to total M&A deals: The bars at the top represent PE buyout deals targeting private firms, while the bars at the bottom represent PE buyout deals targeting public firms. The solid line shows the total number of PE buyout deals.

Figure 2: States' Adoption of the Constructive Fraud Provisions Between 1976 and 2004

Legend:
- Already-treated
- Nebraska (1980)
- Hawaii (1985)
- Louisiana (1985)
- Maine (1986)
- Oregon (1986)
- Colorado (1991)
- Iowa (1995)
- Dist. of Columbia (1996)
- Vermont (1996)
- Kansas (1999)
- Never-treated
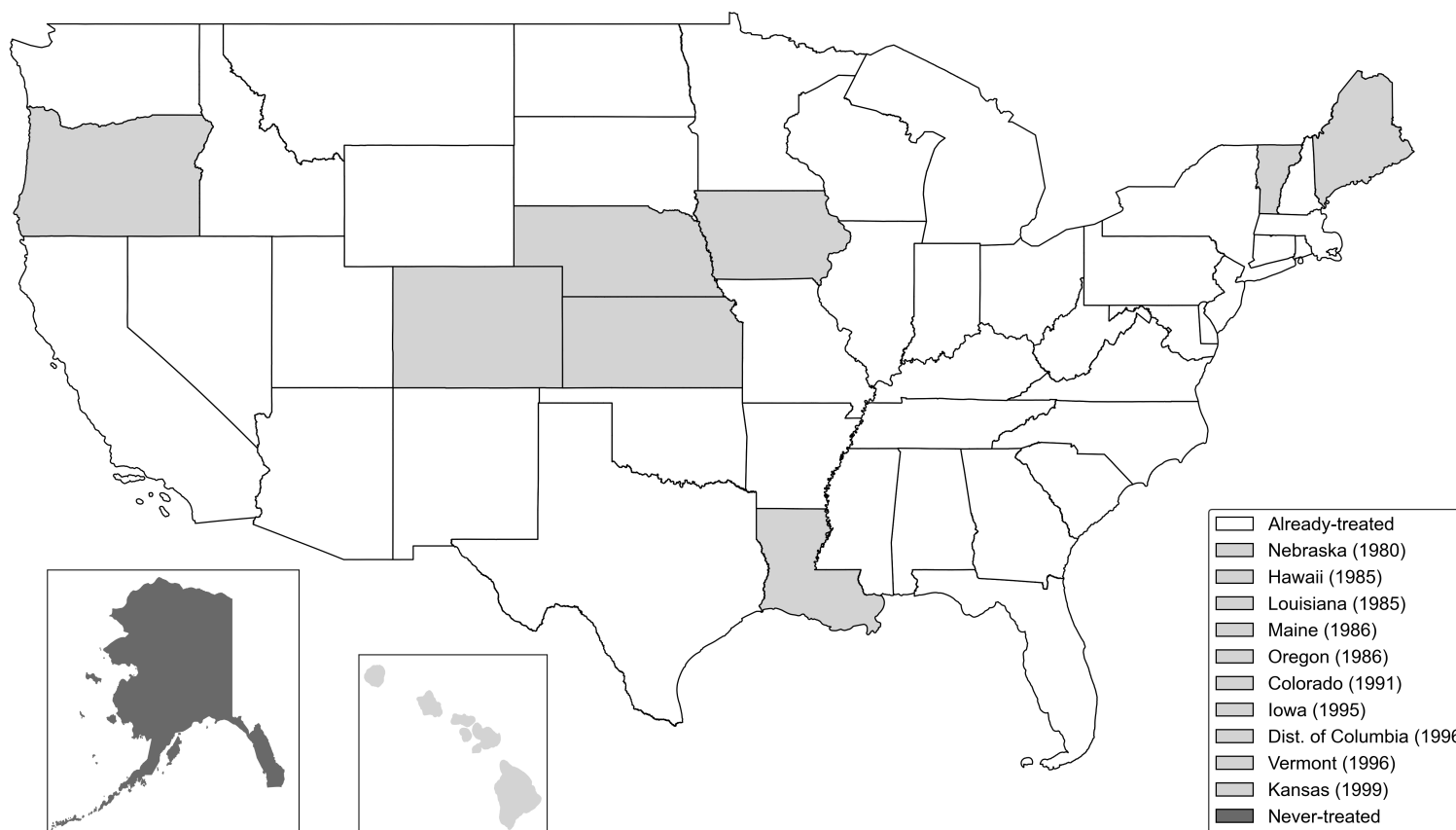
This figure shows the adoption of the constructive fraud provisions (CFP) by states between 1976 and 2004. The ten states that adopted the CFP during this period are depicted in light grey, with the year of adoption indicated in the legend. States that had already adopted the CFP before 1976 are depicted in white. States that did not adopt the CFP between 1976 and 2004 (i.e., only Alaska) are depicted in dark grey.

Figure 3: Dynamic Difference-in-Differences Estimates: Timing of Changes in Private Equity Buyout Activity Around States' Adoption of the Constructive Fraud Provisions
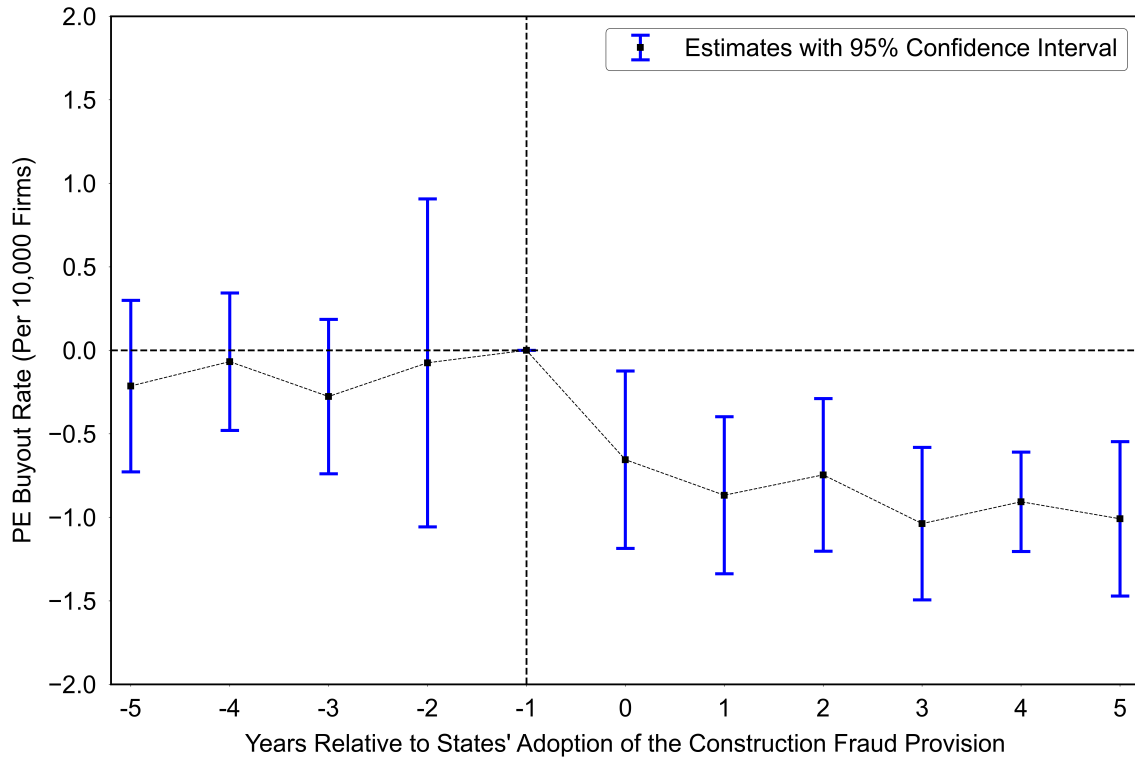
This figure shows a decline in private equity buyout activity at the state level following the adoption of the constructive fraud provisions (CFP). Specifically, the figure plots the estimated β_τ coefficients and 95% confidence intervals from the following regression: $Y_{s,t,k} = \sum_{\tau=-5, \tau \neq -1}^{\tau=5} \beta_\tau \times CFP_{s,k} \times \mathbb{1}\{t = \tau\} + \alpha_{s,k} + \lambda_{t,k} + \theta_s \times t + \gamma' X_{i,k} \times Post_{t,k} + \varepsilon_{s,t,k}$, where s indexes states, t indexes years, and k indexes events (i.e., states' adoption of the CFP). $Y_{s,t,k}$ is the number of completed PE buyouts in state s in year t scaled by the average number of existing firms in state s in the pre-treatment period. $CFP_{s,k}$ is an indicator that equals one if state s is in the treatment group, that is, if state s adopted the CFP in event k. For each event k, τ is the number of years relative to state's adoption of the CFP. The period right before the CFP's adoption ($\tau = -1$) serves as the reference year and is thus omitted.

Figure 4: Cox Proportional-Hazards Model Estimates: Constructive Fraud Provisions and Relocation of Headquarters
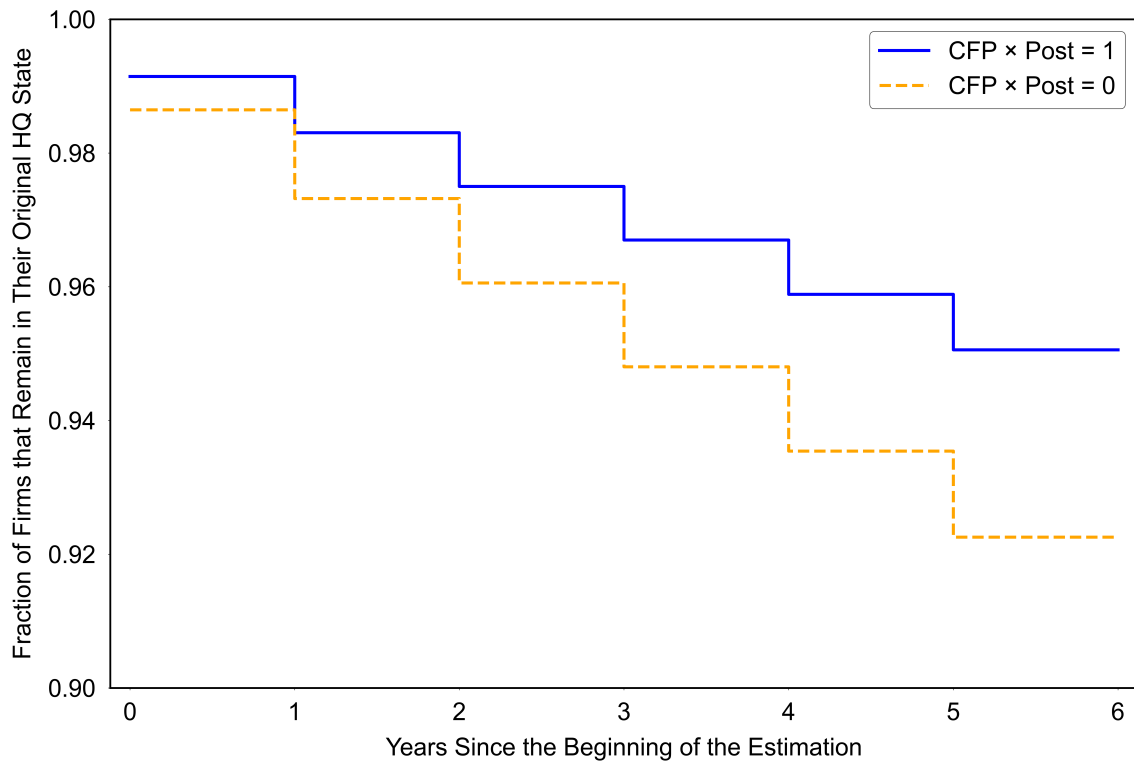
This figure shows a decrease in the likelihood of firms relocating to other states after their home states adopt the constructive fraud provisions (CFP). Specifically, the figure plots the survival curves derived from a Cox Proportional-Hazards Model that estimates the impact of the CFP on the duration that firms remain in their original headquarters state (see equation (6)). The vertical axis represents the fraction of firms that remain in their original headquarters state (i.e., the fraction of firms that did not relocate). The horizontal axis represents the number of years since the beginning of the estimation. The solid line shows the survival curve for firms headquartered in treatment states (i.e., states that transitioned from non-adopting to CFP-adopting). The dashed line shows the survival curve for firms headquartered in control states (i.e., states that have either not yet adopted the CFP or adopted it at least a decade before the beginning of the estimation).

Table 1: Summary Statistics

This table presents summary statistics for the main variables used in the analyses. The analyses use a stacked difference-in-differences approach and, therefore, each observation may be included multiple times in the regression sample. Panel A reports summary statistics for the state-year panel. Panel B reports summary statistics for the firm-year panel. Panel C reports summary statistics for the loan panel. The sample period is from 1991 to 2004. All variables are defined in the Appendix (Table A.1). All continuous variables are winsorized at the 1st and 99th percentiles.

Panel A: State-Year Level Variables							
	Obs.	Mean	SD	p10	p25	p50	p75	p90
Buyout Rate	2,204	0.343	0.314	0.000	0.000	0.313	0.517	0.705
Job Destruction Rate	2,204	5.512	1.149	4.253	4.685	5.298	6.118	7.055

Panel B: Firm-Year Level Variables							
	Obs.	Mean	SD	p10	p25	p50	p75	p90
Capex-to-Assets	178,674	0.078	0.105	0.010	0.023	0.048	0.092	0.167
Cash Flow Volatility	175,590	0.185	1.003	0.039	0.059	0.095	0.163	0.285
Market-to-Book	174,288	2.387	4.705	0.864	1.071	1.462	2.305	4.120
Return on Assets	178,927	-0.114	0.807	-0.372	-0.068	0.027	0.074	0.122
Return on Equity	174,470	-0.226	1.644	-0.428	-0.079	0.031	0.067	0.106
FCF Margin	176,489	-0.439	2.977	-0.396	-0.072	0.003	0.050	0.109
Repurchases-to-Assets	178,940	0.011	0.034	0.000	0.000	0.000	0.002	0.033
Dividends-to-Assets	178,940	0.007	0.017	0.000	0.000	0.000	0.004	0.024
Bankrupt $\times$ 100	178,940	0.219	4.669	0.000	0.000	0.000	0.000	0.000
Target $\times$ 100	187,499	0.273	5.218	0.000	0.000	0.000	0.000	0.000

Panel C: Loan Level Variables							
	Obs.	Mean	SD	Min	p10	p50	p90	Max
Spread (PE Buyouts)	86,275	288.982	68.627	45.000	200.000	275.000	375.000	575.000
Spread (Other M&A)	357,359	258.117	98.502	17.000	137.500	250.000	375.000	575.000

Table 2: Mechanism: Constructive Fraud Provisions and Secured Loan Spreads

This table presents difference-in-differences estimates of the impact of the constructive fraud provisions (CFP) on loan spreads for the sub-samples of secured loans that were used to finance PE buyouts and those for all other M&A deals. Each observation is a loan tranche. The sample period is from 1991 to 2004. The dependent variable, *Spread*, is the all-in spread drawn, measured in basis points. An event is defined as an instance in which a state adopts the CFP. *CFP* is an indicator that equals one if a given state is in the treatment group, that is, if the state adopted the CFP in a given event. For a given event, *Post* is an indicator that equals one if a given year is the year of the CFP's adoption or is in the period following the adoption. An F-test is used to determine whether there is a statistically significant difference between the coefficient estimates of *CFP×Post* between the two sub-samples (i.e., PE buyout loans versus all other M&A loans). Standard errors are clustered at the state level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Dependent Variable:	Spread (In Basis Points)			
Loan Purpose:	PE Buyout	Other M&A	PE Buyout	Other M&A
	(1)	(2)	(3)	(4)
CFP × Post	86.615***	1.380	101.193***	8.214
	(17.860)	(15.240)	(14.854)	(12.602)
Event × Year Fixed Effects	Yes	Yes	Yes	Yes
Event × State Fixed Effects	Yes	Yes	Yes	Yes
Event × Industry Fixed Effects	Yes	Yes	Yes	Yes
Event × Lender Fixed Effects	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Observations	86,275	357,359	86,275	357,359
R^2	0.534	0.420	0.574	0.506
p-value of F-test	0.003		0.000	

Table 3: Constructive Fraud Provisions and Private Equity Buyout Activity

This table presents difference-in-differences estimates of the impact of the constructive fraud provisions (CFP) on private equity buyout activity at the state level. Each observation is a state-year. The sample period is from 1991 to 2004. The dependent variable, *Buyout Rate*, is the number of completed buyouts scaled by the average number of firms in the pre-treatment period. An event is defined as an instance in which a state adopts the CFP. *CFP* is an indicator that equals one if a given state is in the treatment group, that is, if the state adopted the CFP in a given event. For a given event, *Post* is an indicator that equals one if a given year is the year of the CFP's adoption or is in the period following the adoption. $\mathbb{1}\{t = \tau\}$ is an indicator for τ years relative to the CFP's adoption. Standard errors are clustered at the state level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Dependent Variable:	Buyout Rate (Per 10,000 Firms)			
	(1)	(2)	(3)	(4)
CFP $\times$ Post	-0.627***		-0.681***	
	(0.082)		(0.088)	
CFP $\times \mathbb{1}\{t = -5\}$		-0.194		-0.217
		(0.247)		(0.262)
CFP $\times \mathbb{1}\{t = -4\}$		-0.055		-0.066
		(0.213)		(0.207)
CFP $\times \mathbb{1}\{t = -3\}$		-0.273		-0.280
		(0.225)		(0.236)
CFP $\times \mathbb{1}\{t = -2\}$		-0.075		-0.079
		(0.493)		(0.498)
CFP $\times \mathbb{1}\{t = 0\}$		-0.599***		-0.659**
		(0.221)		(0.271)
CFP $\times \mathbb{1}\{t = 1\}$		-0.814***		-0.871***
		(0.204)		(0.238)
CFP $\times \mathbb{1}\{t = 2\}$		-0.700***		-0.752***
		(0.240)		(0.229)
CFP $\times \mathbb{1}\{t = 3\}$		-0.998***		-1.047***
		(0.210)		(0.227)
CFP $\times \mathbb{1}\{t = 4\}$		-0.869***		-0.915***
		(0.149)		(0.148)
CFP $\times \mathbb{1}\{t = 5\}$		-0.980***		-1.021***
		(0.235)		(0.229)
Event $\times$ Year Fixed Effects	Yes	Yes	Yes	Yes
Event $\times$ State Fixed Effects	Yes	Yes	Yes	Yes
State-Specific Time Trends	Yes	Yes	Yes	Yes
Pre-Treatment Controls $\times$ Post	No	No	Yes	Yes
Observations	2,204	2,204	2,204	2,204
R^2	0.551	0.554	0.553	0.556

Table 4: Relocation of Private Equity Buyout Investments into Non-Adopting versus CFP-Adopting States

This table presents difference-in-differences estimates of the impact of the constructive provisions (CFP) on PE firms' likelihood of relocating their buyout investments. Each observation is a PE firm-state of investments-year. The sample period is from 1991 to 2004. In Panel A, the dependent variable, *Relocation*, is an indicator that equals one if a given PE firm undertook a buyout in a state where the CFP had not yet taken effect in a given year. In Panel B, the dependent variable, *Relocation*, is an indicator that equals one if a given PE firm undertook a buyout in a state where the CFP is already in effect in a given year. An event is defined as an instance in which a state adopts the CFP. *CFP* is an indicator that equals one if a given state is in the treatment group, that is, if the state adopted the CFP in a given event. For a given event, *Post* is an indicator that equals one if a given year is the year of the CFP's adoption or is in the period following the adoption. *PE Headquarters* is an indicator that equals one if a given PE firm is headquartered in the treatment state. *PE Local Offices* is an indicator that equals one if a given PE firm has local offices that manage buyout funds in the treatment state. Standard errors are clustered at the state level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Panel A: Relocation into Non-Adopting States		
Dependent Variable:	Relocation (Indicator)	
	(1)	(2)
CFP × Post × PE Headquarters	0.084**	
	(0.041)	
CFP × Post × PE Local Offices		0.037**
		(0.018)
Event × Year Fixed Effects	Yes	Yes
Event × State Fixed Effects	Yes	Yes
Observations	2,440	2,440
R^2	0.163	0.163

Panel B: Relocation into CFP-Adopting States		
Dependent Variable:	Relocation (Indicator)	
	(1)	(2)
CFP × Post × PE Headquarters	0.045	
	(0.037)	
CFP × Post × PE Local Offices		0.015
		(0.009)
Event × Year Fixed Effects	Yes	Yes
Event × State Fixed Effects	Yes	Yes
Observations	340,223	340,223
R^2	0.078	0.078

Table 5: Public versus Private Targets

This table presents difference-in-differences estimates of the impact of the constructive fraud provisions (CFP) on private equity buyout activity at the state level. Each observation is a state-year. The sample of private equity buyout deals is divided into buyouts with public targets (columns 1 and 2) and those with private targets (columns 3 and 4). The sample period is from 1991 to 2004. The dependent variable, *Buyout Rate*, is the number of completed buyouts scaled by the average number of firms in the pre-treatment period. An event is defined as an instance in which a state adopts the CFP. *CFP* is an indicator that equals one if a given state is in the treatment group, that is, if the state adopted the CFP in a given event. For a given event, *Post* is an indicator that equals one if a given year is the year of the CFP's adoption or is in the period following the adoption. Standard errors are clustered at the state level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Dependent Variable:	Buyout Rate (Per 10,000 Firms)			
Sample:	Public Targets		Private Targets	
	(1)	(2)	(3)	(4)
CFP × Post	-0.174***	-0.175***	-0.452***	-0.505***
	(0.060)	(0.054)	(0.094)	(0.109)
Event × Year Fixed Effects	Yes	Yes	Yes	Yes
Event × State Fixed Effects	Yes	Yes	Yes	Yes
State-Specific Time Trends	Yes	Yes	Yes	Yes
Pre-Treatment Controls × Post	No	Yes	No	Yes
Observations	2,204	2,204	2,204	2,204
R^2	0.261	0.264	0.527	0.529

Table 6: Constructive Fraud Provisions and Firms' Likelihood of Becoming Buyout Targets

This table presents difference-in-differences estimates of the impact of the constructive fraud provisions (CFP) on firms' likelihood of becoming a private equity buyout target. Each observation is a firm-year. The sample of firms are public firms from Compustat. The sample period is from 1991 to 2004. The dependent variable, *Target*, is an indicator equal to one if a given firm was acquired in a private equity buyout in a given year. An event is defined as an instance in which a state adopts the CFP. *CFP* is an indicator that equals one if a given state is in the treatment group, that is, if the state adopted the CFP in a given event. For a given event, *Post* is an indicator that equals one if a given year is the year of the CFP's adoption or is in the period following the adoption. The coefficient estimates and standard errors are multiplied by 100 to improve readability. Standard errors are clustered at the state level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Dependent Variable:	Target (Indicator)	
Buyout Target Type:	Public Firms in Compustat	
	(1)	(2)
CFP × Post	-0.389***	-0.463***
	(0.079)	(0.069)
Event × Year Fixed Effects	Yes	Yes
Event × State Fixed Effects	Yes	Yes
Event × Firm Fixed Effects	Yes	Yes
Pre-Treatment Controls × Post	No	Yes
Observations	187,499	187,499
R^2	0.204	0.205

Table 7: Firms' Response to States' Adoption of the Constructive Fraud Provisions

This table presents difference-in-differences estimates of the impact of the constructive fraud provisions (CFP) on firms' capital expenditures, cash flow volatility, likelihood of bankruptcy, and market-to-book ratio. Each observation is a firm-year. The sample period is from 1991 to 2004. *Capex-to-Assets* is capital expenditures divided by lagged book value of total assets. *Cash Flow Volatility* is the annualized standard deviation of the quarterly ratio of cash flow to assets. *Bankruptcy* is an indicator that equals one if a firm filed for bankruptcy in a given year. *Market-to-Book* is the sum of book value of debt and market value of equity divided by book value of total assets. An event is defined as an instance in which a state adopts the CFP. *CFP* is an indicator that equals one if a given state is in the treatment group, that is, if the state adopted the CFP in a given event. For a given event, *Post* is an indicator that equals one if a given year is the year of the CFP's adoption or is in the period following the adoption. Standard errors are clustered at the state level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Dependent Variable:	Capex -to-Assets	Cash Flow Volatility	Bankruptcy (Indicator)	Market -to-Book
	(1)	(2)	(3)	(4)
CFP × Post	-0.041**	-0.117**	-0.003***	-0.227*
	(0.018)	(0.057)	(0.001)	(0.129)
Event × Year Fixed Effects	Yes	Yes	Yes	Yes
Event × State Fixed Effects	Yes	Yes	Yes	Yes
Event × Firm Fixed Effects	Yes	Yes	Yes	Yes
Pre-Treatment Controls × Post	Yes	Yes	Yes	Yes
Observations	178,674	175,590	178,940	174,288
R^2	0.484	0.490	0.169	0.490

Table 8: Implications for Profitability

This table presents difference-in-differences estimates of the impact of the constructive fraud provisions (CFP) on firms' return on assets, and return on equity. Each observation is a firm-year. The sample period is from 1991 to 2004. *Return on Assets* is net income divided by book value of total assets. *Return on Equity* is net income divided by book value of equity. An event is defined as an instance in which a state adopts the CFP. *CFP* is an indicator that equals one if a given state is in the treatment group, that is, if the state adopted the CFP in a given event. For a given event, *Post* is an indicator that equals one if a given year is the year of the CFP's adoption or is in the period following the adoption. Standard errors are clustered at the state level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Dependent Variable:	Return on Assets	Return on Equity
	(1)	(2)
CFP × Post	0.091**	0.167***
	(0.044)	(0.040)
Event × Year Fixed Effects	Yes	Yes
Event × State Fixed Effects	Yes	Yes
Event × Firm Fixed Effects	Yes	Yes
Pre-Treatment Controls × Post	Yes	Yes
Observations	178,927	174,470
R^2	0.424	0.237

Table 9: Possible Reasons for the Higher ROA and ROE

This table presents difference-in-differences estimates of the impact of the constructive fraud provisions (CFP) on firms' free cash flow margin, share repurchases, and dividends payouts. Each observation is a firm-year. The sample period is from 1991 to 2004. *FCF Margin* is cash flow from operations minus capital expenditures divided by revenue. *Repurchase-to-Assets* is purchase of common and preferred stock divided lagged book value of assets. *Dividends-to-Assets* is common dividends divided by lagged book value of total assets. An event is defined as an instance in which a state adopts the CFP. *CFP* is an indicator that equals one if a given state is in the treatment group, that is, if the state adopted the CFP in a given event. For a given event, *Post* is an indicator that equals one if a given year is the year of the CFP's adoption or is in the period following the adoption. Standard errors are clustered at the state level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Dependent Variable:	FCF Margin	Repurchases -to-Assets	Dividends -to-Assets
	(1)	(2)	(3)
CFP × Post	0.225**	0.005**	-0.001
	(0.108)	(0.002)	(0.002)
Event × Year Fixed Effects	Yes	Yes	Yes
Event × State Fixed Effects	Yes	Yes	Yes
Event × Firm Fixed Effects	Yes	Yes	Yes
Pre-Treatment Controls × Post	Yes	Yes	Yes
Observations	176,489	178,940	178,940
R^2	0.504	0.344	0.780

Table 10: Cox Proportional-Hazards Model Estimates: Constructive Fraud Provisions and Relocation of Headquarters

This table presents the hazard coefficients for the impact of the CFP on the duration firms remain in their original state of headquarters, estimated using a Cox proportional-hazards model. The sample period is from 1991 to 2004. The dependent variable, *Relocate HQ*, is the time, measured in years, from the beginning of the estimation (year 1) until a firm relocates from its original headquarters state to another state or until the end of the estimation (year 5), whichever is earlier. For instance, firms that relocate in year 1 will be assigned a value of 1. Firms that remain in their original headquarters state during the estimation period will be assigned a value of 6. The sample period is from 1991 to 2004. An event is defined as an instance in which a state adopts the CFP. *CFP* is an indicator that equals one if a given state is in the treatment group, that is, if the state adopted the CFP in a given event. For a given event, *Post* is an indicator that equals one if a given year is the year of the CFP's adoption or is in the period following the adoption. Standard errors are clustered at the state level and reported in parentheses. Hazard ratios are reported in brackets. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Dependent Variable:	Relocate HQ			
	(1)	(2)	(3)	(4)
CFP $\times$ Post	-0.462**	-0.465**	-0.461**	-0.464**
	(0.221)	(0.221)	(0.222)	(0.221)
	[0.630**]	[0.628**]	[0.630**]	[0.629**]
Event $\times$ Time Fixed Effects	Yes	Yes	Yes	Yes
Event $\times$ State Fixed Effects	Yes	Yes	Yes	Yes
Event $\times$ Industry Fixed Effects	No	No	Yes	Yes
Pre-Treatment Controls $\times$ Post	No	Yes	No	Yes
Observations	23,840	23,840	23,840	23,840
Prob $> \chi^2$	0.000	0.000	0.000	0.000

Table 11: Impact of the Constructive Fraud Provisions on Job Destruction at the State Level

This table presents difference-in-differences estimates of the impact of the constructive fraud provisions (CFP) on job destruction at the state level. Each observation is a state-year. The sample period is from 1991 to 2004. *Job Destruction Rate* is the number of employment losses at existing and closing establishments divided by the number of firms in the pre-treatment period. An event is defined as an instance in which a state adopts the CFP. *CFP* is an indicator that equals one if a given state is in the treatment group, that is, if the state adopted the CFP in a given event. For a given event, *Post* is an indicator that equals one if a given year is the year of the CFP's adoption or is in the period following the adoption. Standard errors are clustered at the state level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Dependent Variable:	Job Destruction Rate (Per 100 Employees)	
	(1)	(2)
CFP × Post	-0.427***	-0.419**
	(0.141)	(0.167)
Event × Year Fixed Effects	Yes	Yes
Event × State Fixed Effects	Yes	Yes
State-Specific Time Trends	Yes	Yes
Observations	2,204	2,204
R^2	0.867	0.867

Appendix - Variable Definitions

Table A.1: Variable Definitions

Panel A: Main Dependent and Independent Variables	
Variable	Definition
Bankrupt	Indicator that equals one if a firm filed for bankruptcy in a given year. Source: Moody's Default and Recovery Database.
Buyout Rate	Number of completed private equity buyouts divided by the average number of firms in the pre-treatment period. Source: SDC Platinum; U.S. Census Bureau.
Capex-to-Assets	Capital expenditures (CAPX) 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 to assets, where the estimation window is twelve quarters. Cash flow to assets is computed as operating income after depreciation (OIADP) minus accruals $[(ACT_t - ACT_{t-1}) - (CHE_t - CHE_{t-1}) - (LCT_t - LCT_{t-1}) + (DLC_t - DLC_{t-1}) - DP_t]$ divided by lagged book value of total assets (AT). Source: Compustat.
CFP	Indicator that equals one if a given state is in the treatment group, that is, if the state adopted the CFP in a given event. An event is defined as an instance in which a state adopts the CFP.
Dividends-to-Assets	Common dividends (DVC) divided by lagged book value of total assets (AT). Source: Compustat.
FCF Margin	Cash flow from operations (OANCF) minus capital expenditures (CAPX) divided by revenue (REVT). Source: Compustat.
Job Destruction Rate	Number of employments losses at existing and closing establishments divided by the average number of firms in the pre-treatment period. Source: U.S. Census Bureau.
Market-to-Book	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.
PE Headquarters	Indicator that equals one if the PE firm is headquartered in the treatment state. Source: VentureXpert.
PE Local Offices	Indicator that equals one if the PE firm has local offices that manage buyout funds in the treatment state. Source: VentureXpert.
Post	Indicator that equals one if a given year is the year of the CFP's adoption or is in the period following the adoption.

Table A.1: Variable Definitions (Continued)

Panel A: Main Dependent and Independent Variables (Continued)

Variable	Definition
Relocate HQ	The time, measured in years, from the beginning of the estimation (year 1) until a firm relocates from its original headquarters state to another state or until the end of estimation (year 5), whichever is earlier. Source: Bill McDonald's Augmented 10-X Header Data; Bai, Fairhurst, and Serfling (2020).
Relocation	Indicator that equals one if a given PE firm undertook a buyout in a state where the CFP had not yet taken effect in a given year. Source: VentureXpert.
Repurchases-to-Assets	Purchase of common and preferred stock (PRSTKC) divided by lagged book value of total assets (AT). Source: Compustat.
Return on Assets	Net income (NI) divided by book value of total assets (AT). Source: Compustat.
Return on Equity	Net income (NI) divided by book value of equity (CEQ). Source: Compustat.
Spread	All-in spread drawn, which is the amount a borrower pays in basis points over LIBOR for each dollar drawn down. Source: Dealscan.
Target	Indicator that equals one if a given firm was acquired in a PE buyout in a given year. Source: SDC Platinum.
Unsecured Debt-to-Assets	The sum of book value of total long-term (DLTT) and short-term debt (DLC) minus secured debt (DM) divided by the book value of total assets (AT). Source: Compustat.
Unsecured Debt-to-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.

Panel B: Control Variables

Variable	Definition
Cash-to-Assets	Cash and short-term investments (CHE) divided by book value of total assets (AT). Source: Compustat.
Covenant	Indicator that equals one if the loan has financial covenants. Source: Dealscan.
HHI	Herfindahl index of sales (REVT) defined over a given firm's four-digit SIC code. Source: Compustat.
HighHHI	Indicator that equals one if the firm has above the sample median HHI. Source: Compustat.
HighOpinc	Indicator that equals one if the firm has above the sample median Operating Income-to-Assets. Source: Compustat.
HighTobinQ	Indicator that equals one if the firm has above the sample median Tobin's Q. Source: Compustat.
Home Ownership Rate	The proportion of homes that are owner-occupied. Source: U.S. Census Bureau.

Table A.1: Variable Definitions (Continued)

Variable	Definition
	Panel B: Control Variables (Continued)
Variable	Definition
Leverage	Sum of book value of long-term (DLTT) and short-term debt (DLC) divided book value of total assets (AT). Source: Compustat.
Ln(Amount)	The natural logarithm of the amount of the loan facility in millions of dollars. Source: Dealscan.
Ln(Assets)	The natural logarithm of the book value of total assets (AT). Source: Compustat.
Ln(Maturity)	The natural logarithm of the loan maturity in months. Source: Dealscan.
Ln(Number of Firms)	The natural logarithm of the number of firms. Source: U.S. Census Bureau.
Ln(Per Capita Personal Income)	The natural logarithm of the personal income of all residents divided by the resident population. Source: U.S. Census Bureau.
Ln(Population)	The natural logarithm of the total resident population. Source: U.S. Census Bureau.
LowOpinc	Indicator that equals one if the firm has below the sample median Operating Income-to-Assets. Source: Compustat.
LowTobinQ	Indicator that equals one if the firm has below the sample median Tobin's Q. Source: Compustat.
Machinery Indicator	Indicator that equals one if the firm's SIC code is between 3400 and 4000. Source: Compustat.
Operating Income-to-Assets	Earnings before interest, taxes, depreciation, and amortization (EBITDA) divided by the sum of the book value of debt (LT) and the market value of equity (PRCC_F×CSHO). Source: Compustat.
Performance Pricing	Indicator that equals one if a given loan has performance pricing provisions. Source: Dealscan.
R&D-to-Sales	Research and development expenditures (XRD) divided by sales (REVT). Source: Compustat.
Refinance	Indicator that equals one if a given loan is used to repay existing debt. Source: Dealscan.
Selling Expenses-to-Sales	Selling expenses (XSGA) divided by sales (REVT). Source: Compustat.
Senior	Indicator that equals one if a given loan is a senior loan. Source: Dealscan.
Sole Lender	Indicator that equals one if a given loan has only one lender. Source: Dealscan.
Tobin's Q	Market value of equity (PRCC_F×CSHO) divided by book vale of equity (CEQ). Source: Compustat.
Unemployment Rate	The number of unemployed individuals as a percentage of the labor force. Source: U.S. Census Bureau.

Internet Appendix for "How Do Firms Respond to Reduced Private Equity Buyout Activity?"

Yi-Hsin Lo

Table of Contents

I Institutional Details

A. Court Cases: Constructive Fraud Provisions and Private Equity Buyouts

This section provides examples of private equity buyouts that were later deemed fraudulent transfers under the constructive fraud provisions (CFP) after the buyout targets went bankrupt and defaulted on debts owed to their initial unsecured creditors (i.e., those who were creditors before and continued to hold claims after the buyout).

Section A.1 discusses the leading case, the *Gleneagles* case, which involved the buyout of Raymond Colliery Co., a major coal producer in the United States. Subsequent cases involving buyouts of firms with a deal value of at least USD 10 million (adjusted for inflation to 1980) that were similarly ruled fraudulent after the buyout targets filed for bankruptcy include, for instance, Almac's Inc. and Almac's Supermarkets, Inc., Aluminum Mills Corp., O'Day Corp, TLC Pattern Inc., and Wieboldt Stores, Inc. Section A.2 examines the case of Almac's Inc. and Almac's Supermarkets, Inc., which involved the buyout of a supermarket chain.

Of course, not all buyouts where the targets subsequently go bankrupt and are challenged as fraudulent transfers are ultimately deemed fraudulent by a court. Nevertheless, the defendants in these cases (e.g., the secured lenders who financed the buyouts and the buyout targets' selling shareholders) may still incur significant costs. For instance, due to the high litigation costs, the lengthy process, and the uncertainty of lawsuit outcomes, the defendants might decide to settle with the plaintiffs (i.e., the targets' initial unsecured creditors) before the case reaches trial. Section A.3 provides an example: a lawsuit involving the buyout of Healthco International Inc. The buyout was challenged as a fraudulent transfer after Healthco went bankrupt two years later. Before the case went to trial, the secured lenders who financed the buyout settled with the target's initial unsecured creditors by paying USD 10 million. However, the court

ultimately did not rule the buyout as a fraudulent transfer.[1]

A.1 Leading Case: The *Gleneagles* Case

The leading case where a private equity buyout was deemed fraudulent under the constructive fraud provisions (CFP) is the *Gleneagles* case. The buyout target in this case was Raymond Colliery Co. (hereafter, Raymond).

Raymond was a major anthracite coal producer in the United States, mainly operating in and incorporated in Pennsylvania. In 1973, an investor group acquired Raymond through a shell company, Great American Coal Co. (hereafter, Great American). The acquisition was partly financed by a loan of USD 8.5 million from Institutional Investors Trust (IIT), with Raymond's assets pledged as collateral for the loan. At the time of the buyout, Raymond had existing debt of at least USD 20 million, which included liabilities for federal income taxes, municipal real estate taxes, and strip mining and backfilling obligations.

Shortly after the buyout, Raymond struggled to meet its tax payments, loan obligations, and operating expenses. In 1980, the federal government filed a lawsuit against Raymond, challenging the buyout as a fraudulent transfer under the Pennsylvania Uniform Fraudulent Conveyances Act (UFCA), the state's fraudulent transfer law incorporating the CFP. The lawsuit sought repayment of Raymond's unpaid federal income taxes from 1966 to 1971, which amounted to USD 2.8 million.[2]

The court ruled that the buyout constituted both constructive and actual fraud under the UFCA. Under the constructive fraud provisions (CFP), a transfer may be deemed fraudulent if the transferor did not receive a reasonably equivalent value in return and was left insolvent as a result. In this case, the court's judgment was based on the fact that the loan proceeds from IIT, which were secured by Raymond's assets, flowed through Raymond and were immediately transferred to its selling sharehold-

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[1]Note that charges against the secured lenders and those against the selling shareholders are treated as separate matters. Therefore, if the secured lenders settle with the plaintiffs, the charges against the selling shareholders may still proceed to trial.

[2]See, for instance, 565 F. Supp. 556 (M.D. Pa. 1983) for details of the case.

ers. Consequently, Raymond was deemed to have received no fair payment for pledging its assets as collateral for the buyout loan and merely acted as a conduit in the buyout. As a result, the court invalidated IIT's claims on Raymond's assets that had been pledged as collateral for the loan. Additionally, Raymond's selling shareholders were found to have breached their fiduciary duty because they were aware that the buyout would adversely affect both Raymond and its initial creditors (i.e., those who were creditors before and continued to hold claims after the buyout).

A.2 Almac's Inc. and Almac's Supermarkets, Inc.

This section discusses another case where a buyout was similarly ruled fraudulent under the constructive fraud provisions (CFP). The buyout target in this case was Almac's Inc. and Almac's Supermarkets, Inc. (collectively, Almac's).

Almac's was a grocery store chain primarily operating in Rhode Island and incorporated in Delaware. In 1991, Almac's was acquired in a buyout by a firm named MAFCO. To finance the buyout and to pay off some of its existing debt, Almac's borrowed USD 94 million from a bank syndicate led by Citicorp, which required Almac's to pledge nearly all of its assets as collateral for the loan.

On August 6, 1993, Almac's filed for Chapter 11 bankruptcy. On May 9, 1995, Arnold Zahn, the bankruptcy trustee, filed a lawsuit against several parties, challenging the buyout as a fraudulent transfer under Rhode Island's UFTA (i.e., the state's fraudulent transfer law incorporating the CFP).[3] The lawsuit sought to invalidate the transfers made to the defendants in connection with the buyout. The defendants named in the lawsuit included: (1) the bank syndicate that helped finance the buyout—Citicorp Securities Markets, Inc., Citicorp North America, Inc. and Citibank, N.A.—and (2) the corporate and partnership entity that owned and sold Almac's stock in the buyout.

The court ruled in favor of the plaintiffs, affirming that the buyout constituted a fraudulent transfer. The defendants subsequently filed a motion to dismiss the plain-

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[3]See, for instance, 202 B.R. 648 (D.R.I. 1996) for details of the case.

tiffs' claims, but the court denied this motion.

A.3 Healthco International, Inc.

This section provides an example of a buyout that was challenged as a fraudulent transfer under the constructive fraud provisions (CFP) but was ultimately not deemed fraudulent by the court. The buyout target in this case was Healthco International, Inc. (hereafter, Healthco).

Healthco was a distributor of dental products and services, formed in 1967 and incorporated in Delaware. Its stocks were publicly traded on the NYSE. On May 22, 1991, Healthco was taken private by a holding company, HMD Acquisition Corp., established by Hicks, Muse & Co. (hereafter, Hicks, Muse), a Dallas-based investment firm. As the surviving entity of the buyout, Healthco inherited all of HMD Acquisition Corp.'s debts, including the multimillion-dollar liabilities owed to the banks that financed the buyout.

On June 9, 1993, Healthco petitioned for Chapter 11 bankruptcy, which was later converted to a Chapter 7 bankruptcy upon court approval. On June 8, 1995, William Brandt, the bankruptcy trustee, filed a lawsuit against several parties, alleging that the buyout was a fraudulent transaction.[4] The defendants named in the lawsuit were accused of being either responsible for or beneficiaries of the buyout that led to Healthco's bankruptcy. The defendants named in the lawsuit included (1) Hicks, Muse; (2) the banking group that financed the buyout; and (3) financial advisors and professionals involved in facilitating the buyout, such as Lazard Freres Co.

The banking group that financed the buyout settled with the trustee before the case went to trial, paying USD 10 million. However, other defendants, including Lazard Freres Co. and Hicks, Muse, did not reach a settlement with the trustee. After the hearings, the court ruled that the buyout did not constitute a fraudulent transfer.

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[4]See, for instance, 208 B.R. 288 (Bankr. D. Mass. 1997) for details of the case.

B. Debate on the CFP's Application to Private Equity Buyouts

There has been considerable debate within the legal profession regarding whether the constructive fraud provisions (CFP) should apply to PE buyouts. For instance, in their seminal work, Baird and Jackson (1985) argue against penalizing buyouts under the CFP. They contend that the CFP was designed to prevent "genuinely" fraudulent transfers and that "a firm that incurs obligations in the course of a buyout does not seem at all like the Elizabethan deadbeat who sells his sheep to his brother for a pittance." Subsequent contributions to this debate include works by Liss (1987), Sherwin (1988), Smyser (1988), Baird (1991), Weinstein (1993), Guisado (2013), and Femino (2014).

Following Baird and Jackson's (1985) seminal work, some courts initially hesitated to apply the constructive fraud provisions (CFP) to buyouts.[5] However, most courts have since generally accepted the CFP's applicability to buyouts. Examples include, for instance, *Boyer v. Crown Stock Dist*, *Crowthers McCall Pattern, Inc. v. Lewis*, *In re Almac's, Inc.*, *In re Consolidated Capital Equities Corp.*, *In re Healthco Intern., Inc.*, *Lippi v. City Bank*, *Mellon Bank v. Metro Communications, Inc.*, *Moody v. Security Pacific Business Credit*, *O'Donnell v. Royal Business Group, Inc.*, *United States v. Tabor Court Realty Corp.*, *Vadnais Lumber Supply, Inc. v. Byrne*, and *Wieboldt Stores, Inc. v. Schottenstein*.

The following quotes from court rulings illustrate the majority view that the CFP should apply to PE buyouts. In *United States v. Tabor Court Realty Corp.*, the court held that the broad language of the CFP, which covers any "payment of money … and also the creation of any lien or encumbrance," does not justify the exclusion of specific transactions such as buyouts. The court also held that if buyouts were to be exempt from the CFP, such an exemption should be decided by state legislatures rather than by the courts. In *Crowthers McCall Pattern Inc. v. Lewis*, the court held that "leveraged buy out[s] … can harm creditors in exactly the way fraudulent conveyance laws are

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[5]The only cases where courts have hesitated to apply the CFP to PE buyouts are *Kupetz v. Wolf* and *Credit Managers Ass'n of Southern California v. Federal Co.*, as identified by subsequent court rulings and the legal literature.

designed to prevent" and also noted that "it would be inappropriate for courts in determining the rights of creditors of a corporation to turn a blind eye to the fact that the loan proceeds were merely passed through the corporation to the shareholders."

C.   Buyouts of Public Targets and Section 546(e) of the Bankruptcy Code

The application of Section 546(e) of the Bankruptcy Code to public firm buyouts has been a subject of ongoing debate and ambiguity in the legal profession, with views divided until recently.[6] Section 546(e) provides that bankruptcy trustees may not recover a pre-bankruptcy transfer if it is a settlement payment made by or to a financial institution. Some legal scholars have thus argued that bankruptcy trustees should not be permitted to recover from the selling shareholders of a publicly-traded firm involved in a buyout. Notable discussions in the legal literature include works by Garfinkel (1991), Rand (1991), and Smith and Kennedy (1992).

Courts have also differed on this issue. The following cases, which involved buyouts of public firms around my sample period, illustrate this divide. In *Kaiser Steel Corp. v. Charles Schwab & Co., Inc.*, the court held that payments made to buy out the target's selling shareholders, when processed by brokers, fall under the settlement payments exception covered by Section 546(e). However, this view has not been consistently upheld in subsequent cases. For instance, in *Wieboldt Stores v. Schottenstein*, the court held that such payments were not meant to be covered by Section 546(e). Similar views are expressed in cases such as *In re Healthco Intern., Inc.* and *In re Hechniger Inv. Co. of Delaware*, *Munford v. Valuation Research Corp.*, and *Zahn v. Yucaipa Capital Fund*.

What are the implications for the CFP's impact on the cost of buyouts of public firms given the ambiguity on whether shareholders of public firms are exempt from

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[6]See, for instance, the article titled "U.S. Supreme Court Declines Review of Landmark Tribune Safe Harbor Ruling" by Jones Day at https://www.jonesday.com/en/insights/2021/05/us-supreme-court-declines-review-of-landmark-tribune-safe-harbor-ruling. This article discusses court cases that upheld the 2019 landmark decision in *Tribune*, which holds that selling shareholders in public firm buyouts are exempt from fraudulent transfer charges under Section 546(e) of the Bankruptcy Code.

fraudulent transfer charges under Section 546(e) of the Bankruptcy Code? Specifically, if the proceeds from the sale of shares by selling shareholders cannot be clawed back in the event that a buyout is deemed fraudulent, it might suggest that the CFP's application does not apply to buyouts of public firms and, therefore, the CFP's adoption might not increase the cost of such buyouts.

The following reasons suggest that the CFP still poses a risk to buyouts of public firms. First, the inconsistent application of exemptions for public firm shareholders from fraudulent transfer charges suggests that the risk remains present. Second, secured lenders who finance buyouts may still lose their liens on the targets' assets if the buyout is deemed fraudulent. This is because fraudulent transfer charges against the targets' selling shareholders and against the secured lenders financing the buyouts are treated as separate matters. Therefore, even if the selling shareholders of public buyout targets are exempt, the CFP continues to pose a risk to the secured lenders involved in the buyout.

D. Constructive Fraud Provisions at the State versus the Federal Level

The constructive fraud provisions (CFP) was first introduced by the NCCUSL into state laws through the UFCA and maintained in subsequent amendments, including the UFTA and UVTA. In addition to state laws, the CFP has also been incorporated into federal law, first in the Bankruptcy Act of 1938, and later in the Bankruptcy Reform Act of 1978. While both state and federal laws contain the CFP, the state laws allow for a longer look-back period. Specifically, the look-back period under state laws is four to six years, whereas the look-back period under federal law is two years. This means that unpaid creditors may be able to challenge their debtors' prior transactions made a longer time ago under state laws than under federal law.

E. States' Adoption of the Constructive Fraud Provisions

Table IA.1: Adoption of a Constructive Definition of Fraud by State

TThis table lists the adoption of a constructive definition of fraud by state. Columns (1) and (2) show the year in which states first adopted one of the NCCUSL's fraudulent transfer acts (i.e., UFCA, UFTA, or UVTA) and their effective year. This information is obtained from NCCUSL and Thomson Reuters Westlaw. Column (3) reports whether states adopted a constructive definition of fraud through statutory or case law before their earliest adoption of the NCCUSL's acts. This information is obtained from Ersahin, Irani, and Waldock (2020).

State	NCCUSL Fraudulent Transfer Act - UFCA/UFTA/UVTA Statutory Citation (1)	Effective (2)	Pre-Existing Statutory or Case Law 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	-

II Data Appendix

This section describes the data used in this paper in more detail. My empirical analyses exploit the state-by-state adoption of the CFP and employ a stacked difference-in-differences approach, with the sample including five years of data before and after each state's adoption of the CFP. As discussed in the introduction, my main analyses focus on the period following the junk bond market crash at the end of 1989. Therefore, my sample begins in 1991, the year after the junk bond market crash, and ends in 2004 (because the last state to adopt the CFP did so in 1999).

A. Private Equity Buyout Deals and Loans

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 self-tenders and recapitalizations. Additional information on the PE firms and their buyout investments, including the locations of the PE firms' headquarters, funds, and portfolio companies, as well as the dates of each investment round comes from VentureXpert.

From Refinitiv's Dealscan database, I collect U.S. dollar-denominated secured loans made to U.S. firms that were used to finance acquisitions and buyouts. I exclude loans with missing information on all-in spread drawn, loan amount, maturity, and the state in which the borrower is located. I classify loans with a primary or secondary purpose of either an "LBO" or an "MBO" as PE buyout loans. Non-PE buyout M&A loans are those with a primary or secondary purpose of an "acquisition" or a "takeover."

A.1 Identifying Public versus Private Buyout Targets

To determine whether a PE buyout target is a public or private firm, I use information from CRSP and Compustat. Although SDC Platinum provides information on whether a buyout target firm is public or private, there appear to be issues with the accuracy of their classification. For example, SDC may classify a buyout target as public

if the target was previously public but is no longer traded at the time of the buyout deal announcement. An example is the buyout deal announced on 1984/03/08, in which Geosource Inc. sold one of its division. According to stock price data from CRSP, Geosource Inc. was publicly traded between 1977/06/21 and 1982/07/01. However, SDC classifies the firm as public.

SDC may also fail to identify targets that should be classified as public firms. For example, in a buyout deal announced on 1976/09/02, KKR acquired AJ Industries Inc. Based on stock price data from CRSP, AJ Industries Inc. was publicly traded on the NYSE from 1926/01/04 to 1977/04/07. However, SDC classifies the firm as private.

Table IA.2 reports the number of private equity buyout deals announced and completed in the U.S. between 1976 and 2004, with deals classified based on whether the targets are public or private firms.

B. Public Firms

I construct a panel of U.S. public firms using 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. From Moody's Default and Recovery Database, I obtain data on the firms' bankruptcy filings. Specifically, I extract all observations that constitute a bankruptcy under Moody's definition.

A limitation of the Compustat database is that it only provides information on the firms' most recent state of incorporation and headquarters. Since my identification strategy relies on variation generated from the states where firms are headquartered, it is important to use the correct information on the location of headquarters. To this end, I obtain historical data on the firms' headquarters states from the following two sources: (1) Bill McDonald's Augmented 10-X Header Data, 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, Bai, Fairhurst, and Serfling (2020), which contains information

derived from WRDS SEC Analytics Suite, supplemented by data collected manually from Mergent.

A potential concern is that firms may choose whether to be subject to the CFP by relocating their headquarters to another state, which could bias my estimates (e.g., Gormley and Matsa 2016). For this reason, I exclude firms that moved their headquarters and thereby affected whether they are subject to the CFP. Nontheless, Table IA.9 in the Internet Appendix shows that my estimates are robust to including these firms.

Table IA.2: Private Equity Buyout Deals Between 1976 and 2004: Public versus Private Targets

This table reports the number of private equity buyout deals announced and completed in the U.S. between 1976 and 2004. The deals are classified based on whether the targets are public or private firms.

Year	Public Target: Firm	Public Target: Division	Private Target	Total
(1)	(2)	(3)	(4)	(5)
1976	1	0	0	1
1977	0	0	0	0
1978	0	0	0	0
1979	0	0	0	0
1980	4	0	0	0
1981	1	1	9	11
1982	11	0	8	19
1983	26	6	27	59
1984	34	14	56	104
1985	28	22	90	140
1986	54	33	140	227
1987	54	41	123	218
1988	87	29	173	289
1989	40	35	177	252
1990	14	22	109	145
1991	8	19	147	174
1992	12	26	167	205
1993	12	12	157	181
1994	11	23	136	170
1995	10	10	175	195
1996	18	14	143	175
1997	35	9	152	196
1998	28	4	147	179
1999	53	4	151	208
2000	54	6	273	333
2001	20	4	160	184
2002	23	4	165	192
2003	22	1	168	191
2004	25	1	317	343
Total (1976-2004)	685	340	3,370	4,395

III Supplemental Analyses and Robustness Tests

Table IA.3: Measuring Buyout Activity using the Number of Buyout Deals

This table presents difference-in-differences estimates of the impact of the constructive fraud provisions (CFP) on private equity buyout activity at the state level. Each observation is a state-year. The sample period is from 1991 to 2004. The dependent variable is the number of completed buyouts. An event is defined as an instance in which a state adopts the CFP. *CFP* is an indicator that equals one if a given state is in the treatment group, that is, if the state adopted the CFP in a given event. For a given event, *Post* is an indicator that equals one if a given year is the year of the CFP's adoption or is in the period following the adoption. $\mathbb{1}\{t = \tau\}$ is an indicator for τ years relative to the CFP's adoption. Standard errors are clustered at the state level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Dependent Variable:	Number of Buyouts			
	(1)	(2)	(3)	(4)
CFP × Post	-2.418**		-2.822***	
	(0.901)		(0.945)	
CFP × $\mathbb{1}\{t = -5\}$		-0.539		-0.759
		(0.952)		(0.931)
CFP × $\mathbb{1}\{t = -4\}$		-0.825		-0.961
		(0.833)		(0.800)
CFP × $\mathbb{1}\{t = -3\}$		-0.967		-1.057
		(0.886)		(0.876)
CFP × $\mathbb{1}\{t = -2\}$		-0.937		-0.982
		(1.413)		(1.410)
CFP × $\mathbb{1}\{t = 0\}$		-2.293*		-2.766**
		(1.355)		(1.337)
CFP × $\mathbb{1}\{t = 1\}$		-3.611**		-4.043**
		(1.585)		(1.559)
CFP × $\mathbb{1}\{t = 2\}$		-3.174*		-3.560**
		(1.756)		(1.696)
CFP × $\mathbb{1}\{t = 3\}$		-4.224**		-4.565**
		(1.715)		(1.697)
CFP × $\mathbb{1}\{t = 4\}$		-4.331***		-4.626***
		(1.407)		(1.421)
CFP × $\mathbb{1}\{t = 5\}$		-4.993**		-5.240**
		(2.328)		(2.330)
Event × Year Fixed Effects	Yes	Yes	Yes	Yes
Event × State Fixed Effects	Yes	Yes	Yes	Yes
State-Specific Time Trends	Yes	Yes	Yes	Yes
Pre-Treatment Controls × Post	No	No	Yes	Yes
Observations	2,204	2,204	2,204	2,204
R^2	0.901	0.901	0.898	0.898

Table IA.4: Dynamic Difference-in-Differences Estimates: Control Group Excludes Already-Treated States

This table presents difference-in-differences estimates of the impact of the constructive fraud provisions (CFP) on private equity buyout activity at the state level, where the control group excludes already-treated states. Each observation is a state-year. The sample period is from 1991 to 2004. The dependent variable is the number of completed buyouts scaled by the average number of firms in the pre-treatment period. An event is defined as an instance in which a state adopts the CFP. *CFP* is an indicator that equals one if a given state is in the treatment group, that is, if the state adopted the CFP in a given event. $\mathbb{1}\{t = \tau\}$ is an indicator for τ years relative to the CFP's adoption. Standard errors are clustered at the state level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Dependent Variable:	Buyout Rate (Per 10,000 Firms)	
	(1)	(2)
CFP $\times$ $\mathbb{1}\{t = -5\}$	-0.397	-0.248
	(0.308)	(0.419)
CFP $\times$ $\mathbb{1}\{t = -4\}$	-0.078	0.018
	(0.251)	(0.293)
CFP $\times$ $\mathbb{1}\{t = -3\}$	-0.425	-0.360
	(0.425)	(0.462)
CFP $\times$ $\mathbb{1}\{t = -2\}$	-0.001	0.032
	(0.527)	(0.562)
CFP $\times$ $\mathbb{1}\{t = 0\}$	-0.670	-0.593
	(0.423)	(0.546)
CFP $\times$ $\mathbb{1}\{t = 1\}$	-0.706*	-0.654
	(0.325)	(0.473)
CFP $\times$ $\mathbb{1}\{t = 2\}$	-0.730	-0.717
	(0.399)	(0.552)
CFP $\times$ $\mathbb{1}\{t = 3\}$	-0.819**	-0.846**
	(0.296)	(0.337)
CFP $\times$ $\mathbb{1}\{t = 4\}$	-0.419	-0.486
	(0.286)	(0.410)
CFP $\times$ $\mathbb{1}\{t = 5\}$	-0.961***	-1.091***
	(0.210)	(0.294)
Event $\times$ Year Fixed Effects	Yes	Yes
Event $\times$ State Fixed Effects	Yes	Yes
State-Specific Time Trends	Yes	Yes
Pre-Treatment Controls $\times$ Post	No	Yes
Observations	164	164
R^2	0.474	0.474
p-Value of Test of $H_0 : \sum_{\tau=-5}^{\tau=-2} \beta_\tau = 0$	0.553	0.553
p-Value of Test of $H_0 : \sum_{\tau=0}^{\tau=5} \beta_\tau = 0$	0.018	0.018

Table IA.5: Constructive Fraud Provisions and Firms' Likelihood of Becoming Buyout Targets: Additional Analyses Addressing Potential Identification Concerns

This table presents difference-in-differences estimates of the impact of the constructive fraud provisions (CFP) on firms' likelihood of becoming a private equity buyout target. Each observation is a firm-year. The sample of firms are public firms from Compustat. The dependent variable, *Target*, is an indicator equal to one if a given firm was acquired in a private equity buyout in a given year. An event is defined as an instance in which a state adopts the CFP. *CFP* is an indicator that equals one if a given state is in the treatment group, that is, if the state adopted the CFP in a given event. For a given event, *Post* is an indicator that equals one if a given year is the year of the CFP's adoption or is in the period following the adoption. The coefficient estimates and standard errors are multiplied by 100 to improve readability. Standard errors are clustered at the state level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Dependent Variable:	Target (Indicator)							
Buyout Target Type:	Public Firms in Compustat							
Sample:	Control Group: Only Alr.-Treated		Control Group: Excl. Alr.-Treated		Before Junk Bond Market Crash		Control for Business Combination Laws	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CFP × Post	-0.386***	-0.460***	-1.186**	-0.789**	1.147	1.132	-0.398***	-0.471***
	(0.079)	(0.068)	(0.426)	(0.274)	(1.049)	(1.033)	(0.079)	(0.063)
Event × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event × State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event × Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Treatment Controls × Post	No	Yes	No	Yes	No	Yes	No	Yes
Observations	186,614	186,614	2,003	2,003	169,381	169,381	187,499	187,499
R^2	0.205	0.205	0.206	0.212	0.233	0.234	0.204	0.205

Table IA.6: Impact of the Constructive Fraud Provisions on Firms' Debt Composition: Proportion of Unsecured Debt

This table presents difference-in-differences estimates of the impact of the constructive fraud provisions on firms' debt composition. Each observation is a firm-year. The sample period is from 1991 to 2004. In Column (1), the dependent variable is *Unsecured Debt-to-Debt*, defined as one minus secured debt divided by the sum of book value of total long-term and short-term debt. In Column (2), the dependent variable is *Unsecured Debt-to-Assets*, defined as the sum of book value of total long-term and short-term debt minus secured debt divided by book value of total assets. An event is defined as an instance in which a state adopts the CFP. *CFP* is an indicator that equals one if a given state is in the treatment group, that is, if the state adopted the CFP in a given event. For a given event, *Post* is an indicator that equals one if a given year is the year of the CFP's adoption or is in the period following the adoption. Standard errors are clustered at the state level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Dependent Variable:	Unsecured Debt-to-Debt	Unsecured Debt-to-Assets
	(1)	(2)
CFP $\times$ Post	0.036	-0.009
	(0.035)	(0.017)
Event $\times$ Year Fixed Effects	Yes	Yes
Event $\times$ State Fixed Effects	Yes	Yes
Event $\times$ Firm Fixed Effects	Yes	Yes
Pre-Treatment Controls $\times$ Post	Yes	Yes
Observations	139,117	162,924
R^2	0.627	0.489

Table IA.7: Firms' Response to States' Adoption of the Constructive Fraud Provisions: Period Before the Junk Bond Market Crash

This table presents difference-in-differences estimates of the impact of the constructive fraud provisions (CFP) on firms' capital expenditures, cash flow volatility, likelihood of bankruptcy, and market-to-book ratio. Each observation is a firm-year. The sample period is from 1976 to 1990. *Capex-to-Assets* is capital expenditures divided by lagged book value of total assets. *Cash Flow Volatility* is the annualized standard deviation of the quarterly ratio of cash flow to assets. *Bankruptcy* is an indicator that equals one if a firm filed for bankruptcy in a given year. *Market-to-Book* is the sum of book value of debt and market value of equity divided by book value of total assets. An event is defined as an instance in which a state adopts the CFP. *CFP* is an indicator that equals one if a given state is in the treatment group, that is, if the state adopted the CFP in a given event. For a given event, *Post* is an indicator that equals one if a given year is the year of the CFP's adoption or is in the period following the adoption. Standard errors are clustered at the state level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Dependent Variable:	Capex -to-Assets	Cash Flow Volatility	Bankruptcy (Indicator)	Market -to-Book
	(1)	(2)	(3)	(4)
CFP × Post	-0.004	0.007	0.002	-0.100
	(0.021)	(0.016)	(0.003)	(0.060)
Event × Year Fixed Effects	Yes	Yes	Yes	Yes
Event × State Fixed Effects	Yes	Yes	Yes	Yes
Event × Firm Fixed Effects	Yes	Yes	Yes	Yes
Pre-Treatment Controls × Post	Yes	Yes	Yes	Yes
Observations	145,091	131,209	145,091	144,780
R^2	0.564	0.750	0.171	0.689

Table IA.8: Firms' Response to States' Adoption of the Constructive Fraud Provisions: Controlling for the States' Adoption of Business Combination Laws

This table presents difference-in-differences estimates of the impact of the constructive fraud provisions (CFP) on firms' capital expenditures, cash flow volatility, likelihood of bankruptcy, and market-to-book ratio. Each observation is a firm-year. The sample period is from 1991 to 2004. *Capex-to-Assets* is capital expenditures divided by lagged book value of total assets. *Cash Flow Volatility* is the annualized standard deviation of the quarterly ratio of cash flow to assets. *Bankruptcy* is an indicator that equals one if a firm filed for bankruptcy in a given year. *Market-to-Book* is the sum of book value of debt and market value of equity divided by book value of total assets. An event is defined as an instance in which a state adopts the CFP. *CFP* is an indicator that equals one if a given state is in the treatment group, that is, if the state adopted the CFP in a given event. For a given event, *Post* is an indicator that equals one if a given year is the year of the CFP's adoption or is in the period following the adoption. Standard errors are clustered at the state level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Dependent Variable:	Capex -to-Assets	Cash Flow Volatility	Bankruptcy (Indicator)	Market -to-Book
	(1)	(2)	(3)	(4)
CFP × Post	-0.042**	-0.114*	-0.003***	-0.200
	(0.017)	(0.059)	(0.001)	(0.136)
Event × Year Fixed Effects	Yes	Yes	Yes	Yes
Event × State Fixed Effects	Yes	Yes	Yes	Yes
Event × Firm Fixed Effects	Yes	Yes	Yes	Yes
Pre-Treatment Controls × Post	Yes	Yes	Yes	Yes
Control for BC Law	Yes	Yes	Yes	Yes
Observations	178,674	175,590	178,940	174,288
R^2	0.484	0.490	0.169	0.490

Table IA.9: Firms' Response to States' Adoption of the Constructive Fraud Provisions: Including Firms that Moved their State of Headquarters

This table presents difference-in-differences estimates of the impact of the constructive fraud provisions (CFP) on firms' capital expenditures, cash flow volatility, likelihood of bankruptcy, and market-to-book ratio. The sample of firms include those that moved their state of headquarters and thereby changed whether they are subject to the CFP. Each observation is a firm-year. The sample period is from 1991 to 2004. *Capex-to-Assets* is capital expenditures divided by lagged book value of total assets. *Cash Flow Volatility* is the annualized standard deviation of the quarterly ratio of cash flow to assets. *Bankruptcy* is an indicator that equals one if a firm filed for bankruptcy in a given year. *Market-to-Book* is the sum of book value of debt and market value of equity divided by book value of total assets. An event is defined as an instance in which a state adopts the CFP. *CFP* is an indicator that equals one if a given state is in the treatment group, that is, if the state adopted the CFP in a given event. For a given event, *Post* is an indicator that equals one if a given year is the year of the CFP's adoption or is in the period following the adoption. Standard errors are clustered at the state level and reported in parentheses. Statistical significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Dependent Variable:	Capex -to-Assets	Cash Flow Volatility	Bankruptcy (Indicator)	Market -to-Book
	(1)	(2)	(3)	(4)
CFP × Post	-0.023**	-0.094**	-0.003**	-0.185*
	(0.010)	(0.046)	(0.001)	(0.094)
Event × Year Fixed Effects	Yes	Yes	Yes	Yes
Event × State Fixed Effects	Yes	Yes	Yes	Yes
Event × Firm Fixed Effects	Yes	Yes	Yes	Yes
Pre-Treatment Controls × Post	Yes	Yes	Yes	Yes
Observations	179,087	175,736	179,087	174,435
R^2	0.588	0.490	0.169	0.490

Table IA.10: State-Level Macroeconomic Dynamics Prior to the Adoption the Constructive Fraud Provisions

This table presents state-level macroeconomic dynamics prior to states' adoption of the constructive fraud provisions (CFP). Each observation is a state-year. The sample period is from 1976 to 2004. An event is defined as an instance in which a state adopts the CFP. *CFP* is an indicator that equals one if a given state is in the treatment group, that is, if the state adopted the CFP in a given event. $\mathbb{1}\{t = \tau\}$ is an indicator for τ years relative to the CFP's adoption. Standard errors are clustered at the state 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).

Dependent Variable:	GDP Growth	Ln(Per Capita Personal Income)	Ln(Taxes)	Ln(Number of Firms)	Ln(Population)	Unemployment Rate (%)	Home Ownership Rate (%)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CFP × $\mathbb{1}\{t = -1\}$	-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)
CFP × $\mathbb{1}\{t = -2\}$	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)
CFP × $\mathbb{1}\{t = -3\}$		0.001	0.012	0.014	0.019	-0.226	-1.103
		(0.015)	(0.039)	(0.024)	(0.016)	(0.417)	(0.921)
CFP × $\mathbb{1}\{t = -4\}$		0.002	0.027	0.018	0.024	-0.265	-1.525
		(0.016)	(0.048)	(0.022)	(0.018)	(0.453)	(1.110)
CFP × $\mathbb{1}\{t = -5\}$		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 × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event × State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Specific Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Treatment Controls × Post	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	930	4,985	4,904	4,776	4,985	4,985	4,146
R^2	0.224	0.994	0.994	0.999	0.999	0.820	0.944

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