Non-Compete Agreements and the Market for Corporate Control*

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Abstract

Non-compete agreements (NCAs) limit outside employment options and, therefore, increase personal costs of job displacement for managers. Using state-level changes in NCA enforceability as a natural experiment, we find that managers are more averse to horizontal takeovers when NCA enforcement tightens. In particular, higher enforceability is associated with fewer same-industry takeovers. Those that do materialize are more likely to be hostile, involve higher premiums, and are less likely to complete. Overall, the findings indicate that the use of NCAs and their enforceability have important implications for the market for corporate control and that banning NCAs could actually promote consolidation.

Keywords: Takeovers, Executive Mobility, Non-Compete Agreements, Enforcement

JEL Codes: G34; K31; M12; M55

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1 Introduction

CEOs and other top executives play key roles in the corporation by setting the firm's strategy and spearheading major initiatives and deals. One of the most consequential initiatives a firm's executives could pursue is mergers and acquisitions (M&As), especially when the firm is to become the target in a transaction. While M&As typically enhance value for target shareholders through takeover premiums, they can be a double-edged sword for management due to likely job displacement after the firm is sold (e.g, Martin and McConnell (1991), Agrawal and Walkling (1994), Guo, Gupta, Mortal, and Nanda (2024)). This paper examines how top executives' career concerns affect their disposition towards takeovers and the associated impact on deal outcomes.

In the U.S., many employees and the majority of top executives are subject to so-called non-compete agreements (NCAs) – parts of an employment contract that effectively restrict the post-employment options of workers. The main purpose of this contractual device is to safeguard a firm's intangible capital, such as trade secrets and valuable client relationships, by imposing industry-specific and geographic limitations on former employees. Typically, an NCA prohibits employees from joining a rival firm or launching a new enterprise within the same industry for a duration ranging between one to two years subsequent to their departure from the company. While there is an ongoing debate in the academic and policy circles regarding the desirability of such arrangements, with a few exceptions (most notably California) state courts currently do enforce them.[1]

The use of NCAs appears to impose real constraints on executives. For instance, Garmaise (2011) shows that NCAs substantially inhibit executive mobility, with executives experiencing fewer same-industry transitions and smaller compensation increases upon changing firms. Kini, Williams, and Yin (2021) show that CEOs bound by NCAs are more likely to be fired for poor performance. Ertimur, Rawson, Rogers, and Zechman (2018) show that NCAs result in employment gaps for CEOs characterized as specialists. Consequently, executives bound by NCAs bear significant personal costs associated with potential job displacement. This presents an interesting potential link between NCA usage and the market for corporate control – a link we investigate in this paper.

[1] On April 23, 2024, the U.S. Federal Trade Commission (FTC) moved to ban NCAs nationwide. The ban is expected to be challenged in courts. See "FTC Bans Noncompete Agreements That Restrict Job Switching", *The Wall Street Journal*, 23 April 2024.

Since takeovers are firmly associated with executive job losses, we could expect NCAs to affect takeover dynamics in several ways. First, executives subject to higher post-employment risk due to NCAs are likely to be more averse to takeovers, leading to a reduction in the number of attempted deals. Second, when bids are made, the target firm's management is more likely to exhibit a hostile attitude. Furthermore, executives may negotiate for a higher premium to partially offset their personal costs through their own holdings of the target shares, if any. This premium may also serve as a deterrent to discourage certain bidders from proceeding with their offers. These combined effects are then also expected to result in a decrease in the overall deal completion rate.

At the same time, executives' aversion to takeovers is not the only possible channel for NCAs to affect the takeover market. To the extent that NCAs reduce the mobility of a firm's employees more generally (see, e.g., Johnson, Lavetti, and Lipsitz (2023)), valuable human capital is less likely to voluntarily leave post acquisition. This has the effect of securing the value of the acquired firm and raises the viability of acquisitions as a means of gaining access to specialized labor resources (the so-called "acquihires").[2] For instance, Chen, Gao, and Ma (2021) find that a state's recognition of the Inevitable Disclosure Doctrine (IDD) – which also limits employee mobility – is associated with more acquisitions. One could therefore expect NCAs to be associated with a *higher* likelihood of being targeted, higher deal completion rates, and higher takeover premiums. Note that the former two predictions are *opposite* to the effects predicted by executive career concerns. Which effect dominates, if any, is therefore an empirical matter.

To test these predictions we exploit changes in the *enforceability* of NCAs at the state level. These changes are introduced through state legislative and/or court decisions that are unlikely to be influenced by the takeover outcomes we study, making them plausibly exogenous for our purposes. Moreover, the multiplicity of the enforcement reforms we use makes contemporaneous confounding events less likely. To further address potential omitted factors, in our main tests we focus on horizontal deals where the target and the acquirer operate in the same industry – deals in which a higher level of operational overlap results in a higher post-merger turnover rate for executives (Buchholtz, Ribbens, and Houle (2003)).[3] This allows us to use non-horizontal takeovers as an effective placebo test where we expect weaker or no effects. To the extent that the timing of

[2] On the use of acquisitions to gain access to human capital, see, e.g., Chen, Hshieh, and Zhang (2023).

[3] In contrast, unrelated mergers more often necessitate retaining of target employees to ensure smooth ongoing operations. Target executives' firm-specific knowledge makes them more difficult to replace.

NCA enforcement changes may be endogenous to takeover activity and outcomes, any such omitted variable would have to affect horizontal and diversifying deals differentially for our identification strategy to suffer.

We use the twelve questions proposed by Malsberger (2002) to quantify the state-level enforcement and examine how changes in NCA enforceability affect firms' takeover activities. The enforcement score ranges from zero to twelve, with zero indicating no enforcement of NCAs, and a higher score means tighter enforcement. This measure has been widely used in prior literature regarding NCAs and labor mobility (Garmaise (2011), Kini, Williams, and Yin (2021), Conti (2014), Ysmailov (2022)). Our research design amounts to a difference-in-differences approach with multiple staggered treatments, and we use methods that are robust to the associated challenges pointed out in the recent econometrics literature (e.g., Baker, Larcker, and Wang (2022)).

In most of our tests, we analyze the consequences of NCA enforcement changes for public firm takeovers. We focus on public targets for two reasons. First, access to human capital as a motive for acquisitions is less likely to apply to large listed firms, allowing us to zero-in on the channel of executive career concerns. Second, some of the outcomes we are interested in – such as takeover premiums or hostility – are not available or not applicable to private firm acquisitions. However, we separately analyze deal activity levels for privately-held targets and their subsets, where access to valuable human capital as a motivation for the acquisition is more likely.

Using the CRSP-Compustat panel of U.S. listed firms spanning the years 1981-2013 and M&A data from Thomson Reuters SDC over the same period, we find that greater NCA enforcement has a robust negative effect on the likelihood a firm becomes a target of a horizontal takeover. In our most comprehensive regression specification controlling for firm-level characteristics, state economic conditions, and other relevant state laws, a one-point increase in NCA enforceability is associated with a 5.2% reduction in the likelihood of horizontal takeover. Note that some of the reforms change the NCA enforcement score by more than one point, meaning that these events have a large economic effect on the takeover market. We further examine acquisitions of privately-held targets, especially small firms and high-tech firms. If greater NCA enforcement makes "acquihires" more viable, we would expect *increases* in deal activity in this part of the market – but we find no significant effects (perhaps because NCAs are less prevalent below the management ranks).

We then turn to a deal-level analysis of public firm acquisitions and investigate the impact of

NCA enforceability on the characteristics and outcomes of deals that do materialize. Starting with deal attitude, our findings reveal a notable positive correlation between changes in NCA enforceability and the likelihood of the deal being characterized as hostile or unsolicited – as opposed to friendly. Specifically, increasing NCA enforcement by one point is associated with a 4.69 percentage point increase in the probability of negative target attitude, representing a relative increase of as much as 74%.

We next examine the potential relationship between NCA enforcement and takeover premiums. We find that a one-point increase in the NCA enforcement score corresponds to a 4.55 percentage point increase in takeover premium, which is about a 12% increase relative to the average takeover premium in the reforming states prior to treatment. Again, notice that certain states experience changes in NCA enforcement scores greater than one, leading to even larger effects. These findings for takeover premiums are corroborated by evidence from the target firm's abnormal returns. We also examine the corresponding acquirers' returns and find no significant effects on them.

We also investigate whether deal withdrawal rates are influenced by changes in NCA enforcement. The results indicate that a one-point increase in the NCA enforcement score is associated with a significant 5.8 percentage point increase in the deal withdrawal rate. Considering that the average withdrawal rate in our sample is 13.1%, this effect is highly economically significant. Overall, our findings on takeover likelihood, deal attitude, takeover premiums, and deal withdrawal rates are consistent with our conjecture that executives subject to stricter NCA enforcement become more averse to takeovers that threaten their jobs.[4]

To buttress a causal interpretation of our results we take advantage of a "holdout" sample of non-horizontal takeovers. To the extent that our findings above are driven by executives' career concerns due to changes in NCA enforceability, we expect weaker or even no effects for deals in which the likelihood of executive turnover is lower. If, on the other hand, our findings are driven by an omitted variable correlated with both NCA enforcement changes and takeover outcomes, then we would expect similar effects in the sample of diversifying deals (unless the omitted variable in question is particularly relevant for horizontal deals, which we view as less likely). We repeat all of our tests using non-horizontal takeovers and find that none of the outcomes is robustly affected

[4]As another form of potential takeover resistance, we also investigate whether tighter NCA enforcement compels managers to spearhead the adoption of anti-takeover defences, but we find no such effects.

by NCA enforceability in that sample, which raises the bar for potential alternative explanations. Moreover, consistent with managerial career concerns, the negative effect of NCA enforcement tightening on horizontal takeover likelihood is muted for CEOs approaching retirement age, who arguably are less concerned about finding subsequent employment.

Finally, we return to the overall panel of listed firms and examine whether changes to NCA enforceability are associated with contemporaneous changes in various firm fundamentals. We find no robust effects on characteristics such as firm size, profitability, book-to-market ratio, leverage, and cash holdings. The lack of such effects gives us more confidence that our findings above are driven by changes in executives' attitude towards takeovers, rather than by some changes to the fundamentals of firms comprising the pool of available targets.

We close our analysis by conducting a back-of-the-envelope counterfactual analysis using our reduced-form estimates. Given that executive resistance to horizontal mergers takes different forms – some of which preclude shareholder gains (fewer takeover attempts and fewer completed deals) while others benefit shareholders (higher premiums in completed deals) – the net effect of NCA enforcement on shareholder welfare is ambiguous.[5] We compute the aggregate target shareholder gain observed in our sample under the current regime, and we compare it to two counterfactual scenarios: one that tightens NCA enforcement by one point across the board, and one that lowers it by the same amount. Interestingly, we find that *both* counterfactual scenarios result in lower target shareholder wealth. Thus, it appears that the current level of NCA enforcement is close to optimal from the point of view of minimizing the agency cost arising from executive and shareholder interest misalignment due to NCAs in the context of takeovers.

Overall, our paper contributes to the literature on executive mobility and executive contracts, as well as to the voluminous literature on M&A and corporate governance. While earlier papers by Garmaise (2011), Kini, Williams, and Yin (2021), and Jeffers (2023) have shed light on executive NCA agreements and their potential effects on firm investments, none have comprehensively explored the impact of these contractual devices on the market for corporate control. Our findings also add to the literature on corporate governance by showing how NCA agreements may inadvertently heighten agency conflicts arising from executive self-interest in the context of takeovers.

[5]Here we refer exclusively to the shareholder wealth effects operating through the impact of NCA enforceability on takeover resistance. Other, more direct impacts of NCAs on firm value are not considered.

Most closely related study in this regard is Jenter and Lewellen (2015), who examine the impact of CEO proximity to retirement age on takeover likelihood. Other related work includes Harford and Li (2007) and Hartzell, Ofek, and Yermack (2004), who detail the self-interest of acquirer and target executives, respectively. In terms of policy implications, our findings add to the debate on the benefits and costs of NCA agreements – a debate that is highly relevant given the recent decision by the U.S. Federal Trade Commission for a national ban on the use of NCAs.[6] While the stated intent of the ban is to promote labor market competition, our analysis would suggest that banning NCAs could actually promote consolidation in labor and product markets through more horizontal M&A activity.

The rest of the paper is organized as follows. Section 2 discusses related literature. Section 3 provides institutional background on NCAs and their state-level enforcement. Data sources and research design are described in Section 4. Section 5 presents our main analysis of the effects of the NCA enforcement on M&A outcomes. Section 6 elaborates on further tests designed to alleviate identification concerns and presents our reduced-form counterfactual analysis. Finally, Section 7 concludes the paper with a summary of the findings.

2 Related Literature

NCAs have been receiving increasing attention from researchers. Early studies have largely focused on the implications of NCAs for labor mobility, concluding that NCAs indeed reduce the rate of job transitions (see McAdams (2019) for a review). A growing body of literature investigates broader economic impacts of NCAs, such as their effect on inventors (Marx, Strumsky, and Fleming (2009)), venture capital activities, innovation, and patents (Samila and Sorenson (2011), Johnson, Lipsitz, and Pei (2023)), as well as their influence on entrepreneurial prominence and the emergence of Silicon Valley (Gilson (1999)). In terms of firm dynamics, Stuart and Sorenson (2003) show that NCAs discourage the founding of new firms after liquidity events such as initial public offerings. Further, Starr, Balasubramanian, and Sakakibara (2018) show that NCAs also discourage same-industry spin-outs. Overall, these studies underscore the significant implications of NCAs on the broader economy.

[6]For public comments regarding the initial FTC proposal see: `https://www.regulations.gov/docket/FTC-2023-0007/comments`.

In terms of NCAs for executives, the majority of CEOs in the U.S. are bound by NCAs (Bishara, Martin, and Thomas (2015)). The presence of NCAs has been found to have a substantial impact on executive mobility. For instance, Garmaise (2011) demonstrates that NCAs make it less likely that top executives switch jobs – as driven by fewer same-industry job transitions. He also shows that greater NCA enforceability is associated with lower overall executive pay. More recently, Kini, Williams, and Yin (2021) document that CEOs subject to more enforceable NCAs experience higher forced turnover-to-performance sensitivity. However, somewhat contrary to Garmaise (2011), Kini, Williams, and Yin (2021) conclude that more enforceable NCAs are associated with higher total compensation. Ertimur, Rawson, Rogers, and Zechman (2018) find that non-compete constraints are more likely to result in employment gaps for specialist CEOs (as opposed to generalist CEOs). NCAs also appear to limit the willingness of executives to leverage their information advantage for personal gain. Gao, Guo, Lisic, and Omer (2023) find that higher NCA enforceability decreases insider trading profits by making it less likely that management times their stock sales prior to unfavorable corporate earnings announcements. Overall, the literature would suggest that NCAs shape the behavior of executives by raising their personal cost of displacement.

Less is known about the impact of NCAs on *corporate* decisions and outcomes. Conti (2014) finds that more enforceable NCAs are associated with riskier R&D projects. Jeffers (2023) demonstrates that more stringent NCA enforcement is associated with fewer employee departures in knowledge-intensive occupations, while knowledge-intensive firms increase their investment in physical capital. Focusing on the banking sector, Bird and Knopf (2015) show that tighter NCA enforcement is associated with reduced growth, lower labor expenses, and increased bank profitability. Ysmailov (2022) finds that stricter NCA enforceability is associated with more conservative capital structures when out-of-state employment alternatives are limited. Bai, Eldemire-Poindexter, and Serfling (2023) show that tighter NCA enforcement is associated with lower sensitivity of investment to GDP growth.

Two recent studies evaluate the optimality of current NCA enforcement regimes. Chen, Li, Thakor, and Ward (2024) consider how labor mobility affects a firm's incentives to invest in intangible capital and knowledge workers' incentives to exert effort in the context of a structural model, which they estimate using variation in NCA enforceability. Their counterfactual analysis suggests that the current level of restrictions on labor mobility is close to optimal. Focusing on executive

NCAs in particular, Shi (2023) finds that the presence of an NCA is associated with significantly reduced executive mobility and only marginally higher investment in intangibles, with both effects intensifying with NCA enforceability. Her counterfactual analysis suggests that the optimal policy is close to a full ban on executive NCAs.

To the best of our knowledge, the only two studies to examine the possible link between NCAs and the market for corporate control are Kobeissi, Sun, and Wang (2010) and Younge, Tong, and Fleming (2015). Kobeissi, Sun, and Wang (2010) view NCA enforceability as a governance tool in the context of acquirers' decision-making. They conduct a purely *cross-state* comparison and find that acquirers in states with stricter NCA enforcement are more likely to pay with cash, pay lower premiums, and exhibit higher announcement returns, with the latter two associations flipping sign for small acquirers. Like us, Younge, Tong, and Fleming (2015) study the association between changes in NCA enforceability and takeover likelihood. However, unlike our analysis, they examine only *one* episode of NCA enforcement change – the 1985 legislative change in Michigan – and find that greater enforcement of NCAs is associated with *higher* takeover likelihood. We use a comprehensive set of NCA reforms in our analysis resulting in the *opposite* conclusion regarding takeover likelihood, and we also examine the impact of NCAs on other important takeover outcomes.[7]

3 Institutional Background

This section provides institutional background for our empirical methodology, focusing on NCAs, their implications on executive mobility, and changes to their enforceability at the state level.

3.1 Executive Non-Compete Agreements

Non-Compete Agreements (NCAs) are contractual arrangements between employers and employees that restrict the employee's ability to work for or establish a competing business after leaving the company. The primary purpose of an NCA is to protect the employer's confidential information, trade secrets, customer relationships, or unique expertise, thereby preventing the employee from directly competing against the employer within a specific geographic area and for a certain period

[7]Somewhat further afield, Fich, Rice, and Tran (2016) consider NCA enforceability as one of the determinants of merger bonus grants to target CEOs during merger negotiations. They find that merger bonuses are more likely to be granted to target CEOs in states that recognize NCAs – likely because such bonuses often entail a non-competition clause.

of time, typically ranging from one to two years after the termination of employment. See Blake (1960) for a discussion of the legality of NCAs and how courts approach their enforcement.

NCAs are widely prevalent in the U.S. labor market. According to a comprehensive survey conducted by Starr, Prescott, and Bishara (2021), approximately 38.1% of the US labor force has signed an NCA at some point in their careers. These agreements are particularly common in positions that require specialized skills and knowledge, making them more prevalent among CEOs and high-level executives. Unlike rank-and-file employees, executives pose a greater threat to their former employers when joining competitor companies because they possess sensitive information. Bishara, Martin, and Thomas (2015) indicate that approximately 80% of CEOs are bound by non-compete restrictions, highlighting the significance of these agreements in the executive employment landscape. Furthermore, there has been a discernible increase in the usage of NCAs over time.[8]

NCAs are a distinctive legal practice that specifically impacts executive post-employment mobility compared to other agreements that protect a company's intellectual property and market share. These agreements may also be accompanied by Non-Solicitation Agreements, which restrict employees from soliciting the firm's employees, clients, or customers for a specified period, as well as Non-Disclosure Agreements, which prevent employees from disclosing confidential information to third parties. Although the general purpose of these contracts is similar, it is often more straightforward to determine whether a former employee continues working within a specific industry than to ascertain whether the employee is using confidential information. Appendix A provides examples of NCAs in CEO employment contracts. To ensure enforceability, NCAs must adhere to reasonable limitations, specifying criteria such as time frames, geographical boundaries, and a clear definition of the competing business.

The existence of NCA decreases executive mobility by limiting post-employment options. Garmaise (2011) highlights that increasing enforceability substantially reduces CEO mobility, particularly within-industry job changes. However, enforcing NCAs across state boundaries poses challenges compared to within-state enforcement.[9] In general, court decisions on NCA enforcement

[8]Kini, Williams, and Yin (2021) show that the use of NCA within CEOs increased from 42% to 67% between year 1992 to 2014. Similarly, Shi (2023) reports an increase in the use of NCAs by all executives from 57% in 1992 to 67% in 2015. Importantly, Shi (2023) also shows that NCA use is titled towards states that enforce them more strictly.

[9]The governing case is *Application Group, Inc. v. Hunter Group, Inc., 61 Cal. App. 4th 881, 72 Cal. Rptr. 2d 73 (1st Dist. 1998)* where the employee signed the NCA in Maryland, breached the agreements, and worked for a competing firm in California after resigning. The California court held that the NCA was invalid and unenforceable. A

can be influenced by factors such as choice-of-law provisions, which determine the applicable state law based on the relationship to the parties or the transaction and the state's materially greater interest. In *Sabol-Krutz v. Quad Electronics Inc.*, the employee moved from California to Michigan. The California court deemed Michigan law applies as the employee signed the NCA and worked in Michigan.

3.2 State-Level Enforceability

NCAs are governed at the state level, leading to significant variations in their usage across different states. Moreover, the different states themselves have seen changes to their enforcement regimes over time. To assess these changes in state-level enforceability, we use enforcement indices from three different sources: Bird and Knopf (2015) for the period from 1981 to 1991, Garmaise (2011) for the period from 1992 to 2004, and Kini, Williams, and Yin (2021) for the period from 2005 to 2013.[10] We then identify the month of the change within the year and follow the timing convention used in Jeffers (2023): if the reform takes place in the last three months of the calendar year, we assign the treatment year as the following calendar year.

The enforceability score is constructed based on twelve questions proposed by Malsberger (2002), with each question assigned one point if the enforcement of that perspective exceeds a certain threshold. The questions are designed based on several key criteria, such as the employer's protectable interest, the temporal and geographical restrictions within the covenant, and the court's ability to modify over-broad covenants.[11] A complete list of these questions can be found in Appendix B. The score ranges from zero to twelve, with higher scores indicating stricter enforcement of NCAs, and a score of zero means that the state does not enforce NCAs at all. Table 1 lists all of the changes in the scores across different states over time.

While most states maintain a constant level of NCA enforceability, 16 states experienced some changes in their enforceability levels between 1981 and 2013. Among these states, six states under-

choice-of-law provision in the contract was not binding, as the company failed to present evidence of damage. *Keener v. Convergys Corp., 342 F.3d 1264 (11th Cir. 2003)* Georgia court void the NCA under an Ohio choice-of-law provision because the term was overly broad.

[10] We incorporate one additional reform documented in Jeffers (2023) but not covered in Kini, Williams, and Yin (2021), specifically the Montana 2012 reform. None of our results and conclusions are affected by this given the very small number of firms/deals in Montana.

[11] For a discussion of this, see "How 'Red Penciling' and 'Blue Penciling' Affects Covenants Not to Compete?", available at `https://www.hg.org/legal-articles/how-red-penciling-and-blue-penciling-affects-covenants-not-to-compete-43946`.

went multiple changes, resulting in a total of 26 changes observed across states during the sample period. The most significant reform occurred in Michigan in 1985 when the Michigan Legislature repealed the statutory prohibition on NCAs through the Michigan Antitrust Reform Act, leading to a notable increase in the enforcement score from zero to five. Importantly, changes in enforceability generally affect all NCAs within the corresponding state, regardless of whether the agreements were entered into before or after the law change or court decision.

Figure 1, Panel A depicts the enforceability of NCAs across the U.S. in 2013, which is the last year of our sample period. The highest level of enforceability is observed in Florida, whereas California and North Dakota generally do not enforce NCAs.[12] Figure 1, Panel B provides an illustration of where (state boundaries in bold) and by how much (the color of the state) the enforcement scores have changed between 1981 and 2013. It is these changes that we utilize in our research design. In general, the level of NCA enforcement across the different states has gone up during our sample period.

We argue that NCA enforcement reforms can be considered as plausibly exogenous for the purposes of our study. In other words, we believe that NCA enforcement changes are uncorrelated with unobservable drivers of takeover activity and outcomes. Note that changes to NCA enforcement are the results of amendments to statutory laws or the state's higher court decisions that bind other courts. Although it is possible that state statutes may be influenced by lobbying from firms within the state, any such lobbying is likely driven by considerations of employee mobility and the protection of firms' intellectual property rather than by M&A considerations.

4 Sample and Research Design

Our takeover likelihood tests are conducted at the firm-year level using a panel of U.S. listed firms at the intersection of CRSP and Compustat databases (CRSP-Compustat panel hereafter). Our tests of deal-level outcomes such as hostility, offer premiums, and deal completion are performed on a sample of M&A deals targeting U.S. listed firms from the Thomson Reuters SDC Platinum database (M&A sample hereafter). Identifying targeted firms in the CRSP-Compustat panel also relies on the M&A sample. Below we discuss how we obtain these two samples, followed by a

[12] Oklahoma also does not enforce NCAs, but only if the employee does not solicit customers of the former employer. Since the end of our sample period, Minnesota also instituted a ban on NCAs in 2023.

discussion of our difference-in-differences research design.

4.1 M&A Sample

The M&A data come from the Thomson Reuters SDC Platinum, currently known as Refinitiv Workspace. We focus on acquisitions announced between January 1, 1981 and December 31, 2013.[13] The sample selection criteria are as follows:

1. Deal completion status is either completed or withdrawn.
2. Targets are U.S. public firms listed on NYSE, AMEX, or Nasdaq stock exchanges.
3. Acquirers are U.S. public companies (foreign bidders are excluded).
4. Transaction value is non-missing and at least $1 million USD.
5. The acquisition represents a change of control, where the acquirer initially owns less than 10% of the target and seeks to own more than 50% after the transaction.
6. Transactions classified as repurchases, self-tenders, recapitalization, restructuring, bankruptcy acquisitions, going private, or leveraged buyouts are excluded.

The use of these filters is consistent with standard practices in the M&A literature.[14] After applying these filters, we merge the M&A sample with firm characteristics from CRSP and Compustat, and we exclude target firms within the Real Estate Investment Trusts and Non-Operating Establishment industries (SIC code 6798 and 9995); the resulting pool of M&A deals consists of 6,049 transactions.

We further supplement the data with relevant state-level variables. Based on the target's historical state of headquarters location from SDC, we collect data on state GDP growth from the U.S. Bureau of Economic Analysis, as well as data on the passage of Inevitable Disclosure Doctrine (IDD) laws from Klasa, Ortiz-Molina, Serfling, and Srinivasan (2018). We also collect data on state antitakeover (business combination) laws from Karpoff and Wittry (2018). Since the latter applies

[13]The sample period begins in 1981 because Baker and Savaşoglu (2002) argue that "prior to 1981, SDC does not provide full coverage of mergers and acquisitions". Our sample period ends in 2013 because the NCA enforcement data we use ends in 2013.

[14]See, e.g., Fuller, Netter, and Stegemoller (2002), Moeller, Schlingemann, and Stulz (2004), Masulis, Wang, and Xie (2007), Golubov, Petmezas, and Travlos (2016), Dessaint, Eckbo, and Golubov (2024).

based on the target's state of incorporation, we use historical state of incorporate dataset from Spamann and Wilkinson (2019).[15]

We split our sample M&A deals into two categories – horizontal and unrelated – based on the firm's historical two-digit SIC code. In horizontal takeovers, where both the acquirer and target firms operate in the same industry, acquirer executives possess sufficient industry knowledge due to the similarities in business activities. Meanwhile, there may be overlap and redundancies within the merging company's management teams, where multiple executives perform similar roles and responsibilities. To improve efficiency, the acquiring company may choose to consolidate management teams, resulting in the replacement of the target company's CEO and top executives, who, in turn, become subject to NCAs and face outside option constraints after the takeover. Therefore, in our main analysis we focus on horizontal M&A deals only. Approximately 64% of the acquisitions in our sample are classified as horizontal. The "holdout" sample of non-horizontal deals is used for "placebo" tests, in which we expect little-to-no effects.

For all the deal-level tests, we exclude deals with multiple bidders since our outcome variables are not correctly defined for multiple-bidder contests (e.g., a given bid can be coded as withdrawn even if the target is actually sold to a different bidder; offer premiums for follow-up bids will not be measured accurately).[16] Our final sample consists of 3,005 horizontal M&A deals, although the sample size in each test varies depending on the availability of relevant dependent and control variables. Summary statistics are provided in Table 2. All continuous variables are winsorized at the 1st and 99th percentiles. Detailed definitions can be found in Appendix C.

Percentage premium is calculated using the offer price from SDC and the target stock price 21 business days prior to the acquisition announcement from CRSP. Values beyond the range of [0, 200] are winsorized as in Officer (2003). The mean *Premium* is 42%, with a median of 34%. *Hostile* is a dummy variable that takes the value of one if the deal is classified as hostile or unsolicited (vis-a-vis friendly) based on the *initial* reception. This is the case in 5.2% of the sample. *Withdrawn* is a dummy variable that equals one if the deal is withdrawn and zero if complete. Withdrawn deals comprise 13.1% of our sample. Additionally, 12.7% of the deals are classified as tender offers. To

[15]Historical state of incorporation data from Spamann and Wilkinson (2019) goes back to 1994. For firm-year observations prior to 1994, we back-fill the earliest available value from the Spamann-Wilkinson dataset. For firms never found in the Spamann-Wilkinson dataset, we use the state of incorporation from Compustat.

[16]This exclusion is not applied when identifying targeted firms in the CRSP-Compustat panel.

measure stock market reactions, we use standard event study methodology to calculate buy-and-hold abnormal returns (BHAR). In particular, we compute the BHAR from deal announcement to deal completion using the event window [Announcement Date -21, Completion Date] and the market return as the benchmark. The average target BHAR computed this way is 33%.

4.2 Panel of U.S. Listed Firms

Our panel of listed U.S. firms is an intersection of CRSP and Compustat databases. We start from all firm-year observations in the Compustat North America annual fundamentals file with fiscal-year end dates between January 1980 and December 2013. To identify listed firms, we merge these data with data from CRSP (using the *gvkey-permno* linking table from WRDS) and keep only those observations that correspond to CRSP *permno*s with share codes 10 and 11 (common stock). We then apply the same exclusions as to our M&A sample – this is to ensure that we do not keep takeover candidates that cannot possibly be found in our list of takeovers. Specifically, we exclude:

1. Firms whose country of location (Compustat item *loc*) is not USA.
2. Firms whose historical primary SIC code (Compustat item *sich*) is 6798 (Real Estate Investment Trusts) or 9995 (Non-Operating Establishments).
3. Firms not listed on NYSE, AMEX, or Nasdaq stock exchanges (i.e. we keep only CRSP exchange codes 1, 2, and 3).
4. Firms whose market capitalizations are below $1 million USD (market capitalization is defined as the common stock price at the fiscal year-end (Compustat item *prcc_f*) multiplied by the number of common shares outstanding (Compustat item *csho*).

To identify firms that have been targeted, we merge the resulting sample with our list of horizontal M&A deals. When a match is found, the firm-year observation in the panel is coded as a takeover attempt. In particular, the *Takeover* dummy takes the value of one if a firm is subject to a takeover attempt in a given year, and zero otherwise. Note that some of our M&A deals are ultimately completed and some are ultimately withdrawn. When the firm is subject to a withdrawn takeover attempt, subsequent observations for that firm are kept as the firm is eligible to be targeted

again. When a firm is subject to an ultimately completed bid, subsequent observations for that firm are dropped from the panel, as such firms are no longer eligible to be taken over according to our list of M&A deals.[17] There are approximately 166,000 firm-year observations in the resulting panel.

Table 3 presents the associated descriptive statistics. *Takeover* dummy has a mean value of 1.9%, suggesting that a firm in our sample has a 1.9% likelihood of being targeted in a horizontal takeover deal in a given year. The mean market capitalization of firms in our panel is $1.5 bil., although the median is only $100 mil. Consistent with the extant literature, the average leverage of the firms in our panel is 22.6%, and average cash holdings are 16.9%. Approximately 6% of the firms have dual-class shareholding structures.

4.3 NCA Laws: State of Headquarters

Unlike corporate law (such as Business Combination laws) that applies according to the firm's state of incorporation, employment contracts are governed by the laws pertaining to the location where the employee works. We randomly select and collect a sample of 80 firm-executive pairs from the SEC's EDGAR system, where explicit employment contracts were available. These contracts were typically attached in the 8-K, 10-K, or 10-Q filing exhibits and contained information on the state of the governing law. Within this sample, 64 disclosed the same governing laws and headquarters location, representing 80% of the total sample. This is consistent with the results presented in Kini, Williams, and Yin (2021), who conclude that most contracts are governed by the law of the headquarters state. Therefore, state-level NCA enforcement data is merged to our analysis samples according to the *historical* headquarters state of the firm involved. Specifically, in the M&A dataset we use the target's primary business location at the time of the deal provided by SDC. In the CRSP-Compustat panel we use the firms' historical headquarters locations collected from Gao, Leung, and Qiu (2021).

[17] The latter step is needed because some firms continue to report their financials in subsequent fiscal years while the deal is pending; certain targets are also not fully delisted and continue to report. For completeness, this filter includes post-takeover observations for targets in deals that we have screened out, namely unrelated deals, cross-border deals, and multiple bidder contests, since such firms also leave the pool of eligible targets upon a takeover.

4.4 Research Design

To identify the effect of executive career concerns associated with NCAs on the market for corporate control we use a difference-in-differences design, comparing how the outcome variable of interest changes in states that undergo NCA enforcement reforms, relative to states where no enforcement change takes place. In other words, we use intertemporal variation in the NCA enforcement score within states as a source of identifying variation. The main concern in difference-in-differences designs is that treatment *timing* may be endogenous, i.e. an NCA enforcement reform may coincide with changes in another factor that drives M&A activity. While a single treatment event may be prone to idiosyncratic confounding events, using multiple treatment events will serve to minimize their impact.[18] Since our natural experiment consists of multiple staggered treatments, we use both two-way fixed effect difference-in-difference regressions (TWFE) and a stacked difference-in-difference (Stacked DID) approach as in Gormley and Matsa (2011). We discuss these two approaches in turn.

4.5 Two-Way Fixed Effect Regressions

Our first estimation approach is a TWFE difference-in-differences regression. In particular, we run the following regression specifications estimated by ordinary least squares (OLS):

$$y_{ijst} = \beta \times \text{Score}_{st} + \theta X_{ijst} + \gamma_t + \delta_s + \nu_j + \varepsilon_{ijst} \tag{1}$$

where y_{ijst} is the outcome variables for firm/deal i in industry j state s and year t, $Score_{st}$ represents the NCA enforcement score in state s and year t, X_{ijst} is a vector of controls, and ε_{ijst} is the error term. γ_t, δ_s, and ν_j represent time, state, and industry fixed effects, respectively. Industries are defined based on SIC industry divisions.[19] We keep the definition of industries broad in order to preserve the sample of treated deals.

In all tests, statistical inferences are based on standard errors corrected for heteroskedasticity and double-clustered by state and year. The main coefficient of interest is β, which estimates the

[18] This still leaves the possibility of a *systematic* confounding event. For instance, NCA enforcement reforms could be passed in the context – or in anticipation – of particular economic conditions that affect takeovers. We view this as unlikely. Our tests of parallel pre-trends, as well as placebo tests using a holdout sample further serve to alleviate this concern (see Section 6).

[19] For more information, see `https://www.osha.gov/data/sic-manual`.

effect of changes in NCA enforcement on the takeover outcome in question.

4.6 Stacked Difference-in-Differences

The main criticism of the TWFE model for difference-in-differences designs with multiple staggered treatments is that previously treated units can serve as controls for future treatments. This is problematic when treatment effects are not immediate but rather take a number of periods to take force. Baker, Larcker, and Wang (2022) argue that the severity of the potential bias depends on the number of never-treated units in the sample, since these units are not contaminated. We note that the number of never-treated units in our CRSP-Compustat panel is over 62%, and in the M&A sample the same figure is over 63%. These figures actually understate the pool of valid controls, because not-yet-treated units are also uncontaminated. Figure 2 presents the cumulative percentage of treated observations in the M&A sample and CRSP-Compustat panel over time. Given that many of the reforms occur in the latter parts of our sample period, the fraction of valid controls in the sample never drops below 80%. Finally, the change in executives' behavior resulting from the change in NCA enforcement should be immediate rather than dynamic. Thus, we expect the bias in the TWFE estimation (if any) to be minimal.

To fully eliminate the possibility of "contaminated" comparisons we implement the stacked DID approach. First, we only keep the "clean" treatments where no additional reforms occur within the four years prior to and four years after the specific reform (this also excludes reforms occurring within the first or last four years of our sample period as these cases do not have a complete pre- or post-treatment period). Following this criterion, fourteen reforms remain.[20] Then, for each reform that we keep, we create a stack c of treated and control observations with a nine-year window centered around the treatment year (four years before and four years after). Within each stack, we use all the observations that are not-yet-treated (and will not become treated within the next four years) as the control group. Specifically, we perform the following regression estimated by OLS:

$$y_{ijstc} = \beta \times (\text{Treated}_s \times \text{Post}_t) + \theta X_{ijst} + \gamma_{tc} + \delta_{sc} + \nu_{jc} + \varepsilon_{ijstc} \tag{2}$$

[20] The remaining reforms are: Florida (1990), Florida (1996), Idaho (2008), Kentucky (2006), Louisiana (1990), Michigan (1985), Montana (1986), Oregon (2008), South Carolina (2010), Texas (1989), Texas (1994), Virginia (1992), Wisconsin (2009), and Wyoming (1994).

where $Treated_s \times Post_t$ is an indicator that equals 1 following an increase of the enforceability of the NCA, -1 after a decrease of enforceability, and 0 otherwise. To ensure that treated units of a given stack are compared only to the control units of that same stack, state, year, and industry fixed effects are unique for each stack. Namely, we include state $\times$ stack (δ_{sc}), year $\times$ stack (γ_{tc}), and industry $\times$ stack (ν_{jc}) fixed effects. As in the TWFE method, the standard errors are double-clustered by state and year.

While in the TWFE approach the coefficient of interest shows an effect of a 1-point change in the NCA enforcement score on the outcome variable, in the stacked DID approach the same coefficient shows the effect of an average NCA enforcement reform. Considering that some of these reforms entail a change in the NCA enforcement score of more than one point, we expect a larger magnitude of the estimated effect in the stacked DID approach as compared to the TWFE approach. A further difference between the two approaches is that the stacked DID approach (with a potentially cleaner control group) is based on a subset of 14 reforms out of the full set of 26 reforms in TWFE estimation, i.e. the trade-off is internal validity vs. external validity.

5 Empirical Results

5.1 Takeover Probability

This section investigates the impact of NCA enforcement reforms on the overall activity of the takeover market. As NCA enforcement tightens, executives face increased post-employment risk, resulting in higher personal costs. Consequently, target CEOs may counter potential takeovers by implementing anti-takeover defenses or precluding information sharing or discussions with the acquirer, effectively acting as barriers to takeover attempts. We therefore hypothesize that NCA enforcement tightening will influence the likelihood of firms being targeted in horizontal takeovers. Horizontal refers to cases where the acquirer and target belong to the same 2-digit SIC industry. This shared industry classification results in heightened similarity of business and executive functions between the two entities, and therefore increases the likelihood of job displacement for target firm executives. Conversely, unrelated mergers exhibit a higher probability of retaining existing management to ensure continuity of operations. Therefore, in the following sections, we concentrate all of our analysis on horizontal deals.

Table 4 presents the results of our analysis of horizontal takeover incidence in the CRSP-Compustat panel of U.S. listed firms using the linear probability model. The dependent variable in these tests is $Takeover_{ist}$, which takes a value of one if firm i in state s becomes the target of any horizontal deal within a given year t. In the first two columns, we employ the TWFE method. Column (1) reports the baseline estimate of the effect of the NCA Enforcement Score (*Score*) on the probability of a firm becoming a target. The main explanatory variable, *Score*, has a negative effect on the probability of a specific firm being targeted. The coefficient is -0.0006 and statistically significant at the 1% level.

In column (2), we incorporate firm-level and state-level controls, namely *Return on asset (ROA), ln(Size), Leverage, Book-to-Market, Dual Class Shares*, *Cash Holdings*, *GDP Growth, Business Combination laws, Inevitable Disclosure Doctrine (IDD)*[21]*, and In-state Competition*[22]. The inclusion of these controls results in a coefficient of interest of -0.0009, statistically significant at the 1% level. In terms of economic magnitude of this effect, a one-point increase in the NCA enforcement score is associated with a 5.2% reduction in takeover likelihood when evaluated relative to the average takeover incidence in the treated states prior to treatment. The coefficients on control variables are consistent with prior literature and economic intuition. For instance, firms with lower valuations (higher book-to-market ratio) are more likely to be targeted, consistent with Cremers, Nair, and John (2008) and Edmans, Goldstein, and Jiang (2012)). On the other hand, firms with dual-class shareholding structures are less likely to be targeted given the strong ability of insiders to resist takeovers.

In columns (3) and (4), we further examine the effect of NCAs on the takeover probability using the stacked DID method. Consistent with the TWFE results, the findings reveal a significant negative relationship between the reforms that tighten NCA enforcement and the takeover likelihood. In column (3), an average reform is associated with a 0.24 percentage point decrease in takeover probability, significant at the 5% level, which represents a 17.35% decrease relative to the unconditional sample mean within the treated states prior to the initial reform. In column (4), we include firm- and state-level controls, and the coefficient is little changed and is now significant at

[21]IDD is a legal doctrine positing that a former employee may be enjoined from engaging in employment with a competing firm if the employee would inevitably divulge the originating firm's confidential trade secrets.

[22]Following Garmaise (2011), *In-state Competition* is defined as the fraction of total industry sales generated by in-state competitors, excluding the firm itself.

the 1% level. Overall, these findings demonstrate that increases in NCA enforcement are associated with reductions in takeover likelihood, consistent with executives' aversion to takeovers.

Recall that another channel through which NCA enforceability could affect the takeover market is securing the value of the target that derives from its human capital. If employee mobility is reduced when NCA enforcement tightens, this makes "acquihires" more viable and should promote acquisition activity. Gaining access to human capital is unlikely to motivate acquisitions of large listed firms, but it could be an important motivation in acquisitions of privately held targets, particularly nascent firms and firms in the high-tech industries. We therefore examine deal activity levels for non-public targets. Note that we do not observe the universe of unlisted firms, so we cannot estimate takeover likelihood models, but we can examine how aggregate deal numbers respond to NCA enforcement changes.

To that effect, we conduct another trawl of the Thomson Reuters SDC database and gather a sample of all acquisitions of privately-held firms. This time we do not impose any filters on deal size, since "acquihires" could be very small transactions – to a point where the acquirer is not required to disclose transaction value. We then collapse the data at the state-industry-year level, such that the unit of observation becomes the number of deals in a given industry of a given state in a given year. We then use our two estimation approaches – TWFE and stacked DID regressions – to analyze how deal activity (in logs) changes in response to changes in NCA enforcement. Naturally, we cannot control for target characteristics in these tests, but we continue to control for state-level characteristics and the fixed effects structure is the same as before.

Columns (1)-(4) of Table 5 report the results of this analysis. We find no significant effects of NCA enforcement changes on the aggregate private firm acquisition activity. All coefficients are close to zero. In the remaining columns of Table 5 we narrow down the type of private firm acquisition activity that we consider – in an attempt to further zero-in on deals that could be motivated by access to human capital. In columns (5)-(8) we consider only small private firm acquisitions, defined as deals whose transaction value is below US$50 million or not reported at all. We continue to find no effects of NCA enforcement changes on these types of deals. Finally, in columns (9)-(12) we consider only private acquisitions of firms in the high-tech industries. High-tech industries are defined following Loughran and Ritter (2004). Given the focus on a small set of narrow industries, this time deal numbers are aggregated only at the state-year level and SIC

division fixed effects are omitted. Once again, our inferences are unchanged: the coefficients of interest are close to zero.[23]

Overall, we do not find evidence that greater NCA enforcement promotes acquisition activity via the "acquihire" channel. One reason for this lack of correlation could be the fact that NCAs are less prevalent below the management ranks. Therefore, other determinants of labor mobility may be more relevant for the "acquihire" channel. For instance, consistent with Chen, Gao, and Ma (2021), the coefficient on IDD laws in Table 5 is positive in the TWFE regressions – a state's recognition of the Inevitable Disclosure Doctrine is associated with increased levels of private firm acquisition activity.

5.2 Deal Attitude

So far we have demonstrated that stricter NCA enforcement decreases the likelihood of being targeted in a horizontal deal. For those takeover attempts that do materialize, we investigate whether NCA enforcement has an impact on the deal's initial attitude in terms of hostility. Given the significant job displacement probability associated with horizontal takeovers, executives may become more averse to such transactions, leading to a more negative (hostile) attitude toward the deal. By exploring this dynamic, we aim to shed light on how changes in NCA enforcement influence the overall deal landscape and the decision-making process of target firms.

Our dependent variable in these tests is *Hostile* – an indicator taking the value of one if the deal's initial reception is classified as "hostile" or "unsolicited" by SDC, and zero otherwise. Since the initial reception variable is well populated only from the year 1985 onwards, for observations prior to 1985 we use the final deal attitude and search for the same terms. Table 6 presents the results. Columns (1) and (2) present the TWFE results. The baseline result in column (1) shows an estimated coefficient of 0.0535, which is statistically significant at the 1% level. After adding controls in column (2), the estimated coefficient of interest becomes 0.0469 and is still significant at the 1% level. Specifically, a one-point increase in the NCA enforcement score is associated with a 4.69 percentage-point increase in the likelihood of non-friendly deal attitude. This effect

[23] As an alternative measure of acquisition activity, we take advantage of our CRSP-Compustat panel and examine acquisition expenditures reported on the firm's cash flow statement (Compustat item *aqc*). While this has the benefit of capturing all acquisition spending, regardless of reporting thresholds, we cannot distinguish target types (public or private) as well as the location of the target. We find no effects of NCA enforcement changes on the firms' total acquisition expenditures.

is economically large, representing a 74% increase relative to the average level of hostility in the reforming states in the pre-reform period (6.3%). In columns (3) and (4) we present the stacked DID estimation results. Both the baseline result and the result after incorporating control variables report a coefficient of around 0.13, significant at the 1% and 5% level, respectively. The economic significance of the effect in the stacked DID specification is substantial: the average NCA-tightening reform is associated with a tripling of the fraction of hostile deals. Thus our results support the hypothesis that the increased cost of executive turnover leads to greater reluctance among executives to consider takeover offers, thus fostering a more hostile attitude towards horizontal takeover bids.

5.3 Takeover Premiums, Target Gains, and Acquirer Gains

In this section, we further examine the effect of NCA enforcement changes on the takeover premiums, as well as and target and acquirer buy-and-hold abnormal returns (BHAR). Target executives subject to stricter NCA enforcement may bargain more aggressively with their bidders, thereby increasing acquisition premiums. First, demanding a higher takeover premium may serve to discourage the bidder from proceeding with the deal. Second, executives could seek to offset their personal costs by benefiting from the increase in the value of their shareholdings in the target, if any. Table 7 presents the estimation results for the offer premiums.[24]

Column (1) presents the baseline estimate of the effect of NCA enforcement changes on takeover premiums using the TWFE method. The coefficient estimate indicates that, on average, a one-point increase in *Score* is associated with an increase in the offered premium of approximately 4.5 percentage points, statistically significant at the 1% level. Column (2) adds deal-level, target-level, and state-level control variables to the specification. The estimated coefficient is little changed at 4.73 and still significant at the 1% level. In terms of economic significance, this coefficient implies that a one-point increase in the NCA enforcement score is associated with a 12% increase in premiums when evaluated relative to the average premium in the treated states prior to treatment. The coefficients on control variables are in line with existing studies. For instance, similar to Officer

[24]For roughly 10% of the deals in our sample, the formal announcement of the deal is preceded by revelations of takeover discussions – SDC data fields *"Date Announced"* and *"Date Originally Announced"* do not coincide. In cases where takeover discussions are previously disclosed, the premium is likely to be mismeasured, as the target stock price will incorporate expectations of an impending deal. Indeed, we find that measured premiums in such cases are substantially lower: average of 27% versus 42% normally. To ensure that we measure premiums relative to a truly unaffected stock price, our premium tests exclude observations where the difference between *"Date Announced"* and *"Date Originally Announced"* exceeds 4 weeks.

(2003), we find that tender offers are associated with significantly higher takeover premiums, while larger targets are associated with significantly lower premiums. We also find that premiums increase with target leverage, cash holdings, and book-to-market ratios.

Columns (3) and (4) provide the stacked DID estimates. As is the case with all our prior tests, the magnitude of the estimated effect in the stacked DID approach is higher. An average reform is associated with an 18 percentage point increase in takeover premiums, which is highly economically significant. Overall, our findings in this section are consistent with the hypothesis that executives in states that tighten NCA enforcement attempt to resist takeovers through demanding higher premiums, leading to more favorable terms for target shareholders.

To corroborate the results on takeover premiums, we also consider the target firm's buy-and-hold abnormal returns. To that end, we define *Target BHAR* [−21, *completion*], which is the market-adjusted buy-and-hold return of the target firm stock over the period from twenty-one days before the announcement of the deal to the completion day of the deal (naturally, these tests are limited to completed deals). Columns (1) to (4) of Table 8 present the results. Our findings here mirror those for takeover premiums in both magnitude and statistical significance.

For completeness, we also examine acquiring firm abnormal returns, *Acquirer BHAR* [−21, *completion*], defined the same way. On the one hand, to the extent that stricter NCA enforcement is associated with higher premiums paid, we might expect acquirer gains to be negatively affected. On the other hand, stricter enforcement of NCAs might benefit acquirers through better protection of the acquired firm's intangible assets. The net effect of NCA enforcement on acquirer returns is therefore an empirical matter. Columns (5) to (8) of Table 8 present the results, showing that NCA enforcement changes are not associated with changes in acquiring firm abnormal returns.

Overall, we conclude that, conditional on the takeover bid materializing and completing, stricter NCA enforcement benefits target firms through higher takeover premiums – likely a result of greater resistance from target firm management, but also consistent with greater viability of "acquihires". At the same time, acquirers do not appear to lose out.

5.4 Deal Withdrawal

Our final outcome of interest is whether an announced deal is completed or withdrawn. We have shown that, as NCA enforcement tightens, deals are more likely to be hostile and entail higher

takeover premiums. This negative sentiment may manifest in a higher deal withdrawal rate, as target management may be more inclined to resist the proposed takeover and bidders are put off by higher required premiums. We therefore examine the effects of NCA enforcement changes on the deal withdrawal rate.

The dependent variable *Withdrawn* is a binary variable that equals one if the deal is withdrawn and zero if it is completed. Table 9 presents the results. Columns (1) and (2) report the TWFE estimates. The coefficient in column (1) is 0.0536 and significant at the 1% level, indicating that a one-point increase in the NCA enforcement score is associated with an approximately 5.4 percentage-point increase in the incidence of deal withdrawal. Given that the average withdrawal rate in the reforming states in the pre-reform period is 15.5%, the estimated effect is a sizeable 34% relative increase. Column (2) repeats the analysis with deal-level, target-level, and state-level control variables included and shows that the coefficient is largely unchanged at 0.0586 – a 5.9 percentage point increase for a one-point increase in the NCA enforcement score. In terms of control variables, we find that cash offers are more likely to be withdrawn, while tender offers are less likely to be withdrawn.

Columns (3) and (4) repeat the analysis using the stacked DID approach. Consistent with all prior results, we estimate a larger effect in the stacked DID regressions: an average NCA enforcement reform is associated with an 11-13 percentage point increase in the likelihood of deal withdrawal. Overall, the results of these tests suggest that as NCA enforceability tightens, proposed M&A deals are more likely to be withdrawn, likely due to heightened resistance from target executives concerned about job displacement risks.

Recall that greater enforcement of NCAs reduces the mobility of the target workforce more generally, thereby reducing the likelihood that key target employees voluntarily leave upon deal announcement. This could have a positive impact on deal completion rates in acquisitions motivated by gaining access to specialized labor. We therefore once again examine private firm acquisitions – both overall and the subsets of small private targets and high-tech private targets (where "acquihires" are more likely). Unreported for brevity, we find no effect of NCA enforcement changes on deal completion rates in those samples.

6 Further Tests and Discussion

6.1 Analysis of Pre-Trends

The main identifying assumption behind a difference-in-differences research design is that of parallel trends, i.e. that the outcomes of interest in the treated and control states would have evolved in a similar fashion in the absence of treatment. While this parallel trends assumption is inherently untestable – it is a statement about the counterfactual that we do not observe – a certain level of comfort could be gained from observing similar evolution of the outcomes *prior* to treatment, i.e. the so-called parallel pre-trends. We therefore implement the dynamic version of the difference-in-differences test, in which we estimate separate coefficients for each of the pre- and post-treatment periods relative to the omitted reference period. Given that the same state can experience multiple reforms, our stacked DID approach – which requires a "clean" window around the reform – is more suitable for such dynamic difference-in-differences specification.

Note that acquisitions are relatively rare events, such that a given state could experience very few transactions in a single pre- or post-treatment year. This would result in the coefficient for a given year (or the benchmark level of the outcome variable in the reference year) being estimated from just a handful of observations. To circumvent this small-sample problem, we bundle adjacent years and estimate separate coefficients for each two-year period.[25] The two-year period for the years $-1/-2$ serves as the reference period, and we exclude year 0 from our nine-year event window to ensure equal-sized periods. In particular, we estimate the following regression, where D_k is a dummy for two-year periods either prior to or following the NCA enforcement reform and β_k are the coefficients of interest:

$$y_{ijstc} = \sum \beta_k \times (\text{Treated}_s * \text{D}_k) + \gamma_{tc} + \delta_{sc} + \nu_{jc} + \varepsilon_{ijstc} \tag{3}$$

Figure 3, Panel A plots the period-by-period coefficients and the associated confidence intervals of the dynamic stacked DID specification for takeover likelihood as the outcome variable. The evolution of takeover likelihood between the years $-3/-4$ and years $-1/-2$ (the reference period) in the treated states is similar to that of the control states: the dynamic DID coefficient for the period

[25] Our use of two-year periods should not be confused with "binning" of distant relative-time periods criticized by Baker, Larcker, and Wang (2022).

$-3/-4$ is not statistically different from zero. In other words, we observe similar trends in terms of takeover likelihood prior to the reform. In contrast, we observe a divergence in outcomes starting with the two-year period for the years $+1/+2$, whereby firms in the treated states experience an incremental decline in takeover likelihood (relative to the reference period) as compared to the firms in control states. The effect persists into the two-year period for the years $+3/+4$, albeit the confidence interval for the latter coefficient is wide.

We repeat the same test for initial hostility as the outcome variable and plot the period-specific treatment coefficients in Panel B. Once again, we observe similar pre-trends: initial hostility for the deals occurring in the treated and control states is on the same path when comparing the periods for the years $-3/-4$ and years $-1/-2$. This is followed by a relative increase in initial hostility for the deals in the treated states in the period for the years $+1/+2$ as well as the subsequent two-year period. Both post-treatment coefficients are statistically different from zero. Panel C plots the coefficients from the same test for takeover premiums as the outcome variable and reveals a perfectly parallel pre-trend, followed by a sustained relative increase in premiums for transactions taking place in the treated states in the subsequent periods. Finally, Panel D reveals the same pattern for deal withdrawal rates: no differential pre-trends, followed by a sustained increase in deal withdrawals for transactions in the treated states, albeit the confidence interval for the final period (years $+3/+4$) marginally straddles zero.

Overall, the results of these tests confirm that the effects we document kick-in after the associated NCA reforms and are not due to the continuation of pre-existing trends. This provides some comfort that the changes in takeover outcomes we document are actually due to the reforms we study. Of course, there is still a possibility that the timing of NCA reforms coincides perfectly with changes in other drivers of takeover outcomes, i.e. the *timing* of the reforms is endogenous. To address this possibility, below we examine treatment effect heterogeneity that would be expected given the hypothesized channel for the documented effects, but would not be expected if our results are simply due to omitted time-varying drivers of takeover outcomes.

6.2 Heterogeneous Treatment Effects by CEO Proximity to Retirement

The hypothesized channel behind our results is managers' resistance due to concerns for their post-takeover employment. If the results we document are indeed due to this, then we would

expect weaker effects for managers who are less concerned about finding subsequent employment – such as managers approaching retirement age. We therefore explore heterogeneous treatment effects according to CEO age. Testing this prediction is a non-trivial task, because data on CEOs is generally available only for very large firms. For instance, Execucomp data is limited to S&P 1,500 firms, whereas takeover targets tend to be smaller companies. To overcome this challenge, we compile data on CEOs from three different sources: Execucomp, Boardex, and the extended CEO turnover dataset from Fee, Hadlock, and Pierce (2013). Pulling together the three sources, we are able to obtain data on CEO age for almost two-thirds of our original CRSP-Compustat panel (which includes both targets and non-targets), but only for a third of our M&A sample. As mentioned above, targets tend to be smaller firms that are not covered in any of our data sources. To avoid further splitting relatively rare outcomes (e.g., hostility, deal withdrawal) in the M&A sample, we focus on the takeover likelihood in the CRSP-Compustat panel.

To that end, we define an indicator variable *CEO Age* $\geq$ *60* taking the value of one for CEOs aged 60 and above, and zero otherwise. We then include it in our regressions and interact it with our main independent variables of interest – *Score* in the TWFE approach and *Treated* x *Post* in the Stacked DID approach. If CEOs close to (or already in) retirement age have fewer concerns for their subsequent employment, then we expect them to be less sensitive to changes in the NCA enforcement regimes in terms of resisting takeovers as compared to younger CEOs. We therefore expect a positive coefficient on the interaction term. The results reported in Table 10 confirm our conjecture: the coefficient on the interaction term is indeed positive and statistically significant in both TWFE and Stacked DID approaches. That is, we see a muted effect of NCA enforcement reforms on takeover likelihood for CEOs aged 60 and above. This result is consistent with our hypothesized channel and reduces the concern about possible omitted variables driving our main effect.

6.3 Non-Horizontal Deals as Placebo

In our analysis so far we have focused on horizontal takeovers where the acquirer and the target come from the same general industry. Since operational overlap and acquirer's familiarity with the target's business are higher in such deals, the likelihood of executive redundancies at the target is also higher. To the extent that diversifying (non-horizontal) deals pose a lower threat of

dismissal for executives, we would expect executives to exhibit lower aversion to such takeovers, if any. Therefore, if our findings above are indeed driven by executive career concerns due to NCAs, the effects of NCA enforcement changes on diversifying deals should be less pronounced or not present at all. If, on the other hand, our findings are driven by some omitted variable correlated with both the timing of NCA enforcement changes and takeover outcomes, we would expect the same findings among diversifying deals.

We repeat all of our main tests using a "holdout" sample of non-horizontal takeovers – those where the 2-digit SIC code of the target is different from that of the acquirer. For the sake of space, these results are reported in Appendix D, Table A1. We find no robust effect of NCA enforcement changes on diversifying deals. Across our four main outcome variables, only deal withdrawal has the same positive response to NCA enforcement changes as in our analysis of horizontal deals – but only in the stacked DID approach. Interestingly, we find a significantly *negative* effect for initial hostility, i.e. tighter NCA enforcement appears to be associated with fewer hostile deals among non-horizontal takeovers; however, this effect is also not robust across the two estimation approaches. In all other cases, we find no effect of NCA enforcement changes on diversifying deals. Overall, the results of these "placebo" tests are consistent with executive self-interest as the channel behind our main results for horizontal deals. This raises the bar for any alternative explanation of our findings, including those involving omitted variables (i.e. any such alternative explanation would have to predict a differential effect for horizontal versus diversifying deals).

6.4 NCA Enforcement Changes and Concurrent Firm Fundamentals

In our penultimate set of tests we return to the overall panel of firms and consider whether changes to NCA enforcement coincide with changes in key firm fundamentals. If this were the case, this could signify changes in the characteristics of firms available for takeovers and could potentially explain some of the changes to deal outcomes that we document. To that end, we relate NCA changes to contemporaneous book-to-market ratio, profitability (ROA), size, leverage, and cash holdings of all firms in the corresponding states. Appendix D, Table A2 shows that none of the firm characteristics we consider is correlated with NCA enforcement changes in a consistent manner across specifications. This gives us comfort that our main findings above are not driven by a change in the composition of firms available for takeover and that executives' self-interest is the

likely driving force behind the associations we document.

6.5 NCA Enforcement Changes and Anti-Takeover Defences

In our final set of tests, we consider another way in which takeover resistance could manifest itself. In particular, managers subject to more stringent non-competes could be putting forward proposals for the adoption of anti-takeover defenses in their firms' charters and bylaws. To investigate this possibility, we complement our CRSP-Compustat panel with data on the main firm-level anti-takeover provisions identified by Bebchuk, Cohen, and Ferrell (2009). Specifically, we use RiskMetrics to gather data on the E-index and its six components: staggered (classified) boards, poison pills, golden parachutes, supermajority provisions, limits to amend charter, and limits to amend bylaws. The E-index counts the number of the aforementioned provisions that are present and has a maximum value of six; the individual components are indicator variables. In Table A3 of Appendix D we report results of the analysis in which we run our TWFE and Stacked DID regressions with the composite E-index and each of the six component dummies as dependent variables. Interestingly, we find no robust significant effects of NCA reforms on the adoption of anti-takeover defenses.

A limitation of the above analysis is that data on anti-takeover provisions is available only for large firms (coverage approximates constituents of S&P 1,500) and only from 1990 onwards. As a result, it is possible that our tests here lack power. In an attempt to broaden the cross-section in this analysis, we take advantage of data from Guernsey, Guo, Liu, and Serfling (2023), who use a combination of machine learning, textual analysis, and manual inspection to generate an extended dataset of staggered board provisions. This more than doubles our sample size for this particular anti-takeover defense. Nevertheless, in the results reported in the last panel of Table A3 we continue to find no significant effects on the adoption of staggered boards. Note that it is still possible that managers would like to adopt anti-takeover defenses but are simply unable to gain shareholder support that is required.

6.6 Discussion

Overall, our findings indicate that the enforceability of NCAs is associated with executive resistance to horizontal takeovers. A question that naturally arises is whether such aversion to takeovers

results in a sizeable agency cost for the target firm shareholders. In other words, are shareholders better off or worse off due to the impacts of NCAs on takeover activity and outcomes? The answer to this is not *a priori* clear. With takeover resistance taking different forms – fewer takeover attempts, fewer completed deals, but higher premiums – the net effect on target shareholders remains to be seen. Here we attempt a back-of-the-envelope calculation of the net impact of NCAs on target shareholder wealth via the takeover aversion channel.

Target shareholders benefit from premiums received when takeovers are completed. Hence, a reduction in takeover likelihood and a reduction in deal completion rates due to executive resistance have a negative impact on target shareholder wealth. On the other hand, an increase in takeover premiums as a result of executive resistance has a positive impact on target shareholder wealth. To compute the effect of changing NCA enforcement on target shareholders, we proceed in three steps. First, we compute the actual gain enjoyed by target shareholders in the completed deals that we observe in our sample. Second, using our reduced-form estimates, we impute the amount of completed takeovers, the associated premiums and the resulting shareholder gain that would be observed upon changing NCA enforcement. Third, we compare the counterfactual target shareholder gain estimated in the second step to the actual gain computed in the first step.

Table 11, Panel A summarizes the key inputs into this analysis, namely the sensitivities of three pertinent takeover outcomes to a one-point change in the NCA enforcement score, as well as the median target firm size (the latter is for illustration only, our analysis is ultimately size-agnostic). Panel B presents the estimates of target shareholder wealth in three scenarios. Column (1) depicts the actual scenario. There are 3,005 horizontal takeovers in our sample, of which 13.1% were withdrawn, resulting in 2,611 completed deals. The mean takeover premium observed in our sample is 42%. With the median target firm market capitalization of $124 million, 2,611 completed deals at a premium of 42% result in a shareholder value gain of $135.86 billion.

Column (2) depicts the scenario in which NCA enforcement score is increased by one point. In such case we would expect to see 2,863 takeovers, of which only 2,320 would complete given an increase in the deal withdrawal rate. At the same time, the average takeover premium would increase to 46.7%. Holding the target size constant, such level of deal activity would generate a target shareholder gain of $134.29 billion. Comparing the actual and the counterfactual values between columns (1) and (2), a one-point increase in the NCA enforcement score results in *lower*

target shareholder wealth. The reduction is $1.57 billion, which is a decline of 1.16%. The forgone takeover premiums in deals that would not materialize and in those that would not close more than offset the increase in takeover premiums in deals that would complete. Thus, target shareholders would be worse off if NCA enforcement became stronger. It is tempting to conclude that target shareholders would be better off if NCA enforcement became weaker – but the counterfactual value change is neither symmetric nor linear given the multiplicity of effects, necessitating an explicit calculation.

In column (3) we examine a second counterfactual scenario in which the NCA enforcement score is lowered by one point. Here, we would observe more takeover attempts (3,147) and relatively more of them would complete (2,919), but average offer premiums would decline to 37.3%. The shareholder gain from premiums in completed deals would be $134.79 billion. Relative to the actual shareholder gain in column (1), this is *also* a reduction, on the order of $1 billion or just under 0.8%. If NCA enforcement were weakened, reduced premiums in completed deals would more than offset the fact more deals would be attempted and completed. Therefore, the counterfactual analyses using our reduced-form estimates suggest that the current level of NCA enforcement could be close to optimal from the point of view of shareholder wealth insofar as it derives from takeovers. In other words, the current setup could be such that minimizes the potential agency costs from the misalignment of executive and shareholder incentives from the use of NCAs in regards to takeovers.

Note that the above analysis is focused exclusively on the shareholder wealth effects associated with executive incentive misalignment in regards to value-increasing takeovers. It does not take into account the effect of NCAs on shareholder wealth through more direct channels, such as potentially benefiting the firm through better protection of its intellectual property, resolution of the hold-up problem associated with investments in intangible assets, or lower wages paid. Such overall impact of NCAs on firm shareholders is beyond the scope of this study.

The combined effects of NCA enforceability on a broader set of stakeholders are even more complex. Nevertheless, the results of our analysis speak to the potential effects of the recent initiative by the U.S. Federal Trade Commission to make NCAs unenforceable nationwide. Presumably, the intent of this regulatory action was to allow labor to move more freely between firms, thereby increasing employees' bargaining power and reducing monopsonistic rents earned by employers. However, to the extent that absence of NCAs increases horizontal M&A activity – our headline

conclusion – one could expect the FTC's initiative to actually increase the level of consolidation in labor as well as product markets. Such consolidation could work against the regulator's stated goal of promoting competition.

7 Conclusion

We examine the impact of executive career concerns on the market for corporate control, focusing on the role of non-compete agreements in M&A deals. We hypothesize that stricter enforcement of NCAs imposes greater personal costs on executives, making them more averse to the type of takeovers that can result in their displacement. Using changes in NCA enforceability across the U.S. states over the period from 1981 to 2013, we show that increased enforceability results in decreases in the probability that a firm will be taken over by firms within the same industry. This effect is muted for firms run by CEOs close to or already in retirement age, who arguably face lower costs of potential dismissal.

Further evidence of aversion/resistance to takeovers can be gleaned from the characteristics and outcomes of takeover attempts that do materialize. In particular, greater enforceability of NCAs is associated with more hostile deal attitude. Stricter enforcement of NCAs is also associated with significantly higher takeover premiums. In addition, more enforceable NCAs are associated with significantly higher withdrawal rates for announced deals. Consistent with the above effects being driven by executive career concerns, we find no robust effects of NCA enforcement changes in a sample of non-horizontal takeovers where the likelihood of executive job losses is smaller.

Overall, our findings suggest that executive career concerns are a significant driver of the market for corporate control. While the use of NCAs might benefit firms by protecting their proprietary information and encouraging investment in knowledge assets, our results imply that an unintended consequence of the use of these agreements for executives is a heightening of agency conflicts when it comes to takeovers. At the same time, our reduced-form counterfactual analysis suggests that the current level of NCA enforcement could be such that it actually minimizes the associated agency costs.

More broadly, our results contribute to the body of evidence on the benefits and costs of this type of contractual agreement for the economy. Our findings help inform the debate on the current

proposal by the U.S. Federal Trade Commission to institute a national ban on the use of NCAs. In particular, our analysis would suggest that such a ban might indirectly promote consolidation in labor and product markets through more horizontal M&A – likely not the intended goal of the regulator.

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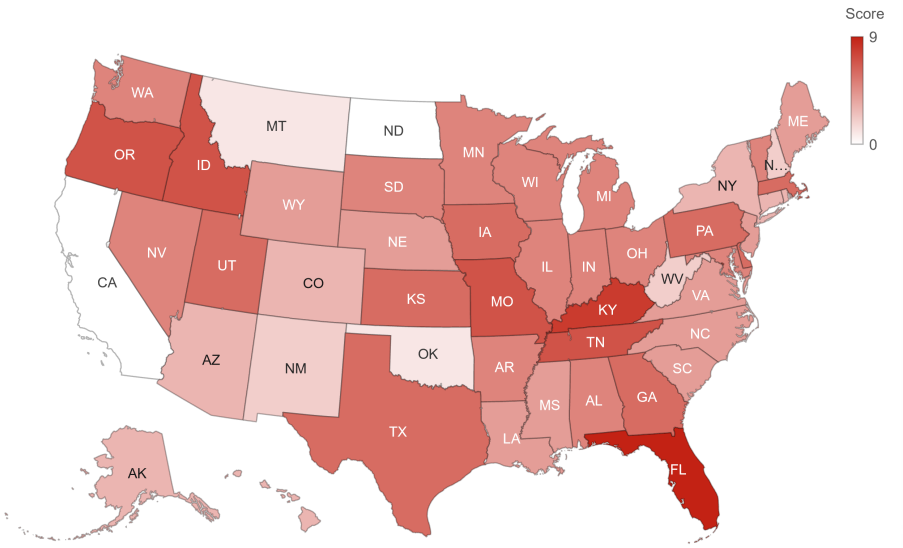
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Figure 1: Enforcement of NCAs in the U.S.

Panel A: NCA Score in 2013



Panel B: Cumulative Change in Score between 1981 and 2013

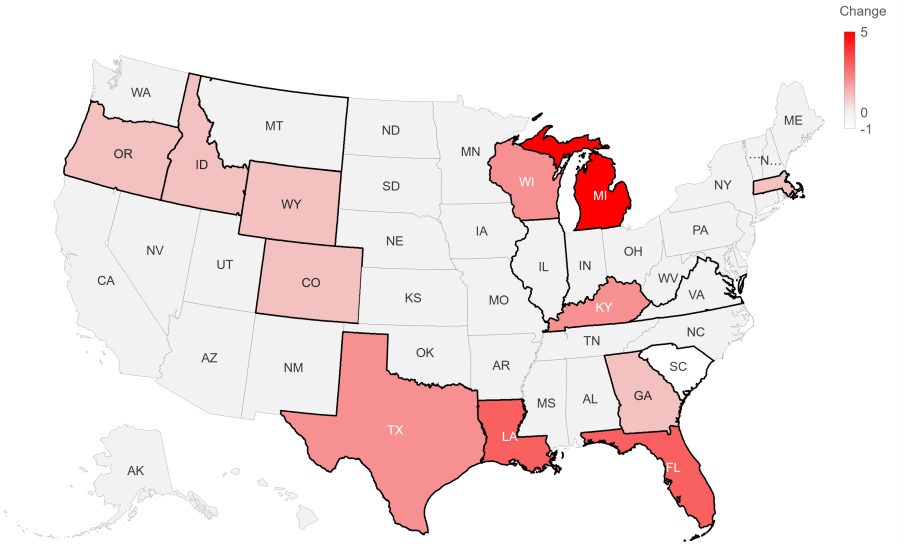


Figure 1: Panel A shows the NCA enforcement score across the U.S. as of 2013. Scores can range from 0 to 12, the highest score observed in our sample is 9. Darker colors indicate higher values. Panel B shows the cumulative change in state-level NCA enforcement scores from 1981 to 2013. Bold state borders indicate states that experienced at least one change. Colors indicate the extent of the cumulative change.

Figure 2: Cumulative Percentage of Treated Observations

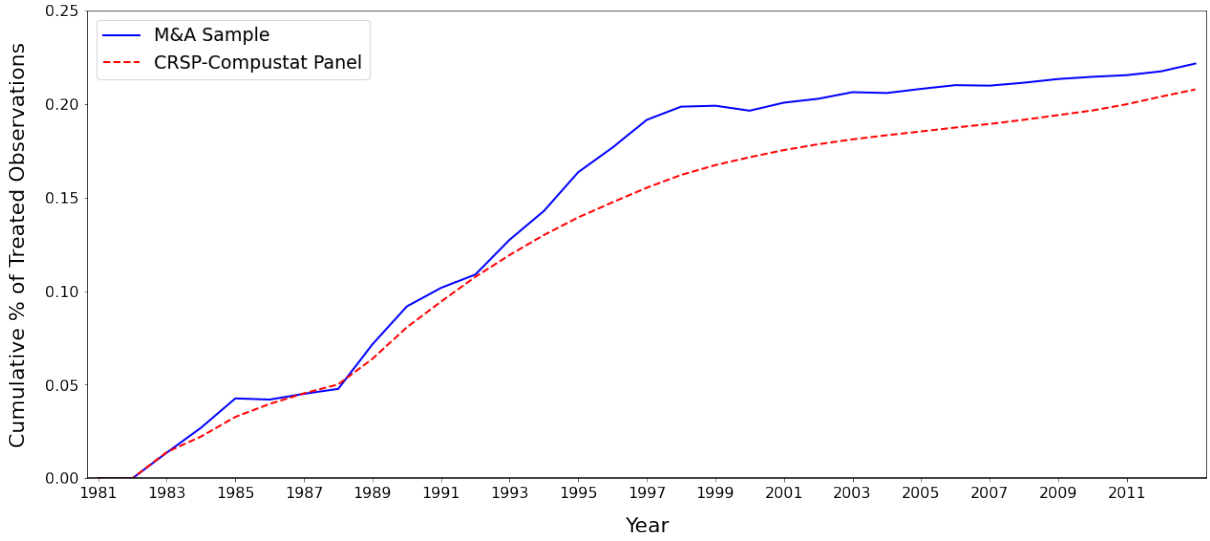
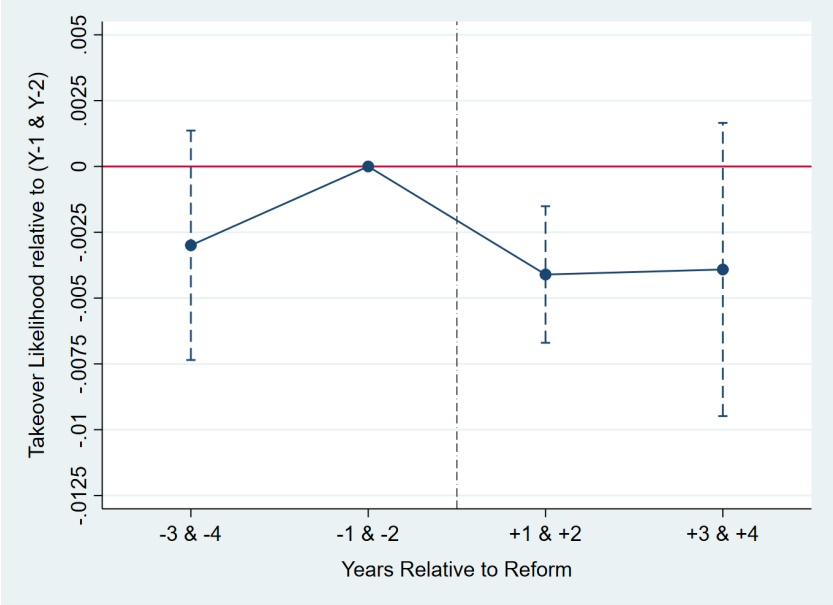


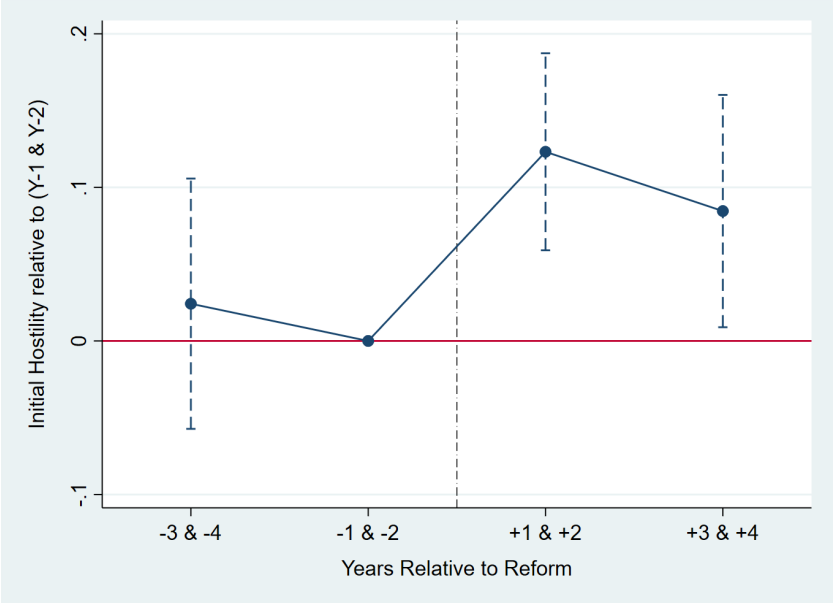
Figure 2: This figure presents the cumulative percentage of treated observations within the M&A sample and CRSP-Compustat panel over time. The ratio is computed by dividing the number of observations pertaining to already treated states by the total number of observations up to that year.

Figure 3: Dynamic Event Study DID Coefficients

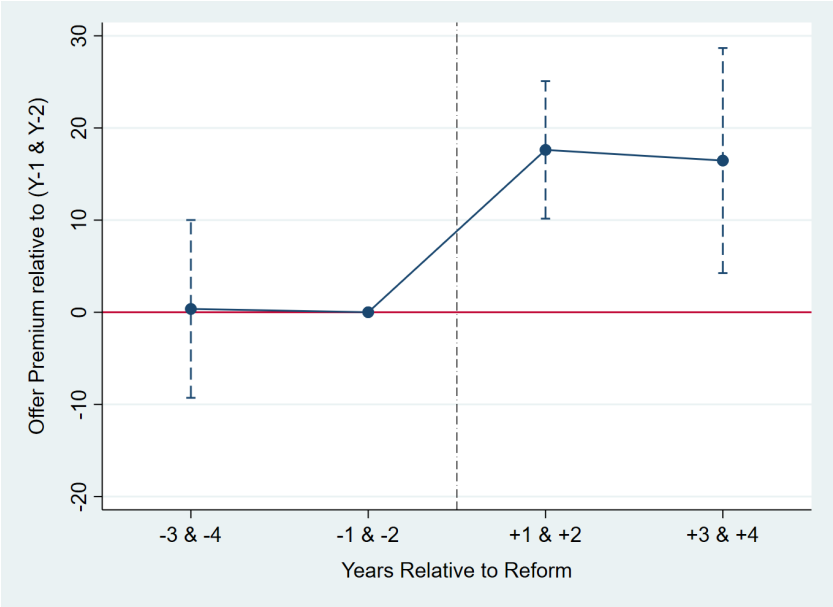
Panel A: Takeover likelihood



Panel B: Deal attitude



Panel C: Offer premium



Panel D: Deal withdrawal

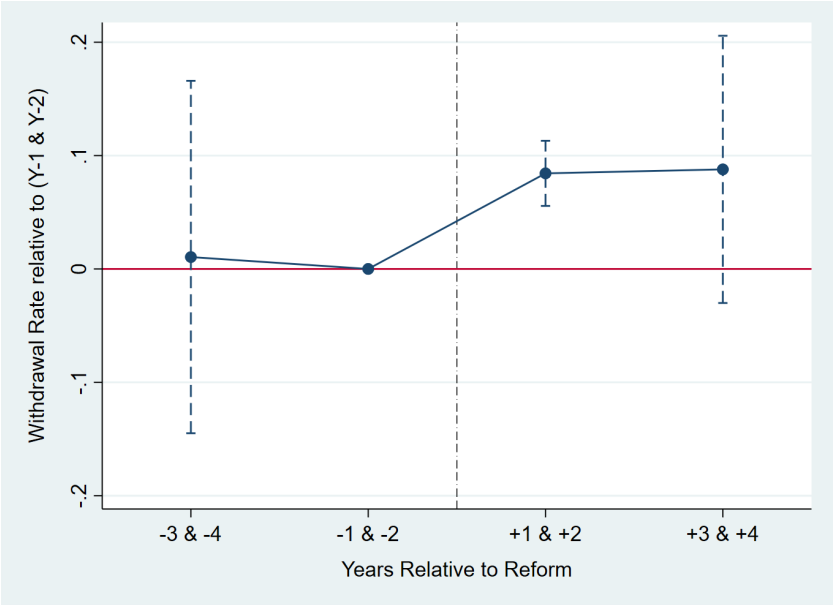


Figure 3: This figure plots the period-specific treatment effect coefficients and the associated 90% confidence intervals for relative two-year periods from $-3/-4$ to $+3/+4$ around the NCA reform. The two-year period for the years $-1/-2$ is the omitted category and serves as the reference period; year 0 is excluded. Panel A, B, C, and D present the results for takeover likelihood, initial hostility, offer premiums, and deal withdrawal, respectively. State fixed effect is defined based on the firm's state of headquarters, and industry fixed effect is defined based on SIC industry divisions. Standard errors are double-clustered by state and year.

Table 1: NCA Enforcement Score Changes

State	Year	Score Change	State	Year	Score Change
Colorado	2011	2 to 3	Montana	1986	1 to 2
Florida	1990	6 to 7	Montana	2012	2 to 1
Florida	1996	7 to 9	Oregon	2008	6 to 7
Georgia	2011	5 to 6	South Carolina	2010	5 to 4
Idaho	2008	6 to 7	Texas	1989	4 to 5
Illinois	2012	5 to 6	Texas	1994	5 to 3
Illinois	2013	6 to 5	Texas	2006	3 to 4
Kentucky	2006	6 to 8	Texas	2009	4 to 5
Louisiana	1990	1 to 4	Texas	2012	5 to 6
Louisiana	2001	4 to 0	Virginia	1992	4 to 3
Louisiana	2003	0 to 4	Virginia	2013	3 to 4
Massachusetts	1983	5 to 6	Wisconsin	2009	3 to 5
Michigan	1985	0 to 5	Wyoming	1994	3 to 4

This table presents changes in state-level NCA enforcement scores (*Score*) over the period from 1981 to 2013 following the twelve questions proposed by Malsberger (2002). Reforms are collected from Garmaise (2011), Bird and Knopf (2015), Kini, Williams, and Yin (2021), Ysmailov (2022), and Jeffers (2023). If the reform takes place in the last three months of the calendar year, we assign the treatment year as the following year. *Score* ranges from 0 to 12, where 12 means the strictest enforcement state, and 0 means not enforceable; the highest value observed in our sample is 9.

Table 2: Descriptive Statistics: M&A Sample

Variables	Mean	SD	Min	Median	Max	N
Deal characteristics						
Premium	41.958	35.555	0.000	34.400	200.000	2,843
Target BHAR [-21, completion]	33.070	42.891	-57.834	27.800	205.863	2,536
Hostile	0.052	0.221	0.000	0.000	1.000	3,005
Withdrawn	0.131	0.338	0.000	0.000	1.000	3,005
Cash Offer	0.234	0.423	0.000	0.000	1.000	2,857
Tender Offer	0.127	0.334	0.000	0.000	1.000	3,005
Target characteristics						
ROA	0.038	0.187	-0.885	0.043	0.361	2,923
Size ($bil.)	0.875	3.508	0.000	0.124	56.549	2,988
Leverage	0.194	0.193	0.000	0.141	0.836	2,911
Book-to-Market	0.700	0.580	-0.327	0.574	3.502	2,905
Cash Holdings	0.170	0.216	0.000	0.068	0.867	2,923
Dual Class Shares	0.037	0.188	0.000	0.000	1.000	3,005
State level characteristics						
Score	3.838	2.284	0.000	4.000	9.000	3,005
In-state Competition	0.093	0.155	0.000	0.034	0.944	2,924
GDP Growth	0.061	0.029	-0.028	0.058	0.143	2,995
Business Combination	0.830	0.375	0.000	1.000	1.000	2,837
IDD	0.452	0.498	0.000	0.000	1.000	3,005

This table presents summary statistics for the M&A sample. The sample is made up of U.S. target, public-to-public horizontal M&A deals during the period 1981 to 2013 from Thomson Reuters SDC. The sample only includes horizontal deals, where the target and acquirers share the same two-digit SIC Codes. *Premium* is winsorized to be between 0 and 200. All other continuous variables are winsorized at the 1st and 99th percentiles. Variables are defined in Appendix C.

Table 3: Descriptive Statistics: CRSP-Compustat Panel

Variables	Mean	SD	Min	Median	Max	N
Firm characteristics						
Takeover	0.019	0.136	0.000	0.000	1.000	165,927
ROA	0.045	0.226	-1.156	0.091	0.394	162,304
Size ($bil.)	1.562	10.139	0.001	0.100	626.550	165,927
Leverage	0.226	0.212	0.000	0.181	0.944	161,863
Book-to-Market	0.675	0.635	-1.113	0.549	3.617	163,012
Cash Holdings	0.169	0.211	0.000	0.077	0.905	162,510
Dual Class Shares	0.059	0.235	0.000	0.000	1.000	165,927
State level characteristics						
Score	3.888	2.190	0.000	4.000	9.000	165,927
GDP Growth	0.063	0.033	-0.030	0.061	0.152	165,485
In-state Competition	0.092	0.155	-0.024	0.030	1.000	162,202
Business Combination	0.727	0.445	0.000	1.000	1.000	160,358
IDD	0.433	0.495	0.000	0.000	1.000	165,927

This table presents summary statistics for the CRSP-Compustat panel. The sample is made up of CRSP-Compustat public firms listed on NYSE, AMEX, or Nasdaq stock exchanges that have share codes 10 and 11 with fiscal-year end dates between January 1980 and December 2012. Firms with market capitalization below $1 million USD are excluded. All continuous variables are winsorized at the 1st and 99th percentiles. Variables are defined in Appendix C.

Table 4: NCAs and Takeover Activity

	TWFE		Stacked DID	
	(1)	(2)	(3)	(4)
Score	-0.0006***	-0.0009***		
	[-3.99]	[-6.63]		
Treated×Post			-0.0024**	-0.0029***
			[-2.06]	[-2.80]
ROA		-0.0019		0.0010
		[-1.12]		[0.87]
ln(Size)		0.0003		0.0001
		[0.97]		[0.43]
Leverage		-0.0064***		-0.0019
		[-3.12]		[-0.91]
Book-to-Market		0.0017**		0.0026***
		[2.24]		[4.49]
Cash Holdings		-0.0009		-0.0023
		[-0.21]		[-0.60]
Dual Class Shares		-0.0082***		-0.0071***
		[-4.38]		[-4.89]
GDP Growth		0.0285*		0.0239
		[1.86]		[1.10]
In-state Competition		0.0036		0.0042
		[0.70]		[0.66]
Business Combination		-0.0003		-0.0010
		[-0.21]		[-0.55]
IDD		-0.0011		-0.0005
		[-1.06]		[-0.31]
State FE	Yes	Yes		
Year FE	Yes	Yes		
Industry FE	Yes	Yes		
Stack×State FE			Yes	Yes
Stack×Year FE			Yes	Yes
Stack×Industry FE			Yes	Yes
Adjusted R-Squared	0.0098	0.0101	0.0101	0.0105
N	155,591	155,591	454,127	454,127

This table presents the estimates of the effect of NCA law amendments on takeover activity. The sample is made up of CRSP-Compustat firm-year observations from 1981-2013. The dependent variable *Takeover* is a dummy variable that equals one if the firm was targeted by the acquirer from the same industry defined by the 2-digit SIC code within that specific year. *Score* represents the NCA enforcement level. Variables are defined in Appendix C. State fixed effect is defined based on the firm's state of headquarters, and industry fixed effect is defined based on SIC industry divisions. Standard errors are double-clustered by state and year (t-statistics in parentheses). Symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. N denotes the number of observations.

Table 5: NCAs and Acquisitions of Private Firms

	Private all				Private small				High tech			
	TWFE		Stacked DID		TWFE		Stacked DID		TWFE		Stacked DID	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Score	0.0034	0.0120			0.0037	0.0123			-0.0130	0.0173		
	[0.09]	[0.34]			[0.10]	[0.35]			[-0.12]	[0.17]		
Treated×Post			-0.0068	-0.0085			-0.0115	-0.0135			0.0026	0.0214
			[-0.08]	[-0.10]			[-0.14]	[-0.16]			[0.01]	[0.11]
GDP Growth		-0.1755		0.1850		-0.2148		0.1303		-1.1185		0.6690
		[-0.57]		[0.63]		[-0.70]		[0.45]		[-1.17]		[0.86]
Business Combination		0.0370		-0.0230		0.0375		-0.0231		0.1592		0.0179
		[0.75]		[-1.07]		[0.76]		[-1.22]		[0.99]		[0.33]
IDD		0.1035*		0.0347		0.1016*		0.0289		0.3268*		0.2005
		[1.77]		[0.88]		[1.76]		[0.77]		[1.81]		[1.50]
State FE	Yes	Yes			Yes	Yes			Yes	Yes		
Year FE	Yes	Yes			Yes	Yes			Yes	Yes		
Industry FE	Yes	Yes			Yes	Yes						
Stack×State FE			Yes	Yes			Yes	Yes			Yes	Yes
Stack×Year FE			Yes	Yes			Yes	Yes			Yes	Yes
Stack×Industry FE			Yes	Yes			Yes	Yes				
Adjusted R-Squared	0.5850	0.5859	0.6214	0.6214	0.5802	0.5810	0.6149	0.6149	0.7777	0.7837	0.8219	0.8227
N	18,150	18,150	48,664	48,664	18,150	18,150	48,664	48,664	1,650	1,650	4,414	4,414

This table presents the estimates of the effect of NCA law amendments on the aggregate private firm acquisition activity. The sample is made up of U.S. private target, horizontal M&A deals during the period 1981 to 2013 from Thomson Reuters SDC. The dependent variable in all columns is ln(1+aggregate number of deals). Columns (1)-(4) report the results at the state-industry-year level. Columns (5)-(8) perform similar tests, but only for small acquisitions where the transaction value is below US$50 million or not reported. Columns (9)-(12) only includes high-tech industries, and the deal numbers are aggregated at the state-year level. *Score* represents the NCA enforcement level. Variables are defined in Appendix C. State fixed effect is defined based on the firm's state of headquarters, and industry fixed effect is defined based on SIC industry divisions. Standard errors are double-clustered by state and year (t-statistics in parentheses). Symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. N denotes the number of observations.

Table 6: NCAs and Deal Attitude

	TWFE		Stacked DID	
	(1)	(2)	(3)	(4)
Score	0.0535***	0.0469***		
	[6.07]	[4.51]		
Treated×Post			0.1260***	0.1284**
			[2.91]	[2.70]
Cash Offer		0.0510***		0.0596***
		[4.85]		[3.86]
Tender Offer		0.0606**		0.0364
		[2.23]		[1.27]
ROA		0.0229		0.0133
		[1.51]		[0.43]
ln(Size)		0.0111***		0.0162***
		[2.99]		[3.67]
Leverage		-0.0108		-0.0232
		[-0.31]		[-0.42]
Book-to-Market		0.0281***		0.0260*
		[3.26]		[1.95]
Cash Holdings		-0.0463		-0.0674
		[-1.17]		[-0.94]
Dual Class Shares		-0.0184		-0.0129
		[-1.05]		[-0.40]
GDP Growth		-0.1889		-0.2170
		[-0.90]		[-0.66]
In-state Competition		0.0477		0.0908
		[1.45]		[1.43]
Business Combination		-0.0274		-0.0406
		[-1.59]		[-1.60]
IDD		-0.0119		0.0218
		[-0.67]		[0.46]
State FE	Yes	Yes		
Year FE	Yes	Yes		
Industry FE	Yes	Yes		
Stack×State FE			Yes	Yes
Stack×Year FE			Yes	Yes
Stack×Industry FE			Yes	Yes
Adjusted R-Squared	0.0477	0.0751	0.0511	0.0838
N	2,704	2,704	5,883	5,883

This table presents the effect of change in NCA enforcement level on deal attitude. The sample is made up of U.S. target, public-to-public horizontal M&A deals during the period 1981 to 2013 from Thomson Reuters SDC. The dependent variable in all columns is *Hostile*, which is a dummy equal to one if the deal is classified as "Hostile" or "Unsolicited" according to the initial reception (final deal attitude prior to 1985), and zero otherwise. *Score* represents the NCA enforcement level. Variables are defined in Appendix C. State fixed effect is defined based on the firm's state of headquarters, and industry fixed effect is defined based on SIC industry divisions. Standard errors are double-clustered by state and year (t-statistics in parentheses). Symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. N denotes the number of observations.

Table 7: NCAs and Offer Premiums

	TWFE		Stacked DID	
	(1)	(2)	(3)	(4)
Score	4.4987***	4.7253***		
	[3.13]	[3.51]		
Treated×Post			18.0557***	18.9392***
			[3.37]	[3.81]
Cash Offer		-1.1680		-0.2227
		[-0.57]		[-0.09]
Tender Offer		9.7330***		7.6699***
		[4.36]		[3.32]
ROA		-2.2570		-2.7054
		[-0.28]		[-0.30]
ln(Size)		-3.1920***		-3.1198***
		[-7.05]		[-7.02]
Leverage		8.9493**		6.6165
		[2.06]		[1.69]
Book-to-Market		10.0597***		13.7960***
		[6.76]		[5.34]
Cash Holdings		11.0603**		18.6137***
		[2.05]		[3.36]
Dual Class Shares		4.9001		4.7433
		[0.93]		[0.53]
GDP Growth		29.3252		50.0043
		[1.18]		[0.94]
In-state Competition		-15.5128**		-14.8396
		[-2.29]		[-1.51]
Business Combination		-2.5548		-3.3521
		[-1.16]		[-1.49]
IDD		-0.8266		-1.4608
		[-0.39]		[-0.66]
State FE	Yes	Yes		
Year FE	Yes	Yes		
Industry FE	Yes	Yes		
Stack×State FE			Yes	Yes
Stack×Year FE			Yes	Yes
Stack×Industry FE			Yes	Yes
Adjusted R-Squared	0.0552	0.1190	0.0781	0.1531
N	2,445	2,445	5,204	5,204

This table presents the estimates of the effect of NCA law amendments on M&A offer premiums. The sample is made up of U.S. target, public-to-public horizontal M&A deals during the period 1981 to 2013 from Thomson Reuters SDC. The dependent variable in all columns is *Premium*, defined as the offer price from SDC divided by the target stock price 21 trading days prior to the announcement from CRSP, minus one and multiplied by 100. *Score* represents the NCA enforcement level. Variables are defined in Appendix C. State fixed effect is defined based on the firm's state of headquarters, and industry fixed effect is defined based on SIC industry divisions. Standard errors are double-clustered by state and year (t-statistics in parentheses). Symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. N denotes the number of observations.

Table 8: Target & Acquirer BHARs

	Target BHAR [-21, Completion]				Acquirer BHAR [-21, Completion]			
	TWFE		Stacked DID		TWFE		Stacked DID	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Score	5.2554**	4.8124**			0.4434	0.4841		
	[2.68]	[2.16]			[0.30]	[0.32]		
Treated×Post			21.3568***	20.7102***			1.5176	2.4059
			[4.84]	[3.73]			[0.38]	[0.68]
Firm Controls		Yes		Yes		Yes		Yes
Deal Controls		Yes		Yes		Yes		Yes
State Controls		Yes		Yes		Yes		Yes
State FE	Yes	Yes			Yes	Yes		
Year FE	Yes	Yes			Yes	Yes		
Industry FE	Yes	Yes			Yes	Yes		
Stack×State FE			Yes	Yes			Yes	Yes
Stack×Year FE			Yes	Yes			Yes	Yes
Stack×Industry FE			Yes	Yes			Yes	Yes
Adjusted R-Squared	0.0423	0.0710	0.0578	0.0839	0.0265	0.0388	0.0284	0.0667
N	2,221	2,221	4,575	4,575	2,079	2,079	4,279	4,279

This table presents the estimates of the effect of change in NCA enforcement level on the target and acquirer BHARs. The sample contains all U.S. target, public-to-public horizontal M&A deals during the period 1981 to 2013 from Thomson Reuters SDC. Firm controls include *ROA, Size*, *Leverage*, *Book-to-Market, Dual-class Shares*, and *Cash Holdings*. Deal Controls include *Cash offer*, and *Tender offer*. State-level controls include *GDP Growth, In-State Competition, Business Combination Law*, and *IDD*. *Score* represents the NCA enforcement level. Variables are defined in Appendix C. State fixed effect is defined based on the firm's state of headquarters, and industry fixed effect is defined based on SIC industry divisions. Standard errors are double-clustered by state and year (t-statistics in parentheses). Symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. N denotes the number of observations.

Table 9: NCAs and Deal Withdrawal

	TWFE		Stacked DID	
	(1)	(2)	(3)	(4)
Score	0.0536***	0.0586***		
	[3.60]	[4.11]		
Treated×Post			0.1194***	0.1364**
			[3.11]	[2.64]
Cash Offer		0.0522**		0.0451
		[2.68]		[1.67]
Tender Offer		-0.1153***		-0.1299***
		[-5.20]		[-6.34]
ROA		0.0065		-0.0157
		[0.21]		[-0.41]
ln(Size)		-0.0010		-0.0030
		[-0.31]		[-0.58]
Leverage		0.0216		-0.0252
		[0.38]		[-0.25]
Book-to-Market		0.0397**		0.0301
		[2.25]		[1.35]
Cash Holdings		-0.0608		-0.1285*
		[-1.29]		[-1.70]
Dual Class Shares		-0.0355		-0.0485
		[-1.16]		[-1.07]
GDP Growth		-0.1257		-0.5590
		[-0.48]		[-1.37]
In-state Competition		0.0532		0.0490
		[0.97]		[0.50]
Business Combination		0.0184		0.0196
		[0.67]		[0.65]
IDD		0.0138		0.0491
		[0.70]		[0.76]
State FE	Yes	Yes		
Year FE	Yes	Yes		
Industry FE	Yes	Yes		
Stack×State FE			Yes	Yes
Stack×Year FE			Yes	Yes
Stack×Industry FE			Yes	Yes
Adjusted R-Squared	0.0905	0.1044	0.0822	0.0990
N	2,704	2,704	5,883	5,883

This table presents the effect of change in NCA enforcement level on M&A deal withdrawal rate. The sample is made up of U.S. target, public-to-public horizontal M&A deals during the period 1981 to 2013 from Thomson Reuters SDC. The dependent variable in all columns is *Withdrawn*, which is a dummy taking the value of one for withdrawn bid, and zero otherwise. *Score* represents the NCA enforcement level. Variables are defined in Appendix C. State fixed effect is defined based on the firm's state of headquarters, and industry fixed effect is defined based on SIC industry divisions. Standard errors are double-clustered by state and year (t-statistics in parentheses). Symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. N denotes the number of observations.

Table 10: NCAs and Takeover Activity by CEO Age

	TWFE		Stacked DID	
	(1)	(2)	(3)	(4)
CEO Age $\geq$ 60 × Score(Demean)	0.0011***	0.0010**		
	[2.87]	[2.61]		
CEO Age $\geq$ 60 × Treated×Post			0.0048**	0.0049*
			[2.23]	[1.74]
Score(Demean)	-0.0015***	-0.0016***		
	[-3.68]	[-2.89]		
Treated×Post			-0.0031**	-0.0046***
			[-2.08]	[-3.00]
CEO Age $\geq$ 60	-0.0013	-0.0013	-0.0011	-0.0012
	[-1.22]	[-1.22]	[-0.99]	[-1.08]
Firm Controls		Yes		Yes
State Controls		Yes		Yes
State FE	Yes	Yes	No	No
Year FE	Yes	Yes	No	No
Industry FE	Yes	Yes	No	No
Stack×State FE	No	No	Yes	Yes
Stack×Year FE	No	No	Yes	Yes
Stack×Industry FE	No	No	Yes	Yes
Adjusted R-Squared	0.0037	0.0041	0.0043	0.0048
N	91,357	91,357	244,558	244,558

This table presents the estimates of the effect of NCA law amendments on takeover activity. The sample is made up of CRSP-Compustat firm-year observations with non-missing CEO age data from 1981-2013. The dependent variable *Takeover* is a dummy variable that equals one if the firm was targeted by the acquirer from the same industry defined by the 2-digit SIC code within that specific year. The indicator variable *CEO Age* $\geq$ *60* takes the value of one for CEOs aged 60 and above, and zero otherwise. *Score(Demean)* represents the NCA enforcement level demeaned by subtracting the average enforcement score across the whole sample. The main explanatory variable in all columns is the interaction of *Score(Demean)* (or *Treated×Post*) and *CEO Age* $\geq$ *60*. Variables are defined in Appendix C. State fixed effect is defined based on the firm's state of headquarters, and industry fixed effect is defined based on SIC industry divisions. Standard errors are double-clustered by state and year (t-statistics in parentheses). Symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. N denotes the number of observations.

Table 11: Reduced-form "What-if" Analysis

Panel A: Estimated sensitivities and other inputs			
Δ Takeover per Δ NCA Score	-0.001		
Δ Withdrawn per Δ NCA Score	0.059		
Δ Premium per Δ NCA Score	4.725		
Target size ($ mil.)	124		
Panel B: "What-if" analysis	**(1)**	**(2)**	**(3)**
Scenario	Current	Increase	Decrease
Δ NCA Score	0	+1	−1
Takeover	0.019	0.018	0.020
Withdrawn	0.131	0.190	0.072
Premium, %	41.96	46.68	37.23
Total deals	3,005	2,863	3,147
Completed deals	2,611	2,320	2,919
Shareholder gain ($ mil.)	135,863	134,293	134,788
Difference ($ mil.)	N/A	-1,570	-1,075
% change	N/A	**-1.16%**	**-0.79%**

This table presents the results of the "what-if" (counterfactual) analysis of target shareholder wealth using the reduced-form estimates of the effect of NCA enforcement changes on takeover outcomes. Panel A lists the inputs into the analysis, namely the estimated effects of NCA enforcement changes on takeover likelihood, deal completion, and takeover premiums, as well as target size. Panel B provides estimates of the target shareholder gains in three scenarios. Scenario in column (1) is based on the actuals observed in the data. Scenario in column (2) is based on increasing the NCA enforcement *Score* by one point. Scenario in column (3) is based on decreasing the NCA enforcement *Score* by one point. The last two rows compare target shareholder wealth in each counterfactual scenario with the actual.

Appendices

Appendix A: Examples of NCAs

NCA Example 1: CVS Caremark Corporation

Source: `https://www.sec.gov/Archives/edgar/data/64803/000119312511040351/dex1034.htm`

This Employment Agreement is entered into by and between CVS Caremark Corporation, a Delaware corporation (the "Company" or "CVS"), and Per G.H. Lofberg (the "Executive").

Non-Competition. During the Executive's employment with the Company or one of its subsidiaries and during the 24-month period following the termination of Executive's employment for any reason (the "Non-Competition Period"), Executive will not, directly or indirectly, engage in Competition with the Company. "Competition" shall mean engaging in any activity for a Competitor of the Company, whether as a principal, agent, partner, officer, director, employee, independent contractor, investor, consultant or stockholder (except as a less-than one percent shareholder of a publicly traded company) or otherwise. A "Competitor" shall mean any person, corporation or other entity (and its parents, subsidiaries, affiliates and assigns) doing business in any geographical area in which the Company or any of its subsidiaries or affiliates are doing or have imminent plans to do business, and which is engaged in the operation of: (a) a retail business which includes or has imminent plans to include a pharmacy (i.e. the sale of prescription drugs) as an offering or component of its business, including but not limited to, chain drug store companies such as Walgreen Co. and Rite Aid Company, mass merchants such as Wal-Mart Stores, Inc. and Target Corp., and food/drug combinations such as The Kroger Co. and Supervalu Inc.; and/or (b) a business which includes or has imminent plans to include mail order prescription, specialty pharmacy and/or pharmacy benefits management or any other services offered by Caremark Rx, LLC as an offering or component of its business, such as Medco Health Solutions, Inc. or Express Scripts, Inc., and/or (c) a business which includes or has imminent plans to include offering, marketing or the sale of basic acute health care services at retail or other business locations, similar to the services provided by MinuteClinic, LLC (and excluding hospitals, private physicians' offices or other businesses dedicated to the direct provision of health care services). During Executive's employment by the Company or one of its subsidiaries and during the Non-Competition Period, Executive will not, directly or indirectly, engage in any activity that involves providing audit review or other consulting or advisory services with respect to any relationship between the Company and any third party.

NCA Example 2: International CCE, Inc.

Source: https://www.sec.gov/Archives/edgar/data/1491675/000119312510225941/dex101.htm

THIS EMPLOYMENT AGREEMENT ("Agreement"), entered into as of October, 2010, between International CCE, Inc., a Delaware corporation (the "Company"), and John F. Brock (the "Executive"). The Company and the Executive may be referred to herein collectively as the "Parties," or individually as a "Party."

Noncompetition. Provided the Company is not in breach of its obligations to make any of the payments or provide any of the benefits provided in Sections 4 through 11 of this Agreement, during the period beginning with the Executive's termination of employment during the Initial Term for any reason and ending upon the later of the 12-month anniversary of the Executive's termination of employment or following the number of months of severance to which the Executive is entitled under Section 11(b) (if any), but no later than 24 months following the Executive's termination of employment (such period of time shall hereinafter be referred to as the "Restricted Period"), the Executive shall not directly or indirectly, on the Executive's own behalf or on behalf of any person or entity, compete with the Company by performing activities or duties substantially similar to the activities or duties performed by the Executive for the Company during the year preceding the Executive's termination of employment for any business entity that is a Direct Competitor of the Company within the Restricted Area.

A "Direct Competitor" of the Company is any business or operations in direct competition with the Company within the Restricted Area owned or operated by (i) PepsiCo, Inc.; (ii) Dr. Pepper Snapple Group, Inc.; (iii) if PepsiCo, Inc. or Dr. Pepper Snapple Group, Inc. do not have the highest or next highest market share among the producers and distributors of non-alcoholic beverages within the Restricted Area at the time the Executive's employment terminates, then any company that has the highest or next highest market share among the producers and distributors of non-alcoholic beverages within the Restricted Area at the time the Executive's employment with the Company terminates; or (iv) any company that provides bottling operations to the companies listed in subparts (i), (ii), and (iii) within the Restricted Area. The "Restricted Area" is any geographic area within the scope of the Executive's management authority. The Executive expressly acknowledges and agrees that, because of the nature of the services the Executive has provided to the Company, the Executive has provided services throughout the Restricted Area and, therefore, the Restricted Area is reasonably defined to protect the Company's legitimate business interests.

NCA Example 3: The United Air Lines

Source: https://www.sec.gov/Archives/edgar/data/100517/000010051706000046/mcdemp.htm#:~:text=United%20agrees%20not%20to%20willfully,to%20the%20Executive's%20employment%20with

This Employment Agreement (this "Agreement") is made as of this 29th day of September, 2006 (the "Effective Date"), by and between UAL Corporation, a Delaware corporation (together with its successors and assigns, "UAL"), United Air Lines, Inc. (together with its successors and assigns, "UA," UAL and UA sometimes collectively referred to as "United" or the "Company") and Peter D. McDonald (the "Executive").

Non-Competition: Without the consent in writing of the Board, during the Employment Period and, if the Executive's employment is terminated without Cause by United or by the Executive for Good Reason and the Executive receives severance pursuant to either Section 5(d)(B) or Section 5(e)(ii), for a period of two years after termination of the Executive's employment, regardless of whether such termination occurs during or after the Employment Period, (i) the Executive will not become a consultant to, or an officer, employee, agent, advisor, principal, partner, director or substantial stockholder of any airline, air carrier, or any company or other entity affiliated, directly or indirectly, with another airline or air carrier, including holding company thereof, and (ii) the Executive will not, directly or indirectly, for the benefit of any airline or air carrier or any company or other entity affiliated, directly or indirectly, with another airline or air carrier other than United, solicit the employment or services of, hire, or assist in the hiring of any person who is employed as a management employee.

Appendix B: NCA Score Measurement Questions and Thresholds

The following questions are the original questions and thresholds from Malsberger (2002), these questions are used to determine the level of NCA enforcement in a given state. Each question will be assigned 1 point if the answer is above the threshold.

Question 1. Is there a state statute of general application that governs the enforceability of covenants not to compete?

Threshold 1. States that enforce noncompetition agreements outside a sale-of-business context receive a score of 1.

Question 2. What is an employer's protectable interest and how is it defined?

Threshold 2. States in which the employer can prevent the employee from future independent dealings with all the firm's customers, not merely with the customers with whom the employee had direct contact, receive a score of 1.

Question 3. What must the plaintiff be able to show to prove the existence of an enforceable covenant not to compete?

Threshold 3. Laws that place greater weight on the interests of the firm relative to those of the former employee are above the threshold. For example, a law that requires that the contract be reasonably protective of the firm's business interests and only meet the condition of not being unreasonably injurious to the employee's interests would receive a score of 1.

Question 4. Does the signing of a covenant not to compete at the inception of the employment relationship provide sufficient consideration to support the covenant?

Threshold 4. States for which the answer to Question 4 is clearly "Yes" are above the threshold.

Question 5. Will a change in the terms and conditions of employment provide sufficient consideration to support a covenant not to compete entered into after the employment relationship has begun?

Threshold 5. States for which the answer to Question 5 is clearly "Yes" are above the threshold.

Question 6. Will continued employment provide sufficient consideration to support a covenant not to compete entered into after the employment relationship has begun?

Threshold 6. States for which the answer to Question 6 is clearly "Yes" are above the threshold.

Question 7. What factors will the court consider in determining whether time and geographic restrictions in the covenant are reasonable?

Threshold 7. Jurisdictions in which courts are instructed not to consider economic or other hardships faced by the employee are above the threshold.

Question 8. Who has the burden of proving the reasonableness or unreasonableness of the covenant

not to compete?

Threshold 8. States in which the burden of proof is clearly placed on the employee are above the threshold.

Question 9. What type of time or geographic restrictions has the court found to be reasonable? Unreasonable?

Threshold 9. Jurisdictions in which 3-year statewide restrictions have been upheld receive a score of 1.

Question 10. If the restrictions in the covenant not to compete are unenforceable because they are overbroad, are the courts permitted to modify the covenant to make the restrictions more narrow and to make the covenants enforceable?

Threshold 10. States for which the answer to Question 10 is clearly "Yes" are above the threshold.

Question 11. If the employer terminates the employment relationship, is the covenant enforceable?

Threshold 11. States for which the answer to Question 11 is clearly "Yes" are above the threshold.

Question 12. What damages may an employer recover and from whom for breach of a covenant not to compete?

Threshold 12. If, in addition to lost profits, there is a potential for punitive damages against the former employee, the state receives a score of 1. States that explicitly exclude consideration of the reasonableness of the contract from the calculation of damages are also above the threshold.

Appendix C: Variable Definitions

Variable	Definition
Main dependent variables	
Takeover	Dummy equal to one if a firm in the CRSP-Compustat panel is targeted in a given year by a bidder from same 2-digit SIC industry, and zero otherwise.
Withdrawn	Dummy equal to one if the deal is withdrawn according to SDC, and zero if complete.
Hostile	Dummy equal to one if SDC data field *Initial Reception* is stated as "Hostile" or "Unsolicited", and zero otherwise. Since SDC coverage of *Initial Reception* is sparse prior to 1985, for the first four years of the sample we use SDC data field *Deal attitude* instead (it represents final rather than initial attitude).
Premium	Offer price from SDC divided by the target firm stock price 21 business days prior to the acquisition announcement from CRSP, minus one and multiplied by 100. Target stock price 21 business days prior is adjusted for any stock splits between that date and the announcement date.
Target BHAR [-21, completion]	Target firm buy-and-hold abnormal return for the period from 21 days before the announcement date to the completion date. Value-weighted CRSP index is the benchmark return.
Acquirer BHAR [-21, completion]	Acquiring firm buy-and-hold abnormal return for the period from 21 days before the announcement date to the completion date. Value-weighted CRSP index is the benchmark return.
Synergy	Weighted average of bidder and target buy-and-hold abnormal returns. The weights are the market values of the bidder and the target 22 days prior to the announcement from CRSP, respectively.
Key explanatory variables	
Score	NCA enforcement score in the firm's headquarters state. NCA enforcement score is collected from Bird and Knopf (2015), Ysmailov (2022) for 1981 to 1991, Garmaise (2011) for 1992 to 2004, and Kini, Williams, and Yin (2021) for 2005 to 2013. In the CRSP-Compustat panel we use historical state of the firm's headquarters obtained from Gao, Leung, and Qiu (2021). In the M&A sample, we use the state of the target's primary business location provided by SDC.
Treated × Post	Interaction of treatment status (NCA enforcement score change) and treatment timing. For treated units, take the value of 1 after an increase of the state's NCA enforcement score, the value of -1 after a decrease of the state's NCA enforcement score, and 0 prior to the change. For untreated units always take the value of zero.

Variable	**Definition**
Control variables	
Book-to-Market	Book value of equity (Compustat item *ceq*) divided by the market value of common equity (Compustat item *csho* multiplied by Compustat item *prcc_f*) as of the most recent fiscal year end.
Cash Offer	Dummy equal to one if the deal is paid by all cash, and zero otherwise. For deals in which the entire consideration structure is known (i.e. SDC data field *% of Unknown* equals zero), cash deal indicator takes the value of one if *% of Cash* equals 100, and zero otherwise. For deals with non-zero *% of Unknown*, we use the information contained in the SDC data field *Consideration Offered*. Specifically, if consideration offered includes only the terms "cash", "liabilities", "warrants", "earnout", and "contingent value right", we classify the deal as a cash deal, and zero otherwise. (NB: We have verified that "warrants" in the consideration offered field refers to cash paid for the target's warrants, rather than the acquirer offering its own warrants as payment. "Liabilities" is shorthand for assumed liabilities.)
Cash Holdings	Cash and short-term investments (Compustat item *che*) divided by total assets (Compustat item *at*) as of the most recent fiscal year end.
Tender Offer	Dummy equal to one if SDC tender offer flag states "True", and zero otherwise.
Dual Class Shares	Dummy equal to one for firms classified as dual-class, and zero otherwise. The firm is considered dual-class if at least one of the following is true: i) RiskMetrics (formerly IRRC) classifies the firm as dual-class, ii) the firm is classified as dual-class in Jay Ritter's IPO database, iii) if the firm's Compustat *gvkey* is associated with multiple CRSP *permno*s, iv) number of shares outstanding as of the fiscal year-end date from CRSP (item *shrout*) and Compustat (item *csho*) differ by more than 20%.
Business Combination	Dummy equal to one if the firm's state of incorporation has implemented a Business Combination law, and zero otherwise. Business Combination law data is collected from Karpoff and Wittry (2018). We use historical state of incorporation data from Spamann and Wilkinson (2019) (we backfill values prior to 1994 using the earliest value from the Spamann-Wilkinson dataset, and we use Compustat item *incorp* for firms not covered in that dataset).
GDP Growth	GDP growth rate in the firm's state of headquarters. GDP growth data is from the U.S. Bureau of Economic Analysis. In the CRSP-Compustat panel, we use historical state of headquarters from Gao, Leung, and Qiu (2021). In the M&A sample, target firm state of headquarters is from SDC.

Variable	**Definition**
IDD	Dummy equal to one if firm's state of headquarters recognizes the Inevitable Disclosure Doctrine, and zero otherwise. Data on Inevitable Disclosure laws is from Klasa, Ortiz-Molina, Serfling, and Srinivasan (2018). In the CRSP-Compustat panel, we use historical state of headquarters from Gao, Leung, and Qiu (2021). In the M&A sample, target firm state of headquarters is from SDC.
In-state Competition	The fraction of total 2-digit SIC industry sales (Compustat item *sale*), excluding those of the firm itself, generated by firms in the same state.
Leverage	Total financial debt (Compustat item *dltt* + Compustat item *dlc*) divided by total assets (Compustat item *at*) as of the most recent fiscal year end.
ROA	Operating income before depreciation (Compustat item *oibdp*) divided by total assets (Compustat item *at*) as of the most recent fiscal year end. In the few cases where Compustat item *oibdp* is missing, we use the difference between Compustat items *sale* and *xopr*.
Size	Firm's market capitalization as of the most recent fiscal year-end. Market capitalization is computed as closing price of common stock (Compustat item *prcc_f*) times the number of common shares outstanding (Compustat item *csho*), in US$ million.

Appendix D: Additional Tables

Table A1: Placebo Tests: Unrelated Deals

	TWFE		Stacked DID	
Panel A: Takeover				
Score	0.0004	0.0004		
	[0.94]	[0.69]		
Treated×Post			-0.0012*	-0.0011
			[-1.75]	[-0.92]
Panel B: Hostile				
Score	-0.0435**	-0.0428**		
	[-2.29]	[-2.21]		
Treated×Post			-0.0103	0.0091
			[-0.22]	[0.14]
Panel C: Offer Premium				
Score	1.3281	0.9628		
	[0.42]	[0.28]		
Treated×Post			0.2338	-3.4100
			[0.05]	[-0.33]
Panel D: Withdrawn				
Score	0.0027	-0.0023		
	[0.14]	[-0.09]		
Treated×Post			0.1345**	0.1292**
			[2.35]	[2.22]
Firm Controls		Yes		Yes
Deal Controls (if applicable)		Yes		Yes
State Controls		Yes		Yes
State FE	Yes	Yes		
Year FE	Yes	Yes		
Industry FE	Yes	Yes		
Stack×State FE			Yes	Yes
Stack×Year FE			Yes	Yes
Stack×Industry FE			Yes	Yes

This table presents the estimates of the effect of change in NCA enforcement level on takeover likelihood, deal attitude, offer premium, and withdrawal likelihood in panels A, B, C, and D, respectively. In panel A the sample is the CRSP-Compustat panel of U.S. listed firms. In panels B, C, and D the sample is made up of U.S. target, public-to-public non-horizontal M&A deals from 1981 to 2013 from Thomson Reuters SDC. Firm controls include *ROA, Size*, *Leverage*, *Book-to-Market, Dual-Class Shares*, and *Cash Holdings*. Deal Controls (not applicable in Panel A) include *Cash offer* and *Tender offer*. State-level controls include *GDP Growth, In-State Competition, Business Combination Law*, and *IDD*. *Score* represents the NCA enforcement level. Variables are defined in Appendix C. State fixed effect is defined based on the firm's state of headquarters, and industry fixed effect is defined based on SIC industry divisions. Standard errors are double-clustered by state and year (t-statistics in parentheses). Symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A2: NCAs and Firm Fundamentals

	TWFE		Stacked DID	
	(1)	(2)	(3)	(4)
Panel A: Book-to-Market				
Score	-0.0029	-0.0053		
	[-0.21]	[-0.45]		
Treated×Post			-0.0263	-0.0227
			[-1.18]	[-1.21]
Panel B: ROA				
Score	0.0015	-0.0008		
	[0.42]	[-0.18]		
Treated×Post			0.0062*	0.0040
			[1.80]	[0.97]
Panel C: *ln*(Size)				
Score	0.0435**	0.0284		
	[2.27]	[1.01]		
Treated×Post			-0.0239	-0.0139
			[-0.42]	[-0.21]
Panel D: Leverage				
Score	0.0002	0.0022		
	[0.06]	[0.82]		
Treated×Post			-0.0064	-0.0045
			[-0.73]	[-0.54]
Panel E: Cash holdings				
Score	-0.0064	-0.0063		
	[-1.41]	[-1.09]		
Treated×Post			-0.0036	-0.0029
			[-0.53]	[-0.41]
State Controls		Yes		Yes
State FE	Yes	Yes		
Year FE	Yes	Yes		
Industry FE	Yes	Yes		
Stack×State FE			Yes	Yes
Stack×Year FE			Yes	Yes
Stack×Industry FE			Yes	Yes

This table presents the effect of change in NCA enforcement level on M&A deal withdrawal rate. The sample is made up of U.S. target, public-to-public horizontal M&A deals during the period 1981 to 2013 from Thomson Reuters SDC. The dependent variable in all columns is *Withdrawn*, which is a dummy taking the value of one for withdrawn bid, and zero otherwise. *Score* represents the NCA enforcement level. Variables are defined in Appendix C. State fixed effect is defined based on the firm's state of headquarters, and industry fixed effect is defined based on SIC industry divisions. Standard errors are double-clustered by state and year (t-statistics in parentheses). Symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. N denotes the number of observations.

Table A3: NCAs and Anti-Takeover Provisions

	TWFE		Stacked DID	
	(1)	(2)	(3)	(4)
Panel A: E-Index				
Score	-0.0234	-0.0412		
	[-0.67]	[-1.21]		
Treated×Post			-0.0132	0.0275
			[-0.25]	[0.48]
N	25,349	25,349	69,063	69,063
Panel B: Golden Parachute				
Score	-0.0136	-0.0190**		
	[-1.67]	[-2.45]		
Treated×Post			-0.0161	-0.0009
			[-0.58]	[-0.03]
N	27,389	27,389	74,955	74,955
Panel C: Poison Pill				
Score	-0.0014	-0.0045		
	[-0.08]	[-0.26]		
Treated×Post			-0.0388*	-0.0340
			[-1.92]	[-1.32]
N	27,389	27,389	74,955	74,955
Panel D: Supermajority Provision				
Score	-0.0023	-0.0032		
	[-0.21]	[-0.27]		
Treated×Post			-0.0016	0.0037
			[-0.09]	[0.17]
N	25,349	25,349	69,063	69,063
Panel E: Limits to Amend Bylaws				
Score	-0.0173	-0.0237		
	[-1.03]	[-1.28]		
Treated×Post			-0.0018	0.0057
			[-0.07]	[0.23]
N	27,389	27,389	74,955	74,955
Panel F: Limits to Amend Charter				
Score	0.0019	0.0013		
	[0.52]	[0.28]		
Treated×Post			0.0113	0.0166*
			[1.59]	[1.96]
N	27,389	27,389	74,955	74,955
Panel G: Staggered (Classified) Board				
Score	0.0050	0.0025		
	[0.46]	[0.18]		
Treated×Post			0.0050	0.0083
			[0.23]	[0.37]
N	27,389	27,389	74,955	74,955

	TWFE		Stacked DID	
	(1)	(2)	(3)	(4)
Panel H: Extended Staggered Board				
Score	-0.0022	-0.0051		
	[-0.33]	[-0.62]		
Treated×Post			0.0041	0.0117
			[0.34]	[0.77]
N	101,569	101,569	257,713	257,713
State Controls		Yes		Yes
State FE	Yes	Yes		
Year FE	Yes	Yes		
Industry FE	Yes	Yes		
Stack×State FE			Yes	Yes
Stack×Year FE			Yes	Yes
Stack×Industry FE			Yes	Yes

This table presents the estimates of the effect of change in NCA enforcement level on firm anti-takeover provisions such as the E-index, golden parachute, poison pill, supermajority provision, limits to amend bylaws, limits to amend charter, staggered(classified) board, and extended staggered board in panels A, B, C, D, E, F, G, and H, respectively. The sample is the CRSP-Compustat panel of U.S. listed firms from 1981 to 2013. State-level controls include *GDP Growth, In-State Competition, Business Combination Law*, and *IDD*. *Score* represents the NCA enforcement level. Variables are defined in Appendix C. State fixed effect is defined based on the firm's state of headquarters, and industry fixed effect is defined based on SIC industry divisions. Standard errors are double-clustered by state and year (t-statistics in parentheses). Symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.