Outside Employment Opportunities and Tournament Incentives

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Abstract

We find that firms enlarge the executive pay gap when executive mobility is constrained by more enforceable non-compete agreements. We interpret this finding as evidence that firms increase tournament incentives to keep executives incentivized after the loss of valuable outside employment options. Consistent with this argument, we observe more significant increases in pay gaps for executives with greater ex ante mobility options. However, shocks reducing enforceability have a weaker, less robust impact on pay gaps, contributing to asymmetric effects. Following restrictions to mobility, equity portfolios that long (short) firms that boost (do not boost) executive pay gaps generate positive alphas.

Keywords: Non-compete agreements; Human capital; Executive pay gap; Tournament incentives

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I. Introduction

Establishing effective compensation schemes is essential for incentivizing and retaining human capital. The compensation gap across the ordinal rank of workers in an organization is a form of rank-order tournament incentive in which an individual's performance is relatively evaluated (e.g., Lazear and Rosen (1981)). The reward for the best relative performer is theoretically the leadership of the organization, a position associated with increased pecuniary wealth and social reputation. Thus, a pay gap, like a tournament prize, motivates employees. Promotion-driven pay increases are particularly significant among top executives (Belzil and Bognanno (2008)), especially those who get the coveted chief executive officer (CEO) position. Accordingly, tournament incentives should greatly affect the behavior of a firm's vice presidents (VPs), inducing them to exert effort and boost their willingness to take risks (Hvide (2002), Lazear and Rosen (1981)), thereby shaping various corporate outcomes (e.g., Burns, Minnick, and Starks (2017), Kale, Reis, and Venkateswaran (2009), Kini and Williams (2012)).

While several past findings pertain to the effects of rank-order tournament incentives, there is limited evidence on their determinants (e.g., Bognanno (2001), Burns et al. (2017), Henderson and Fredrickson (2001), Kale et al. (2009), Main, O'Reilly, and Wade (1993)). Furthermore, related studies largely consider tournament incentives in isolation from the external managerial labor market, overlooking the role of executive mobility—an issue that has garnered significant attention from both academics and practitioners. We provide a novel contribution to this literature by studying whether exogenous variations in VPs' outside job opportunities affect the magnitude of the internal tournament incentives that VPs encounter in their current

workplace. In other words, we address whether firms adjust their internal tournament incentives for VPs following changes to the VPs' external job opportunities.

Internal promotion (vertical move) is an important incentive device for VPs, but so are outside employment opportunities. VPs can strive for the CEO position in another firm (diagonal move) or join another firm with a similar job title but better pay and status (horizontal move). Horizontal and diagonal moves are particularly common. Graham, Kim, and Kim (2020) document that chief financial officer (CFO) mobility is higher and has increased faster than CEO mobility since the last century, and Kale, Reis, and Venkateswaran (2014) report that 40% of the VPs in S&P 500 firms assume the CEO position after moving across firms.

We use the term "external tournament incentives" to describe incentives associated with a VP's opportunity to leave for an outside job, which would motivate the VP to perform better. Prior studies highlight that executive tournaments can occur *within* similar product markets or local labor markets (e.g., Coles, Li, and Wang (2018), Ma, Pan, and Stubben (2020)). However, the concept of an external tournament, as defined here, is broader, as it relates to all prospective job opportunities in both horizontal and diagonal career moves, including those *across* different markets. Thus, it is hard to develop a proxy for external tournament incentives. However, we argue that the most obvious factor associated with such incentives is a VP's potential job mobility. If a VP's ability to join another firm is low, the incentives arising from the external executive market are limited.

Similar to Ewens and Marx (2018) and Kini, Williams, and Yin (2021), we use the variations in the state-level enforceability of employee non-compete agreements (NCAs) as exogenous changes in VPs' external tournament incentives. NCAs are legal clauses that can be embedded in employment contracts to restrict employees' ability to join competing firms. Studies

suggest that more than 70% of top executives are constrained by NCAs (Garmaise (2011), Kini et al. (2021)), but the actual proportion is likely higher, owing to underreporting by firms (Lin, Peters, and Seo (2022)). Since executive labor markets are segmented (e.g., Ma et al. (2020), Yonker (2017)), state-level rules increasing (reducing) the enforceability of NCAs should negatively (positively) affect the mobility of a firm's VPs in the labor market (Garmaise (2011)). These rules are unlikely to be influenced by a firm's actions and can therefore be deemed exogenous to the firm's executive pay system. We identify nine (six) states that have been through increases (decreases) in NCA enforceability from 1993 to 2018. Exploiting the changes in enforceability in a difference-in-differences (DID) framework, we examine how labor market frictions shape internal tournaments and, more specifically, the executive pay gap (that is, the pay gap between the CEO and other executive officers).

We argue that an increase in NCA enforceability might lead to a larger executive pay gap, for two reasons. First, the implicit incentives that executives derive from outside opportunities become less effective when NCAs are more enforceable, which makes internal promotions critical. Thus, firms may widen the pay gap between the CEO and VPs to enhance the utility associated with future promotions and make up for the VPs' lost external opportunities (Gibbons and Murphy (1992)). Second, firms might struggle to locate capable candidates externally when executive mobility is curtailed; therefore, selecting and promoting incumbent executives becomes crucial. A more competitive internal tournament would generate signals about the quality of executives which are useful in the promotion process.

However, several mechanisms cast doubt on the link between strengthened NCA enforcement and increases in the executive pay gap. First, firms could react to the weakening of the external tournament for VPs by embedding performance-vesting (PV) provisions in their stock

and option awards. Such provisions could enhance the relative performance evaluation (RPE) of VPs without affecting the executive pay gap. Second, a reduction in the number of outside employment opportunities could motivate VPs to perform better even if their internal tournament incentives are not adjusted, since potential dismissal could result in a costly and delayed reentry to the executive labor market. Third, prior studies find that stricter NCA enforcement raises executives' dismissal risks (Kini et al. (2021), Lin et al. (2022)), which could further motivate VPs to perform. Overall, it is an empirical question whether firms increase their internal tournament incentives when there is a reduction in executives' outside employment opportunities.

We do not predict a monotonic relation; that is, a decrease in NCA enforceability might not have a negative impact on the executive pay gap. While this is ultimately an empirical question, our ex ante prediction is that the specific negative shocks to NCA enforceability in our study should result in a weak or even insignificant effect on the executive pay gap. As we explain in Section II, this expectation is informed by several idiosyncratic features of these events, as well as by prior evidence on how such events impact different outcomes (Lakkis (2023), Mueller (2023), Tang, Wang, and Zhou (2021)).

As mentioned previously, we employ a DID framework in our analyses. We follow Baker, Larcker, and Wang (2022) and adopt a stacked regression approach to mitigate the estimation bias of a standard staggered DID, where already-treated units may later serve as controls (Baker et al. (2022), Goodman-Bacon (2021)). For each NCA enforceability event at time t, we include treated and control observations over the treatment window $(t - 10, t + 10)$, with control observations belonging to states that are never treated by an NCA shock.

In the main DID tests, we find a highly statistically significant rise in the executive pay gap following a state-level increase in NCA enforceability; following a state-level decrease in

NCA enforceability, we find a reduction in the pay gap that is, at best, significant only at the 10% level (when including all the control variables). In these tests, we define the executive pay gap as the difference between a CEO's pay and the average pay of the VPs from the CEO's firm. On average, a firm affected by a positive NCA shock (a treated firm) has a pay gap that is approximately $0.84 million larger than that of a non-treated firm. In the same model, we estimate that the interquartile change of the distribution of firm size—the most economically significant variable in determining executive compensation (Gabaix and Landier (2008), Gabaix, Landier, and Sauvagnat (2014))—enlarges the pay gap by about $3.44 million. Therefore, the impact of stronger NCA enforcement is economically meaningful, as it is almost one-fourth as large as the impact of firm size. We obtain robust findings for increases in NCA enforceability when using alternative treatment windows, not-yet-treated observations as controls, a standard staggered DID method, and the Callaway and Sant'Anna (2021) estimator. However, the negative effect of a decrease in NCA enforceability on the executive pay gap is mostly insignificant across the above specifications.

In a dynamic setting, we find that the effects of an increase in NCA enforceability on the pay gap emerge in the year of the event and persist over time. In contrast, the parallel trends assumption does not hold in the case of decreases in NCA enforceability. Significant pre-shock trends are observed for firms affected by NCA enforceability reductions, calling into question the already weak results for these events. Additionally, while we report significant increases for CEO and VP pay as well as VP mobility when NCAs become more enforceable, we find that, in general, negative shocks to NCA enforceability do not cause significant changes in executive pay levels or VP mobility.

In cross-sectional tests, we evaluate whether executive mobility is the mechanism behind

our findings by focusing on VPs that are more likely to value mobility. We find that the effect of an increase in NCA enforceability is more significant in firms in states where CEOs are frequently hired from other firms, firms in states with higher skewness of CEO pay levels, and firms led by founder CEOs. In these firm subsamples, VPs are likely to value outside employment options more and to have a high ex ante tendency toward mobility because their within-firm promotion expectations are limited, or their across-firm movements are more rewarding. Further, the impact of enhanced enforceability is more pronounced in firms whose VPs have more significant outside job opportunities (i.e., firms with younger VPs or more able executives, or in industries with lower product market concentration). None of these same cross-sectional analyses generate significant findings when the shocks to NCA enforceability are negative.

Following an increase in NCA enforceability, firms may respond to their VPs' weakened external tournament incentives by attempting to incentivize the VPs through additional PV provisions in stock and option awards (Bettis, Bizjak, Coles, and Kalpathy (2010)), so as to stimulate competition with executives across firms through RPE (Bettis, Bizjak, Coles, and Kalpathy (2018), De Angelis and Grinstein (2020), Wruck and Wu (2022)). Thus, firms resorting to PV provisions might not need to change the executive pay gap as much. Our empirical evidence does not support this argument. First, we find that the impact of an NCA enforceability shock (whether positive or negative) on the executive pay gap is not different in firms that tend to use PV provisions. Second, we do not observe variations in firms' reliance on RPE plans following an NCA enforceability event.

A retention mechanism could also be at play here, as more enforceable NCAs hinder executives' mobility and make it less costly for firms to retain valuable executives. This could reduce the need for schemes that incentivize and reward VPs for performance, including

promotion incentives through executive pay gaps. In contrast, shocks that reduce NCA enforceability raise executive retention costs, which could force firms to enhance both executive compensation levels and pay gaps to retain VPs. We consider several proxies to identify firms for which the retention mechanism should be more relevant (i.e., firms whose VPs are ex ante more valuable and more mobile). These proxies measure whether a firm has VPs that are young or capable; operates in a state where CEOs tend to be hired externally or that has highly skewed CEO pay; belongs to an industry with low concentration or high levels of litigation; has a high market-to-book ratio; and depends on highly skilled labor. We find that these variables do not affect the impact of a negative NCA enforceability shock on the executive pay gap. Furthermore, most of these variables actually magnify rather than mitigate the effect of a positive NCA enforceability shock on the pay gap. On the whole, these findings are inconsistent with a retention-cost explanation. Finally, we rule out that our main findings are driven by plausible alternative explanations based on CEO power (Bebchuk, Cremers, and Peyer (2011)), talent differentials (Mueller, Ouimet, and Simintzi (2017), Terviö (2008)), or CEO bargaining power (Kini et al. (2021)). Taken together, our results suggest that the executive pay gap changes we observe following shifts in NCA enforceability most likely capture variations in tournament incentives.

Another important question is whether the adjustments to executive pay gaps driven by shocks to NCA enforceability have implications for firm value. Prior studies highlight the adverse effects of more enforceable NCAs on firm efficiency owing to limitations to the reallocation of talent (Anand, Hasan, Sharma, and Wang (2018), Bai, Eldemire, and Serfling (2024), He and Wintoki (2023), Shi (2023)). Arguably, firms can counteract these adverse effects by adjusting internal tournament incentives. We provide stock-market-based evidence showing that a strategy

that goes long (short) in a portfolio comprising treated companies with (without) increased pay gaps can generate positive alphas. Put differently, firms that increase their internal tournament incentives for VPs when the external ones weaken perform significantly better than other treated firms.

To the best of our knowledge, our study is the first to provide direct evidence of a substitution effect between external and internal tournament incentives for top executives. In a related study, Coles et al. (2018) argue that CEO departures through external tournaments can be the precondition for an effective internal rank-order tournament; otherwise, the VPs' opportunities on the internal promotion ladder would be negligible. This argument is incomplete, given that CEOs can be dismissed and, importantly, VPs also have external career opportunities. In this study, we argue that the interplay between VPs' internal promotion and external employment opportunities is key to our understanding of the drivers of the internal tournament. More broadly, we contribute to the literature investigating the drivers of rank-order tournament incentives, especially the executive pay gap (e.g., Bognanno (2001), Burns et al. (2017), Kale et al. (2009), Main et al. (1993)).

Moreover, Coles and Li (2023) call for future research to explore variations in executive contract design that cannot be fully explained by managerial and firm attributes. Our findings respond to this call by identifying external tournament incentives as a distinct determinant of executive pay arrangements. Similarly, we contribute to the related topic of the effects of labor mobility on managerial compensation (e.g., Chen, Jung, Peng, and Zhang (2022), Kini et al. (2021)). Kini et al. (2021) argue that CEOs receive higher compensation because NCAs raise their unemployment risks. In this paper, however, we identify an alternative explanation for this increase in CEO compensation—one based on VPs' tournament incentives.

Lastly, a recent study by Johnson, Lavetti, and Lipsitz (2024) finds that stronger NCA enforceability exacerbates gender and racial salary gaps. Instead of gender and racial gaps, we study the executive pay gap and show that firms intentionally expand the gap to generate internal tournament incentives as a substitute for the loss of external ones. Importantly, we also study the implications of this practice for firm value, complementing other studies that provide inconsistent results on the subject (Anand et al. (2018), Bai et al. (2024), Garmaise (2011), He and Wintoki (2023)). Finally, our findings help reconcile conflicting prior evidence on the effects of executive mobility and provide insights for the ongoing debate about pay inequality.

II. Institutional Background

Non-compete clauses embedded in a firm's employment contracts restrict signers from joining competitors if their doing so could infringe upon the firm's legitimate business in the future. Prior work shows that these restrictions protect the firm against losing its key human capital for around two years (Malsberger (1996), Vanko (2002)). The scope of geographic constraints specified in NCAs varies, but NCAs typically cover all the places where the employer has operations (Bishara, Martin, and Thomas (2015), Kini et al. (2021)).

By convention, U.S. courts do not scrutinize the reasonableness or adequacy of contract terms under contract law, and employers can freely include non-compete clauses in employment contracts (Vanko (2002)). At the same time, employers have a strong incentive to sue employees who breach the agreements. Thus, the costs of violating NCA terms are potentially significant. In addition to individuals suspected of violating NCAs (who are vulnerable to lawsuits from their

former employers), new employers may be liable for possible monetary damages. At worst, the new employment relation could be ceased forcefully (Bai et al. (2024), Vanko (2002)).

Therefore, non-compete limitations for top executives can be far-reaching, and their reach is not necessarily confined to the same industry or state as the executive's firm (Bishara et al. (2015)). Nonetheless, the real extent of non-compete restrictiveness is determined by state-level enforceability, which is dictated by legislative actions or prior court rulings. Non-compete laws vary considerably across different states. Although individuals can sometimes escape local regulations through cross-state movements (Marx, Singh, and Fleming (2015)), such moves may not be optimal for executives' career development, as many studies find that executive labor markets are geographically segmented (e.g., Ma et al. (2020), Yonker (2017)). Thus, executives are likely to take into account the effect of state-level restrictions. In turn, these rules can also affect firms' decisions, as firms, critically, must grasp their legal surroundings and adjust production factors on the premise of state law compliance (Anand et al. (2018)).

Malsberger (1996), Garmaise (2011) and several subsequent studies (e.g., Bai et al. (2024), He and Wintoki (2023), Kini et al. (2021), Lin et al. (2022)) consider answers to 12 questions to evaluate the level of NCA enforceability in a particular state and track substantial exogenous changes in enforceability.[1] We follow the same empirical strategy and find that, during our sample period, the following nine states have experienced increases in NCA enforceability:

[1]Bai et al. (2024) and Johnson et al. (2024) present formal evidence that an array of state-level economic and political characteristics and other laws for labor protection and trade secrets are not correlated with NCA enforcement. Jeffers (2024) also reports that the distributions of macro conditions are similar across states with or without changes in enforceability. These findings further support the argument that changes in NCA enforceability are plausibly exogenous.

Florida in 1996, Georgia in 2004 and 2010, Ohio in 2004, Vermont in 2005, Idaho in 2008, Wisconsin in 2009, Texas in 2010, Colorado in 2011, and Virginia in 2013. We record decreases in NCA enforceability in six states: Texas in 1994, Louisiana in 2001, Oregon in 2008, New Hampshire in 2012, Illinois in 2013, and Kentucky in 2014. The events analyzed here are comparable to those in studies with similar sample periods, such as Bai et al. (2024), Ewens and Marx (2018), and Kini et al. (2021). A few discrepancies are discussed in detail and motivated in the Online Appendix, which contains a summary of the events.

Though some studies propose using a discrete enforceability index based on Garmaise's (2011) 12 questions as a test variable (e.g., by counting the number of positive answers), we prefer a binary test variable, which captures changes in the index in a difference-in-differences (DID) specification. Three reasons justify our choice. First, it is difficult to assess whether each question is of equal importance and has an equivalent impact on executive mobility. By creating a dummy indicator, we only need to detect if there are events causing qualitative differences in enforceability. Second, our approach allows us to classify a firm into either the treated or control group according to its home state and cleanly estimate the effect of the treatment. Last, having separate dummies for upward and downward changes in enforceability allows us to examine the heterogeneous effects of the two types of events. In robustness checks, we find that our results are in line with our baseline findings when using the discrete enforceability index. If a state has been through more than one change in the same direction during our sample period, we only focus on the first transition. If there are multiple changes in opposite directions, we exclude the observations after the second event.

We expect the upward shifts in NCA enforceability to more strongly affect the executive pay gap than the downward shifts, for several reasons. First, most prior findings support this

asymmetry. Specifically, we are aware of at least six previous studies that have analyzed the effects of increases and decreases in NCA enforceability on a variety of outcomes (but not on the executive pay gap), and most find asymmetric effects. Three studies report insignificant findings for decreases in enforceability (Lakkis (2023), Mueller (2023), Tang et al. (2021)), and one simply shows that the effects are more significant for increases than for decreases (Lin et al. (2022)). Still, we note that Chen, Balasubramanian, and Forman (2024) do not find any evidence of asymmetric effects, and He and Wintoki (2023) report significant effects only for downward shifts in enforceability.

Second, in Table OA.2 of the Online Appendix, we study market reactions to enforceability shocks and report significant abnormal returns during the window from day -1 to day +3 around events that increase NCA enforceability. The reactions to such events are positive (e.g., Market Model CAR [-1;3] is 1.465%, $p < 0.01$), consistent with the view that NCAs favor employers. In contrast, no abnormal returns are reported around events that weaken NCA enforceability, which could indicate that market participants do not expect firms to be significantly affected.

Third, we find that when NCAs become more enforceable, there is a significant reduction in VP mobility (-0.049, $p < 0.05$). When NCAs become less enforceable, there is no significant effect (see Table OA.3 of the Online Appendix).

Fourth, our sample of negative shocks to NCA enforceability is smaller than our sample of positive shocks. Thus, when we study negative shocks, a single event's idiosyncratic characteristics may heavily affect our estimations by adding noise. It is also questionable whether, for three of our six decreases in enforceability, we should expect any significant findings. The decrease for Louisiana in 2001 was quickly reversed in 2003, making this event rather

problematic. Adjustments to executive compensation take time, and the reversal event may have stopped such adjustments in their tracks. Two other decreases—Oregon's in 2008 and New Hampshire's in 2012—resulted from legislative decisions rather than court cases. Ewens and Marx (2018, Section 5.1) highlight that laws, unlike court decisions, are generally forward-looking and apply to future contracts without invalidating existing ones (including NCAs). It is unlikely that firms will update current contracts to incorporate new NCA clauses, given that events that weaken NCA enforceability favor employees.

Finally, when NCA enforcement is weakened, firms' reactions may be muted, since it is hard to adjust the executive pay gap downward. Given that a CEO's pay is significantly larger than that of the average VP and considering the rarity of pay cuts, a reduction in the pay gap would result from substantially larger relative pay raises for VPs than for the CEO. Unequal growth rates in pay that favor VPs are feasible, but they could demotivate the CEO and affect the CEO's performance. Indeed, recent findings relate CEO pay arrangements to issues beyond monetary incentives, such as the perception of fairness (Edmans, Gosling, and Jenter (2023)).

III. Methodology

A. Empirical Methodology

Our identification relies on the variations in NCA enforceability across states and over time. However, staggered DID applications have been questioned due to the potential use of already-treated observations as control firm-years (Baker et al. (2022), Goodman-Bacon (2021)). Also, a comparison between an earlier-treated and a later-treated group potentially suffers from

estimation bias when treatment effects are heterogeneous. This issue is highly relevant in our setting since the timing of the treatment effect of enforceability might vary across a long sample period. To alleviate potential concerns, we follow Baker et al. (2022) and use stacked specifications as our main methodological approach. Specifically, we set a test window from $t-10$ to $t+10$ for each specific event in year t. In the Online Appendix, we show that our results persist when we restrict our analyses to shorter test windows. In each cohort, we use "never treated" firms, that is, those located in states that are never affected by an NCA enforceability shock (regardless of direction), as control firms. Last, we stack the events together and estimate the average effects for all the events in the following specification:

$$(1) \quad Ln\,(Pay\,Gap)_{c,i,t+1} = \alpha + \beta \times NCA\,Enforceability\,Up/Down_{c,t,s} + \gamma \times X_{c,i,t} + \tau \times Z_{c,i,t+1} + \mu_{c,i} + \rho_{c,t} + \theta_{c,s} + \varepsilon_{c,i,t+1},$$

where c, i, t, and s denote cohort, firm, year, and state of firm headquarters, respectively.

The dependent variable captures the gap in pay between a CEO and the average VP in the firm, following prior studies (e.g., Bognanno (2001), Henderson and Fredrickson (2001)). We take the average VP compensation as the benchmark to construct the *Pay Gap* proxy for two reasons. First, all the VPs can be subject to NCAs that restrict their mobility. They are, therefore, all susceptible to changes in state-level NCA enforceability (Chen et al. (2022), Garmaise (2011)). Second, using information for all VPs is helpful in capturing variations due to changes in the size of top management teams. We follow prior studies and take the natural logarithm of the highly skewed *Pay Gap*, but also use alternative proxies and transformations in robustness tests.

We measure the dependent variable at $t+1$ to ease reverse causality concerns and because adjustments to executive pay are not instantaneous and inevitably happen with a lag.

The main test variable, $NCA\ Enforceability\ Up/Down_{t,s}$, is a binary variable that equals one if state-level NCA enforceability has increased (or decreased) in the current or previous years, zero otherwise. We follow Kale et al. (2009) and include a vector of controls related to executive, firm, and industry characteristics that are expected to determine the size of the *Pay Gap*. Specifically, firm size, median industry-level pay gap, stock return volatility, and the number of business segments are firm characteristics that are predicted to directly affect the executive pay gap. In addition, Kale et al. (2009) highlight that the total VP utility in each internal tournament depends on the size of the prize the VPs obtain in the event of promotion and their probability of promotion. Promotion likelihood should be negatively correlated with prize size, since firms do not need to guarantee their executives a large expected benefit from a promotion when such an event is particularly likely. Thus, factors directly associated with VPs' promotion chances can be considered as determinants of the pay gap. These factors include variables capturing whether the CEO is new, an insider, or retiring, as well as CEO duality, CEO age, CEO tenure, number of VPs, whether the CFO is a VP, whether the firm has a propensity for relay succession, and industry homogeneity. Detailed variable definitions can be found in the Appendix. We measure all the executive characteristics, *Z*, at year $t+1$ and firm financials or industry characteristics, *X*, at year t. We do not discuss the expected signs of these controls here for brevity.

The baseline models include firm (μ_i) and year (ρ_t) fixed effects (FEs) in each cohort c to control for time-invariant omitted firm characteristics and time-specific factors. Firm FEs can also help capture the firm-level variations in NCA usage, since NCA usage should persist over time (Chen et al. (2022)). In addition, we control for the time-invariant state attributes with state FEs

(θ_s) in a cohort because firms may relocate their headquarters to other states (Bai, Fairhurst, and Serfling (2020), Chen et al. (2022)). The standard errors are robust to heteroskedasticity and clustered at the headquarter-state level.

B. Sample Selection

Our initial sample comes from the S&P ExecuComp database over the period 1993 to 2018. We use the "CEO annual flag" (CEOANN = CEO) to identify a firm's CEO. However, some firm-year annual flags either do not allow us to find a CEO or indicate the existence of multiple CEOs. BoardEx database helps us to fill or correct 42.2% of these observations. In particular, we identify CEOs by their "role name," considering variants of the term "CEO." Other non-CEO named corporate executive officers are categorized as VPs (Kale et al. (2009), Kini and Williams (2012)). We drop the firm-year observations still without an identifiable CEO or with missing CEO annual pay (ExecuComp item: TDC1). Consistent with Kini and Williams (2012), we retain firm-years as long as ExecuComp lists at least one senior executive besides the CEO.[2]

ExecuComp modified its reporting format of TDC1 after the passage of FAS 123R, making the comparability between pre- and post-FAS 123R problematic. We thus follow Coles, Daniel, and Naveen (2013) and Kini and Williams (2012) to recalculate the value of executives' total compensation to ensure data consistency across the sample period. Though our central focus is on *Total Gap*, we decompose total executive compensation into its short-term (ST) and long-term (LT) components and build two additional outcome variables: *ST Gap* and *LT Gap*. ST pay is the sum of salary and bonus, while LT pay mainly consists of stock and options grants. We

[2]However, requiring firms to have at least two or at least three VPs does not affect our results, as shown in Panel A of Table OA.15 in the Online Appendix.

exclude, from the regression, non-positive values that are less likely to reflect tournament dynamics.[3] When *Total Gap* values are excluded, we argue that VPs are not substantially incentivized in terms of monetary compensation, and we discard the same observations for both *ST Gap* and *LT Gap*.

Executives and senior managers are likely to locate in the states of their firms' headquarters (Bai et al. (2020)). We extract historical location data from Bai et al. (2020) for the period until 2003 and from 10-X Header Data from Bill McDonald's website for the subsequent period. We retain the Compustat location data when the historical data is not available. Firm-years without U.S. headquarters are dropped. Finally, we combine our sample with other financials, mainly from the Compustat and CRSP databases. Firms in the utility or financial industries or without recorded asset values are excluded. All continuous variables are winsorized at the 1st and 99th percentiles.

As previously explained, we obtain two different datasets to separately examine the effects of increases and decreases in NCA enforcement. Table 1 summarizes the descriptive statistics for the main variables used in our models. The sample for Enforceability Up (Down) events consists of 22,091 (20,770) distinct firm-year observations with 1,908 (1,868) unique firms. Because the

[3]Kini and Williams (2012) find that negative executive pay gaps mostly occur when the CEOs are founders who receive low compensation but have large equity stakes. A negative gap is also possible when the former CEO becomes a VP but continues to receive compensation that is higher than that of the new CEO. Thus, negative pay gaps are likely to introduce noise. We alternatively recalculate the variable in different ways: (1) We monotonically add a constant to make all pay gaps positive; (2) We replace the negative values with the median industry pay gap; and (3) When the CEO's pay is smaller than the VPs', we replace it with the industry median CEO pay. Next, we transform the dependent variables using natural logarithms. In all cases, the results are qualitatively similar to those from our baseline estimations (see Panel B, Table OA.15 in the Online Appendix).

control observations can be used multiple times, the sample sizes in the stacked regression are inflated. The mean (median) value of the *Total Gap* is larger than that reported in prior related studies (e.g., Kini and Williams (2012), Kubick and Masli (2016)), but this pattern is not surprising considering the rapid growth of CEO pay in recent years. The distributions of variables do not have significant differences between the two panels and are comparable to prior research (Acharya, Gabarro, and Volpin (2021), Hayes, Lemmon, and Qiu (2012), Kale et al. (2009)).

[INSERT TABLE 1 HERE]

IV. Empirical Results

A. Main Results

Table 2 presents findings for the baseline model on estimating the effects of Enforceability Up or Down events. Panel A, Column 1 reports results for specifications on how enhanced enforceability affects the executive pay gap without time-varying covariates. In Column 2, we add a comprehensive set of other variables following Kale et al. (2009). The results from both columns show positive, highly significant coefficients on *NCA Enforceability Up*, indicating that firms enlarge the pay gap if their headquarters are in a treated state. The effect is also economically meaningful: On average, compared with the control firms, the affected firms experience a growth in pay gap of 20.9% ($= \exp(0.191) - 1$), corresponding to an absolute increase of about \$0.84 million ($= \$3.98$ million $\times\ 20.9\%$). The effects of other variables in Column 2 are generally in line with the empirical results or theoretical predictions by Kale et al. (2009); in particular, they confirm the statistical and economic significance of firm size (Gabaix

and Landier (2008), Gabaix et al. (2014)). Given that the interquartile change in firm size leads to a \$3.44 million ($= \$3.98 \text{ million} \times [\exp(0.282 \times 2.21) - 1]$) larger pay gap, the impact of the enhanced enforceability is almost a quarter of this effect and, therefore, economically substantial.

We further provide insights into the two components of the executive pay gap, namely *ST Gap* and *LT Gap*, in Columns 3 to 6. The coefficients on *NCA Enforceability Up* continue to be positive and significant in both instances. The economic magnitudes, however, are distinct between the two dependent variables: Compared to a control firm, the average treated firm experiences an increase of about 5.9% in *ST Gap* and about 24.9% in *LT Gap*. Thus, *LT Gap* can be interpreted as the main driver of the change in *Total Gap*. This confirms the growing importance of the stock and option components in executive compensation (Graham et al. (2020)) and suggests that the observed variations in the pay gap are mainly due to long-term incentives.

We next turn our focus to reductions in NCA enforceability. We use the binary variable *NCA Enforceability Down* to capture state-level shocks that weaken NCA enforcement. In Panel B, we observe marginally significant or even insignificant negative effects of this test variable on the executive pay gap. The economic impacts on *Total Gap* and *LT Gap* caused by the downward shocks are generally smaller than those caused by upward shocks, suggesting asymmetric effects from variations in state-level enforceability. We note that *ST Gap* decreases by about 5.2%, which could relate to the fact there is greater flexibility in adjusting short-term incentives (salary and bonus). However, we caution the reader against interpreting this finding as a treatment effect since there are significant pre-trends in *ST Gap* as we show later. Our finding that a shock that reduces enforceability has comparatively weak or muted effects is consistent with previous work that examines the effect of NCA enforceability reductions on other corporate financial outcomes (e.g., Lakkis (2023), Lin et al. (2022), Mueller (2023), Tang et al. (2021)).

[INSERT TABLE 2 HERE]

In the Online Appendix, we follow prior research and use several alternative measures of the executive pay gap and model specifications. Tables OA.4 and OA.5 indicate that our findings for the increased enforceability events do not differ across any of the alternative dependent variables, test windows, and estimators. In contrast, the effects of *NCA Enforceability Down* on the executive pay gap are completely insignificant in most tests. Overall, we posit that companies expand the relative pay gap between the CEO and the VPs following enhanced NCA enforceability but keep it similar when enforceability becomes weaker.

B. Dynamic Effects

Our identification employs a DID method, so it is necessary to check whether it satisfies the underlying assumption of parallel trends. We extend our baseline analysis by examining the dynamic effects of a change in NCA enforceability. In particular, we decompose the periods preceding and following each event into five bins. The indicators are equal to one if a firm's headquarters are in a state that experiences or experienced the event (i) either three or four years into the future ($NCA\ Enforceability^{-3,-4}$); (ii) either one or two years into the future ($NCA\ Enforceability^{-1,-2}$); (iii) either in the current or previous year ($NCA\ Enforceability^{0,+1}$); (iv) either two or three years ago ($NCA\ Enforceability^{+2,+3}$); or (v) at least four years ago ($NCA\ Enforceability^{>=+4}$). We re-estimate the regressions of Columns 2, 4, and 6 of Table 2 after replacing the test variable *NCA Enforceability Up (Down)* with this set of binary variables.

The results reported in Table 3 show that in the Enforceability Up subsample, the assumption of parallel trends holds, and the effects of the treatment on the overall pay gap get

stronger over time. We further examine *ST Gap* and *LT Gap*. The statistically significant effect of increased NCA enforceability on *ST Gap* is relatively transient: It mainly exists in the first two periods but then becomes insignificant. By contrast, the impact of the treatment on *LT Gap* persists. The differences in timing are likely attributable to the nature of the different components in the executive pay package. Changes in long-term pay (stock and option grants) usually take longer to materialize and produce more significant effects than changes in short-term pay (salary and bonus).

In contrast, some pre-trends are evident and significant in the sample of Enforceability Down events. Specifically, for *Total Gap*, we report a negative and statistically significant effect in years -1 or -2. The pre-trend is stronger and more persistent when analyzing *ST Gap*, as all the test variables have negative and significant coefficients both before and after a Down event. There is no evidence of any significant change in *LT Gap* around a reduction in NCA enforceability. Therefore, the shocks that reduce NCA enforceability seem to coincide with unrelated trends that lead to reductions in *ST Gap*. The violations of the parallel trends' assumption call into question the reliability of the already weak findings relating to reductions in NCA enforceability.

[INSERT TABLE 3 HERE]

C. Executive Pay Levels

We next examine the effects of changes in NCA enforceability on the components of the executive pay gap—CEO and VP pay—to highlight differences in the adjustment frictions of the gap. Existing studies present mixed findings on the relation between NCA enforceability and executive pay levels. Garmaise (2011) reports a reduction in executive pay after increases in NCA

enforceability, whereas Kini et al. (2021) find a positive relation between increases in NCA enforceability and CEO compensation. Because the determinants of executive pay levels and pay gaps may differ, we adopt model settings from recent studies that specifically address executive pay levels (e.g., Kini et al. (2021)) and replace some control variables in the baseline model. The change in the model specification leads to a reduction in sample size for this test.

Column 1 in Panel A of Table 4 corroborates the findings of Kini et al. (2021), who argue that the affected CEOs get higher pay to reflect higher unemployment risks. We find significant and stable increases in both the short- and long-term components of their pay. We further find that, on average, VPs also benefit from tightened mobility in the labor market, although the growth in VP pay (*Total Pay*, *ST Pay*, or *LT Pay*) is lower than the growth in CEO pay. This evidence indicates that CEOs are not the only ones who benefit from restricted mobility by receiving higher compensation. In contrast, executives generally experience no changes in compensation after events that weaken NCA enforceability. This is consistent with prior evidence that firms are reluctant to reduce CEO pay (Dittmann, Maug, and Zhang (2011)) and that firms respond to labor supply shocks by asymmetrically adjusting executive pay, such as by increasing it when personal income tax rates rise but maintaining similar levels when tax rates fall (Bennett, Coles, and Wang (2021)). The only exception, in our case, is a significant increase in VP ST Pay, which appears to be the main driver of the reduction in *ST Gap* reported in Table 3. However, as we showed in Column 5 of Table 3, this is possibly an unrelated trend, as it does not appear to result from the treatment effect of *NCA Enforceability Down*. Together, these findings suggest asymmetric effects on executive pay levels following the two types of enforcement change events, which help explain the asymmetry we observe in pay gap revisions.

[INSERT TABLE 4 HERE]

V. Mechanisms Underlying the Baseline Findings

A. Outside Opportunities

We argue that the mechanism underlying our findings is that firms enlarge the executive pay gap to motivate and incentivize VPs whenever VPs' external mobility becomes more restricted. Thus, the degree to which shocks to NCA enforceability impact the pay gap should depend on the VPs' pre-shock external opportunities and incentives (also in relation to their internal promotion prospects). Adjustments to the pay gap should be less significant when the ex ante relative importance of VPs' external opportunities is more limited. We consider several variables that capture variations in this dimension to test our conjecture.

We first focus on the incidence of outside hires at the state-industry level, since executive mobility often happens within the same state or industry (Ma et al. (2020), Yonker (2017)). We expect that firms belonging to the cohorts in which CEOs tend to be promoted internally will rely less on the external labor market and thus be less sensitive to mobility shocks.[4] We calculate the fraction of insider CEOs in each state and industry and trisect the sample. In this test, we exclude the ever-relocated firms that are subject to opportunistic actions. In Columns 1 and 2 of Panel A,

[4] A potential concern is that the likelihood of promoting insider CEOs is boosted by changes in NCA enforceability that make external hiring expensive. However, Cziraki and Jenter (2022) and Chen et al. (2022) both show that firms' hiring of CEOs is not affected by NCA frictions. In our sample, we find similar results; that is, neither an increase nor a decrease in enforceability affects CEO hirings (see Panel C, Table OA.15 in the Online Appendix).

Table 5, the top tercile sample shows a significantly lower change in pay gap in response to increases in NCA enforceability, compared to the bottom tercile sample.

Similarly, a VP can become a CEO in her current firm (move vertically) or another one (move diagonally). However, if pay levels are not significantly different across firms, she may be less motivated to leave the current employer and more indifferent to changes in NCA enforceability. We calculate the skewness of CEO pay at time t in a focal firm's headquarter state (excluding the focal firm). Executives in states with a more skewed distribution of CEO pay are more likely to get higher compensation once they leave their current firm. We also remove the observations for ever-relocated firms, as above. In Columns 3 and 4 of Panel A, the top tercile sample based on CEO pay skewness shows a greater change in the pay gap than the bottom tercile sample when enforceability gets stronger.

We subsequently relax the assumption on the prevalence of within-state mobility and consider a firm-specific cross-sectional variable based on whether a CEO is also a founder. Given their significant voting power (on average), founder CEOs are rarely forced to leave their firms. Thus, the incentives of VPs working for founder CEOs mostly arise from external tournaments, since within-firm promotion expectations are limited. We use job titles from ExecuComp to identify founder CEOs. Interim CEOs are excluded from this test because they are likely to depart shortly. In Columns 5 and 6 of Panel A, we find that the increase in the pay gap is significantly larger for firms with founder CEOs than for the non-founder sample. This is consistent with the notion that VPs working for a founder require more internal tournament incentives to compensate them for the lost outside opportunities.

For completeness, we replicate all the above tests in the Enforceability Down sample and report the results in Panel B. The above variables do not affect the relation between the pay gap

and *NCA Enforceability Down* in a statistically significant way, which is consistent with the weak and largely insignificant unconditional effects of Down events reported previously.

We next consider the cross-sectional variation in three variables that reflect outside job opportunities. First, executives' career concerns are associated with the executives' age. Younger VPs usually have more career prospects and thus are more sensitive to the loss of outside options caused by the new rules. Career concerns are weaker for older VPs because the short prospective career decreases the value of opportunities in the external job market (Gibbons and Murphy (1992)). Second, job opportunities depend on the talents and abilities of managers (Rajgopal, Shevlin, and Zamora (2006)). More able executives with excellent past performance are more welcome in the labor market. Third, executives working in less monopolistic product markets can more easily transfer their existing industry-specific skills to competitors.

Building on the above arguments, we augment our baseline specification with interactions of *NCA Enforceability Up* with the following three binary variables. *Young VPs* equals one if the average age of a firm's VPs in a particular year is below the annual sample median. *High Ability* equals one if a firm's managerial ability is above the industry median value each year. As it is impractical and difficult to distinguish the ability of an individual executive, we use the proxy from Demerjian, Lev, and McVay (2012) that models the ability of the whole top management team. *Low HHI* equals one if the Herfindahl–Hirschman Index (HHI) of sales within an industry is below the annual sample median. We use the HHI data developed through textual analysis of company filings by Hoberg and Phillips (2016). Columns 1 to 3 in Panel C of Table 5 show that the effects of increased NCA restrictions on the pay gap are more pronounced in firms with younger VPs, more able managers, and lower product market concentration. The findings are in line with our argument that exogenous changes in VP mobility drive the changes in pay gaps.

However, there is no evidence that these variations drive the effects of Down events on executive pay gaps, as reported in Columns 4 to 6.

[INSERT TABLE 5 HERE]

B. The Role of RPE

In addition to the internal tournament arising from the presence of an executive pay gap, relative performance evaluation (RPE) can induce tournament-style incentives by setting performance targets relative to a benchmark (Wruck and Wu (2022)).[5] Specifically, performance-vesting (PV) provisions in compensation contracts can be set based on firm performance relative to a range of selected indices or peer companies. Managers are likely to focus more on idiosyncratic firm-level outcomes so as to outperform the selected peers and get higher compensation. As the use of PV provisions is effective in providing incentives and encouraging risk-taking (Bettis et al. (2018)), the board may adjust such provisions as a substitute for reductions in external tournament opportunities (Bettis et al. (2010)). If this is the case, then executives could already be shielded from systematic risks after losing outside options, and, consequently, the required level of compensation or internal tournament incentives could remain similar or even be lower after a positive shock to NCA enforceability. This conjecture is at odds with our prediction and main findings, but it raises two questions: (1) Is the effect of an NCA enforceability shock on the implicit incentives arising from internal tournaments lessened or

[5] Although RPE compensates managers based on their relative performance compared to peers, few firms reciprocally select each other as peers to set RPE (De Angelis and Grinstein (2020)). Therefore, these tournament-style incentives have fundamentally different features compared to rank-order tournaments.

crowded out in firms that rely on PV grants? And (2) is the presence of RPE provisions in compensation contracts affected by enforceability shocks?

To answer the former question, we construct a measure of RPE that captures the presence of PV equity awards. *RPE* equals one if a firm's executive compensation includes PV provisions according to ISS Incentive Lab data (e.g., Bettis et al. (2018), De Angelis and Grinstein (2020), Wruck and Wu (2022)). The test sample is limited to the period from 1998 to 2018 due to data availability. In Panel A of Table 6, we extend the baseline findings of Table 2 for the total executive pay gap by interacting the test variable, *NCA Enforceability*, with the conditional variable *RPE*. Consistent with our previous findings, we find significant changes in the pay gap only for *NCA Enforceability Up* (Column 1). For both Enforceability Up and Enforceability Down events, there are no significant estimates for either the RPE dummy or its interaction with *NCA Enforceability*. Therefore, the impact of an NCA enforceability event on the executive pay gap is not different in firms that use PV provisions.

To address the latter question, we adopt a two-stage method (see, for example, Albuquerque (2009) and Jenter and Kanaan (2015)) to indirectly assess whether an executive's compensation contains RPE provisions (implicit RPE).[6] In the first stage, we estimate the

[6]Explicit RPE plans are typically disclosed voluntarily and may not reflect the board's consideration of systematic performance. Moreover, focusing on explicit RPE reduces the available sample size. Therefore, we opt to model the presence of RPE implicitly. However, when we alternatively use explicit RPE information from the ISS database as the dependent variable, we do not observe significant changes following changes in NCA enforceability (Panel D, Table OA.15 in the Online Appendix).

following model:

$$(2) \qquad Firm\ Performance_{i,t} = \gamma + \delta \times Peer\ Performance_{i,t} + \epsilon_{i,t},$$

where for a particular firm-year, we use the firm's stock return to determine the component of its performance that is attributable to systematic factors (*Systematic Performance*), which can be predicted by the equally-weighted stock performance of its peers in terms of industry and size (Albuquerque (2009), Na (2020)), that is, $\hat{\gamma} + \hat{\delta} \times Peer\ Performance_{i,t}$. *Unsystematic Performance* is then obtained as a residual, $\hat{\epsilon}_{i,t}$ ($Firm\ Performance_{i,t} - \hat{\gamma} - \hat{\delta} \times Peer\ Performance_{i,t}$), which reflects firm-specific outcomes after accounting for peer shocks. RPE should make an executive's pay less sensitive to systematic performance factors outside her control. In the second stage, we examine the relation between a CEO's or an average VP's pay and *Systematic Performance* while controlling for *Unsystematic Performance* and whether the sensitivity of executive pay to systematic performance changes after an NCA enforceability event in the following model:

$$\begin{aligned}(3) \quad Ln\ (CEO/VP\ Pay)_{c,i,t+1} = {} & \alpha + \beta_1 \times NCA\ Enforceability\ Up/Down_{c,t,s} \\ & + \beta_2 \times Systematic\ Performance_{c,i,t} \\ & + \beta_3 \times NCA\ Enforceability\ Up/Down_{c,t,s} \times Systematic\ Performance_{c,i,t} \\ & + \beta_4 \times Unsystematic\ Performance_{c,i,t} \\ & + \beta_5 \times NCA\ Enforceability\ Up/Down_{c,t,s} \times Unsystematic\ Performance_{c,i,t} \\ & + \phi \times X'_{c,i,t} + \mu_{c,i} + \rho_{c,t} + \theta_{c,s} + \varepsilon_{c,i,t+1},\end{aligned}$$

where *X*' represents variables capturing firm or industry characteristics measured at year t, as described in Section IV.C. $\mu_{c,i}$, $\rho_{c,t}$, $\theta_{c,s}$ denote firm, year, and state FEs within each cohort, respectively.

The results are reported in Panel B of Table 6. As in Section IV.C, we confirm that both CEOs and VPs receive significant pay raises in the Enforceability Up sample (Columns 1 and 2) but not in the Enforceability Down sample (Columns 3 and 4). In all four specifications, both types of performance are positively associated with executive pay. However, the interaction between *NCA Enforceability* and *Systematic Performance* is never statistically significant, implying that executive pay does not become significantly more or less sensitive to systematic performance after Enforceability Up or Down events. Thus, the level of RPE in executives' pay does not seem to be affected by enforceability shocks.

[INSERT TABLE 6 HERE]

C. Retention Considerations

Retention costs for executives are likely to change in response to mobility shocks. For example, companies may reduce their efforts to retain executives when markets become less mobile due to increased NCA enforceability. In such cases, one might expect no changes to CEO and VP pay levels or, theoretically, even reductions to pay levels, even though the latter is rarely observed in practice (Dittmann et al. (2011), Edmans et al. (2023)). This could lead to less significant changes in the pay gap following positive enforceability shocks. The reverse, where firms enhance CEO and VP pay levels in response to an increase in retention needs resulting from weakened NCA enforceability, could also hold. This could lead to greater pay gaps after NCA

Enforceability Down events. In either case, retention costs might work against our finding results and lead us to underestimate the magnitude of the tournament incentive substitution we report. So, in this section, we turn our attention to the role of retention costs.

First, we acknowledge that retention costs for executives are positively correlated with the executives' outside job opportunities. Retention becomes more expensive for firms when managers have more job-switching opportunities, assuming a scarcity of talent supply (Rajgopal et al. (2006)). Thus, the variables *Fraction of Insider CEOs*, *Skewness of CEO Pay*, *Young VPs*, *High Ability*, and *Low HHI*, which we use in Section V.A to capture differences in outside options, may also relate to retention considerations. Retention costs, such as increased pay (Bennett et al. (2021)), are arguably higher for firms in states where CEOs tend to be hired from other firms and where CEO pay is highly skewed, as voluntary moves to other firms would be more frequent and potentially more rewarding in these states. Similarly, it is more challenging and costly to retain executives who, due to their youth and high ability, have numerous outside opportunities and a willingness to change jobs. Last, a low product market concentration should be associated with significant outside opportunities for executives—and high retention costs for firms.

Following the retention argument above, an *NCA Enforceability Down (Up)* event could cause a positive (negative) effect on the executive pay gap, which could be amplified in firms with high retention costs. However, our findings in Table 5 do not align with these expectations. The impact of a decrease in NCA enforceability on the executive pay gap is not affected by any of the five proxies of retention costs. Regarding Enforceability Up events, we observe that in treated firms with high retention costs, the post-event change in the pay gap is actually positive, and it is larger than the change for treated firms with low retention costs.

Second, we conduct some tests to reinforce our conclusion that a retention story does not dilute the effects of enforceability events on the executive pay gap. We consider three additional proxies of retention costs that are plausibly unrelated to, or that are not directly tied to, executives' outside opportunities. Firstly, our *Litigation Industry* dummy represents industries with a high incidence of litigation. Executives' reputation concerns about litigation can increase executives' voluntary turnover and thus firms' retention costs (Andrus, Withers, Courtright, and Boivie (2019)). Secondly, retaining executives is more valuable—and thus more costly—for firms with a high market-to-book ratio (*High Mkt to Book*), as these firms typically have significant growth opportunities and trade secrets (Chen et al. (2022)). Thirdly, firms that rely less on skilled labor (*Low Skill Reliance*) have lower retention costs, given their relative lack of labor (including executives) with highly valuable skills (Qiu and Wang (2021)). We find that the interactions between these three variables and the test variable *NCA Enforceability* in Table 7 are never statistically significant.

[INSERT TABLE 7 HERE]

We also separately investigate whether a retention mechanism can explain changes in CEO and VP pay levels when NCA enforceability increases or decreases. The results, presented in Table OA.6, show that in most cases such a mechanism has no explanatory power. Overall, these results indicate that retention considerations or costs do not significantly affect our main findings.

D. Firm Value Implications

So far, we have provided robust evidence that firms widen their executive pay gaps following stricter enforcement, while the effects are muted following weaker enforcement. We

next evaluate whether firms' response to stricter NCA enforcement has implications for firm value.

Recent studies highlight that strict mobility restrictions caused by NCA enforcement stifle profitability and productivity because they lower the effectiveness of talent reallocation (Anand et al. (2018), He and Wintoki (2023), Shi (2023)). At the same time, a properly designed compensation system helps develop managerial talents in the internal labor market and creates better performance (Hvide (2002), Kale et al. (2009), Lazear and Rosen (1981)). Such effects are potentially more evident in less mobile labor markets, where executives are more likely to actively participate in rank-order tournaments. Therefore, a trade-off emerges, and the impact of the change in the executive pay gap may counteract the negative impact in firm value from NCA enforceability increases.

We examine changes in firm value in terms of monthly stock returns following increases in NCA enforcement, distinguishing between treated firms with positive and treated firms with non-positive variations in pay gap. We consider the change in the pay gap either from year $t-1$ to t or from year $t-1$ to $t+1$. Since 1996 is the earliest treated year in our sample, the constructed dataset of CRSP monthly returns starts from 1997. In each calendar month, we build a portfolio return as follows: A long position is assumed to be taken in firms with expanded pay gaps, and a short position is assumed in firms with non-expanded pay gaps. The portfolio return is either equally weighted or market-capitalization-weighted (the market capitalization is recorded at the end of the previous month). We use the Newey-West estimator with four lags to regress the monthly portfolio return on the respective Fama-French five factors.[7] As shown in Table 8, the

[7]The dataset is available on Kenneth R. French's Data Library.

portfolios often generate significant positive alphas across different holding periods, i.e., 12, 24, or 36 months. Overall, we report potential heterogeneous value effects when executive mobility is restrained. Firms that decide to enlarge their pay gaps in response to increased NCA enforcement perform significantly better on the stock market.

[INSERT TABLE 8 HERE]

VI. Conclusion

We study how exogenous variations in non-compete agreement (NCA) enforceability affect a firm's executive pay gap (i.e., the gap between CEO and VP pay), which the firm can use to provide tournament incentives to VPs. We find that a positive shock to enforceability causes a significant decrease in executive mobility and, more importantly, an increase in the pay gap. The implications of these findings are significant since many studies find evidence that income dispersion among top executives has substantial effects on financial decisions and other corporate outcomes (e.g., Kale et al. (2009), Kini and Williams (2012)).

Our baseline results are more pronounced for firms in states with a high proportion of outside-hired CEOs or with a more skewed distribution of CEO pay; firms with founder CEOs, younger VPs, or more capable top executives; and firms in industries with lower product market concentration. VPs in firms with these characteristics are expected to value external job opportunities more and to require more significant internal tournament incentives when these opportunities are curtailed.

In contrast, we find that the effects of reduced NCA enforcement are weak or even insignificant, as the board does not significantly adjust the executive pay gap. We present several

pieces of evidence that question the value of shocks decreasing NCA enforceability in our setting and which potentially help explain these asymmetric findings. Alternative explanations, such as the inclusion of performance-vesting provisions in equity pay and the considerations surrounding retention issues, do not drive our main findings.

We document that the potential adverse effects on firm performance due to increases in NCA enforceability vary significantly between firms with and firms without post-shock increases in pay gaps. A portfolio that goes long in treated firms with increased pay gaps and short in the remaining treated firms can generate positive abnormal returns. Thus, a widened pay gap can protect shareholder value from the negative effects of a less mobile executive labor market.

Our findings help explain executive compensation practices in the presence of managerial mobility. In April 2024, the Federal Trade Commission (FTC) released its final rule prohibiting employers from imposing or enforcing new NCAs on workers, including top executives.[8] We anticipate firms' responses to this decision to be muted with regards to compensation incentives, in particular, executive pay gaps, since our results indicate a substitution effect between external and internal tournament incentives only when executive mobility is constrained.

[8] See, "FTC announces rule banning noncompetes," FTC, April 23, 2024.

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TABLE 1

Summary Statistics

This table reports the descriptive statistics (including the number of unique observations, mean, lower quartile, median, and upper quartile) for the variables used in the baseline regression models. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) for the period 1993–2018. Detailed variable definitions can be found in the Appendix.

Panel A: Enforceability Up

	Obs.	Mean	Lower Quartile	Median	Upper Quartile
Compensation variables					
Total Gap (in 000s)	22,091	3,982.31	775.42	2,033.51	4,805.19
ST Gap (in 000s)	21,583	622.32	262.21	437.08	695.21
LT Gap (in 000s)	19,490	3,398.95	590.99	1,751.50	4,326.21
Median Industry Gap (in 000s)	22,091	2,592.10	722.04	1,666.52	3,571.50
Median Industry ST Gap (in 000s)	21,583	507.50	283.55	422.61	586.41
Median Industry LT Gap (in 000s)	19,490	2,285.86	551.83	1,465.00	3,268.68
Executive characteristics					
CEO Age	22,091	55.62	51.00	56.00	60.00
CEO Tenure	22,091	7.23	2.00	5.00	10.00
New CEO	22,091	0.10	0.00	0.00	0.00
CEO Is Insider	22,091	0.66	0.00	1.00	1.00
Retiring CEO	22,091	0.10	0.00	0.00	0.00
Duality	22,091	0.55	0.00	1.00	1.00
No. of VPs	22,091	4.78	4.00	5.00	5.00
Propensity of Relay Succession	22,091	0.54	0.00	1.00	1.00
CFO Is VP	22,091	0.89	1.00	1.00	1.00
Firm/Industry Characteristics					
Ln (Total Asset)	22,091	7.20	6.05	7.09	8.26
Industry Homogeneity	22,091	0.11	0.06	0.10	0.14
Stk. Return Volatility	22,091	0.46	0.30	0.41	0.55
No. of Segments	22,091	2.51	1.00	2.00	4.00

Table 1 (Continued)

Panel B: Enforceability Down

	Obs.	Mean	Lower Quartile	Median	Upper Quartile
Compensation variables					
Total Gap (in 000s)	20,770	4,035.31	775.84	2,033.74	4,863.16
ST Gap (in 000s)	20,155	613.85	252.69	428.76	694.29
LT Gap (in 000s)	18,401	3,424.80	583.22	1,760.55	4,358.76
Median Industry Gap (in 000s)	20,770	2,563.96	698.20	1,629.51	3,547.12
Median Industry ST Gap (in 000s)	20,155	502.47	277.88	413.62	582.48
Median Industry LT Gap (in 000s)	18,401	2,254.52	539.75	1,410.83	3,222.66
Executive characteristics					
CEO Age	20,770	55.57	51.00	56.00	60.00
CEO Tenure	20,770	7.25	2.00	5.00	10.00
New CEO	20,770	0.10	0.00	0.00	0.00
CEO Is Insider	20,770	0.66	0.00	1.00	1.00
Retiring CEO	20,770	0.10	0.00	0.00	0.00
Duality	20,770	0.55	0.00	1.00	1.00
No. of VPs	20,770	4.79	4.00	5.00	5.00
Propensity of Relay Succession	20,770	0.53	0.00	1.00	1.00
CFO Is VP	20,770	0.89	1.00	1.00	1.00
Firm/Industry Characteristics					
Ln (Total Asset)	20,770	7.20	6.00	7.07	8.27
Industry Homogeneity	20,770	0.11	0.06	0.09	0.14
Stk. Return Volatility	20,770	0.46	0.30	0.41	0.55
No. of Segments	20,770	2.53	1.00	2.00	4.00

TABLE 2

Baseline Regressions

This table reports the effect of a state-level change in the enforceability of non-compete agreements (NCAs) on the executive pay gap. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) for the period 1993–2018. The coefficients are estimated using the stacked-regression method by Baker et al. (2022) for a test window of $t \pm 10$ (t is the event year), using firms headquartered in never treated states as "clean" controls. The dependent variables reflect alternative definitions of the pay gap and are measured in year $t + 1$. *NCA Enforceability Up (Down)* is measured in year t and is a binary variable that is set to one if a firm is headquartered in a state that has experienced an increase (decrease) in NCA enforceability over the current or previous years. Control variables related to executive characteristics (*New CEO*, *CEO Is Insider*, *Retiring CEO*, *Duality*, *No. of VPs*, *Propensity of Relay Succession*, *CFO is VP*, *Ln (CEO Age)*, and *Ln (CEO Tenure)*) are recorded in year $t + 1$ while other controls in year t. All models include firm-cohort, year-cohort, and headquarter-state-cohort fixed effects. Detailed variable definitions can be found in the Appendix. The standard errors in parentheses are clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to significance levels of 1%, 5%, and 10%, respectively.

Table 2 (Continued)

Panel A: Enforceability Up

	Ln (Total Gap)		Ln (ST Gap)		Ln (LT Gap)	
	1	2	3	4	5	6
NCA Enforceability Up	0.190***	0.191***	0.060**	0.055**	0.227***	0.218***
	(0.049)	(0.051)	(0.028)	(0.026)	(0.069)	(0.076)
New CEO		0.107*		-0.076***		0.153***
		(0.061)		(0.027)		(0.039)
CEO Is Insider		-0.200***		-0.154***		-0.188***
		(0.031)		(0.022)		(0.036)
Industry Homogeneity		0.207		-0.186*		0.347
		(0.177)		(0.108)		(0.245)
Retiring CEO		-0.085**		-0.011		-0.064
		(0.040)		(0.031)		(0.055)
Duality		0.103***		0.081***		0.129***
		(0.026)		(0.030)		(0.033)
No. of VPs		0.021*		0.030***		0.026**
		(0.011)		(0.008)		(0.010)
Propensity of Relay Succession		-0.047***		-0.053***		-0.058***
		(0.016)		(0.014)		(0.018)
Ln (Median Industry Gap)		0.041*				
		(0.023)				
Ln (Median Industry ST Gap)				0.205***		
				(0.036)		
Ln (Median Industry LT Gap)						0.055***
						(0.020)
CFO Is VP		0.050		-0.013		0.000
		(0.040)		(0.024)		(0.047)
Ln (CEO Age)		-0.399***		0.167**		-0.493***
		(0.123)		(0.081)		(0.139)
Ln (CEO Tenure)		0.066***		0.056***		0.054**
		(0.024)		(0.015)		(0.022)
Ln (Total Asset)		0.282***		0.044***		0.279***
		(0.029)		(0.011)		(0.049)
Stk. Return Volatility		0.043		-0.065		0.132
		(0.084)		(0.048)		(0.144)
No. of Segments		-0.002		0.009		-0.007
		(0.013)		(0.009)		(0.010)
Observations	83,849	83,849	81,839	81,839	74,529	74,529
R-squared	0.623	0.634	0.632	0.649	0.616	0.626
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 2 (Continued)

Panel B: Enforceability Down

	Ln (Total Gap)		Ln (ST Gap)		Ln (LT Gap)	
	1	2	3	4	5	6
NCA Enforceability Down	-0.102**	-0.079*	-0.059*	-0.047*	-0.040	-0.020
	(0.048)	(0.042)	(0.032)	(0.027)	(0.093)	(0.082)
Observations	75,381	75,381	73,021	73,021	67,374	67,374
R-squared	0.642	0.652	0.649	0.666	0.625	0.634
Controls from Panel A	No	Yes	No	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

TABLE 3

Dynamic Effects

This table reports the effect of a state-level change in the enforceability of non-compete agreements (NCAs) on the executive pay gap. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) for the period 1993–2018. The coefficients are estimated using the stacked-regression method by Baker et al. (2022) for a test window of $t \pm 10$ (t is the event year), using firms headquartered in never treated states as "clean" controls. The regressions in Columns 2, 4, and 6 of Panels A and B in Table 2 are re-estimated by replacing the test variable *NCA Enforceability Up (Down)* with a set of binary variables that identify periods preceding and following an increase (decrease) in NCA enforceability. These binary variables are equal to one if the firm is headquartered in a state that: (i) experiences a change in enforceability in either three or four years in the future (*NCA Enforceability*$^{-3,-4}$); (ii) experiences a change in enforceability in either one or two years in the future (*NCA Enforceability*$^{-1,-2}$); (iii) has experienced a change in enforceability in either the current or previous year (*NCA Enforceability*$^{0,+1}$; (iv) has experienced a change in enforceability either two or three years ago (*NCA Enforceability*$^{+2,+3}$); (v) has experienced a change in enforceability four or more years ago (*NCA Enforceability*$^{>=+4}$). The dependent variables reflect alternative definitions of the pay gap and are measured in year $t + 1$. All models include firm-cohort, year-cohort, and headquarter-state-cohort fixed effects. Detailed variable definitions can be found in the Appendix. The standard errors in parentheses are clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to significance levels of 1%, 5%, and 10%, respectively.

	Enforceability Up			Enforceability Down		
	Ln (Total Gap)	Ln (ST Gap)	Ln (LT Gap)	Ln (Total Gap)	Ln (ST Gap)	Ln (LT Gap)
	1	2	3	4	5	6
NCA Enforceability$^{-3,-4}$	0.078	0.036	0.047	-0.135	-0.119**	-0.175
	(0.122)	(0.045)	(0.116)	(0.106)	(0.051)	(0.104)
NCA Enforceability$^{-1,-2}$	0.084	0.002	0.068	-0.161*	-0.084***	-0.160
	(0.075)	(0.031)	(0.084)	(0.090)	(0.024)	(0.100)
NCA Enforceability$^{0,+1}$	0.219***	0.084*	0.215**	-0.163**	-0.092**	-0.152
	(0.075)	(0.042)	(0.094)	(0.063)	(0.045)	(0.111)
NCA Enforceability$^{+2,+3}$	0.222***	0.050	0.285***	-0.180*	-0.102**	-0.097
	(0.080)	(0.031)	(0.082)	(0.106)	(0.043)	(0.121)
NCA Enforceability$^{>=+4}$	0.254***	0.060	0.256**	-0.154	-0.120**	-0.077
	(0.072)	(0.041)	(0.110)	(0.100)	(0.051)	(0.145)
Observations	83,849	81,839	74,529	75,381	73,021	67,374
R-squared	0.634	0.649	0.626	0.652	0.666	0.634
Controls from Table 2	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

TABLE 4

Executive Pay Levels

This table reports the effect of a state-level change in the enforceability of non-compete agreements (NCAs) on CEO and VP pay levels and their components. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) for the period 1993–2018. The coefficients are estimated using the stacked-regression method by Baker et al. (2022) for a test window of $t \pm 10$ (t is the event year), using firms headquartered in never treated states as "clean" controls. The dependent variables reflect alternative definitions of executive compensation, i.e., CEO or VP pay levels, and are measured in year $t + 1$. *NCA Enforceability Up (Down)* is measured in year t and is a binary variable that is set to one if a firm is headquartered in a state that has experienced an increase (decrease) in NCA enforceability over the current or previous years. All models include firm-cohort, year-cohort, and headquarter-state-cohort fixed effects. Detailed variable definitions can be found in the Appendix. The standard errors in parentheses are clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to significance levels of 1%, 5%, and 10% respectively.

Table 4 (Continued)

Panel A: Enforceability Up

	Ln (CEO Pay)	Ln (CEO ST Pay)	Ln (CEO LT Pay)	Ln (VP Pay)	Ln (VP ST Pay)	Ln (VP LT Pay)
	1	2	3	4	5	6
NCA Enforceability Up	0.148***	0.046**	0.167**	0.084***	0.036**	0.115*
	(0.033)	(0.018)	(0.072)	(0.030)	(0.015)	(0.064)
Industry Homogeneity	0.174*	-0.135*	0.362**	0.139	-0.043	0.262*
	(0.101)	(0.076)	(0.177)	(0.084)	(0.052)	(0.154)
ROA	0.210***	0.083***	0.360***	0.164***	0.103***	0.230***
	(0.065)	(0.023)	(0.090)	(0.050)	(0.023)	(0.083)
Ln (Total Asset)	0.315***	0.091***	0.330***	0.284***	0.090***	0.349***
	(0.016)	(0.010)	(0.022)	(0.014)	(0.010)	(0.018)
Mkt to Book Ratio	0.116***	0.015***	0.145***	0.102***	0.011***	0.161***
	(0.008)	(0.005)	(0.013)	(0.006)	(0.003)	(0.008)
Leverage	-0.189***	-0.046	-0.216***	-0.148***	-0.000	-0.259***
	(0.057)	(0.035)	(0.058)	(0.039)	(0.025)	(0.064)
Ln (Median Industry CEO Pay)	0.044**					
	(0.019)					
Ln (Median Industry CEO ST Pay)		0.189***				
		(0.021)				
Ln (Median Industry CEO LT Pay)			0.064***			
			(0.021)			
Ln (Median Industry VP Pay)				0.041***		
				(0.013)		
Ln (Median Industry VP ST Pay)					0.170***	
					(0.016)	
Ln (Median Industry VP LT Pay)						0.054***
						(0.016)
Cash Ratio	-0.016	-0.108*	0.035	-0.043	-0.092**	-0.002
	(0.048)	(0.057)	(0.059)	(0.036)	(0.037)	(0.077)
Dividend Payer	-0.039*	-0.030**	-0.026	-0.019	-0.021**	-0.004
	(0.022)	(0.012)	(0.023)	(0.016)	(0.010)	(0.022)
Stk. Return Volatility	0.056	-0.046	0.187**	0.084***	-0.020	0.290***
	(0.051)	(0.035)	(0.082)	(0.026)	(0.020)	(0.059)
Observations	82,426	80,574	72,968	82,426	80,574	72,968
R-squared	0.755	0.759	0.704	0.803	0.806	0.726
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 4 (Continued)

Panel B: Enforceability Down

	Ln (CEO Pay)	Ln (CEO ST Pay)	Ln (CEO LT Pay)	Ln (VP Pay)	Ln (VP ST Pay)	Ln (VP LT Pay)
	1	2	3	4	5	6
NCA Enforceability Down	-0.045	-0.014	-0.028	0.010	0.035***	-0.034
	(0.038)	(0.015)	(0.056)	(0.028)	(0.011)	(0.059)
Observations	74,180	71,935	65,982	74,180	71,935	65,982
R-squared	0.762	0.771	0.710	0.809	0.815	0.730
Controls from Panel A	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

TABLE 5

Mechanisms Underlying the Baseline Findings

This table reports the effect of a state-level change in the enforceability of non-compete agreements (NCAs) on the executive pay gap. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) for the period 1993–2018. The coefficients are estimated using the stacked-regression method by Baker et al. (2022) for a test window of $t \pm 10$ (t is the event year), using firms headquartered in never treated states as "clean" controls. The regressions in Column 2 of Panels A and B in Table 2 are re-estimated by using sub-sets of observations and adding interaction terms for the test variable *NCA Enforceability Up (Down)*. In Panels A and B, Columns 1 and 2 report the results for the top and bottom terciles of the fraction of insider CEOs in the same industry as the firm's state, Columns 3 and 4 report the results for the top and bottom terciles of the skewness of CEO pay for the state the firm belongs to, while in Columns 5 and 6, the split is based on whether the CEO is also the founder of the firm, with founders required not to be interim CEOs. Panels A and B also report the *p*-values of *F*-statistics testing the differences in coefficients on *NCA Enforceability Up (Down)* between the respective partitioned subsamples. In Panel C, *NCA Enforceability Up (Down)* is interacted with measures of average VP age, managerial ability score (Demerjian et al. (2012)), and product market HHI (Hoberg and Phillips (2016)). *Young VPs* equals one if the average age of VPs is below the sample annual median; *High Ability* equals one if managerial ability is above the sample annual median in the same industry; *Low HHI* equals one if the HHI is below the annual median in the same industry. The dependent variable is *Ln (Total Gap)*, measured in year $t + 1$. *NCA Enforceability Up (Down)* is measured in year t and is a binary variable that is set to one if a firm is headquartered in a state that has experienced an increase (decrease) in NCA enforceability over the current or previous years. All models include firm-cohort, year-cohort, and headquarter-state-cohort fixed effects. Detailed variable definitions can be found in the Appendix. The standard errors in parentheses are clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to significance levels of 1%, 5%, and 10%, respectively.

Table 5 (Continued)

Panel A: State Insider, Skewness of CEO Pay, and Founder CEOs: Enforceability Up

	Ln (Total Gap)					
	Fraction of insider CEOs		Skewness of CEO pay		CEO is founder	
	Top tercile	Bottom tercile	Top tercile	Bottom tercile	Founder	Non-founder
	1	2	3	4	5	6
NCA Enforceability Up	0.115**	0.486***	0.420***	0.088	0.374***	0.194***
	(0.057)	(0.156)	(0.074)	(0.057)	(0.085)	(0.054)
p-value	(0.027)		(0.001)		(0.072)	
Observations	24,420	23,615	23,103	22,431	6,888	74,353
R-squared	0.678	0.630	0.659	0.695	0.666	0.636
Controls from Table 2	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: State Insider, Skewness of CEO Pay, and Founder CEOs: Enforceability Down

	Ln (Total Gap)					
	Fraction of insider CEOs		Skewness of CEO pay		CEO is founder	
	Top tercile	Bottom tercile	Top tercile	Bottom tercile	Founder	Non-founder
	1	2	3	4	5	6
NCA Enforceability Down	-0.076	-0.163	-0.100	-0.116	0.133	-0.097**
	(0.088)	(0.127)	(0.065)	(0.080)	(0.313)	(0.045)
p-value	(0.574)		(0.876)		(0.456)	
Observations	21,997	21,261	20,659	20,335	6,301	66,782
R-squared	0.699	0.650	0.684	0.705	0.667	0.656
Controls from Table 2	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 5 (Continued)

Panel C: Average Age of VPs, Managerial Ability, and Product Market HHI

	Ln (Total Gap)					
	Enforceability Up			Enforceability Down		
	1	2	3	4	5	6
NCA Enforceability	0.167***	0.163***	0.142**	-0.046	-0.045	-0.075
	(0.046)	(0.052)	(0.054)	(0.036)	(0.040)	(0.045)
Young VPs	0.029			0.028		
	(0.020)			(0.019)		
NCA Enforceability × Young VPs	0.048**			-0.085		
	(0.023)			(0.076)		
High Ability		0.043*			0.046***	
		(0.024)			(0.015)	
NCA Enforceability × High Ability		0.062**			-0.078	
		(0.027)			(0.071)	
Low HHI			-0.027			-0.023
			(0.021)			(0.021)
NCA Enforceability × Low HHI			0.095**			-0.062
			(0.037)			(0.050)
Observations	83,598	82,176	81,735	74,487	73,226	72,804
R-squared	0.634	0.631	0.634	0.645	0.642	0.646
Controls from Table 2	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

TABLE 6

Mechanisms Conditional on RPE

This table reports the effect of a state-level change in the enforceability of non-compete agreements (NCAs) on the executive pay gap. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) for the period 1993–2018. The coefficients are estimated using the stacked-regression method by Baker et al. (2022) for a test window of $t \pm 10$ (t is the event year), using firms headquartered in never treated states as "clean" controls. In Panel A, the regressions in Column 2 of Panels A and B in Table 2 are re-estimated by adding interaction terms for the test variable *NCA Enforceability Up (Down)*. *NCA Enforceability Up (Down)* is interacted with the measure of relative performance evaluation (RPE). *RPE* equals one if the firm includes performance-vesting provisions in executive compensation according to ISS Incentive Lab. Due to data availability, the test sample is limited to the period from 1998 to 2018. The dependent variable is *Ln (Total Gap)*, measured in year $t + 1$. In Panel B, the regressions in Columns 1 and 4 of Panels A and B in Table 4 are re-estimated by adding interaction terms for the test variable. The dependent variables are *Ln (CEO Pay)* and *Ln (VP Pay)*, measured in year $t + 1$. *NCA Enforceability Up (Down)* is interacted with *Systematic Performance* and *Unsystematic Performance*, which are the systematic and unsystematic components of firm stock returns, respectively, following Albuquerque (2009) and Jenter and Kanaan (2015). *NCA Enforceability Up (Down)* is measured in year t and is a binary variable that is set to one if a firm is headquartered in a state that has experienced an increase (decrease) in NCA enforceability over the current or previous years. All models include firm-cohort, year-cohort, and headquarter-state-cohort fixed effects. Detailed variable definitions can be found in the Appendix. The standard errors in parentheses are clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to significance levels of 1%, 5%, and 10%, respectively.

Table 6 (Continued)

Panel A: Explicit Measurement of RPE

	Ln (Total Gap)	
	Enforceability Up	Enforceability Down
	1	2
NCA Enforceability	0.248***	0.032
	(0.061)	(0.044)
RPE	0.003	0.009
	(0.045)	(0.044)
NCA Enforceability × RPE	-0.106	-0.142
	(0.065)	(0.087)
Observations	43,243	38,259
R-squared	0.519	0.539
Controls from Table 2	Yes	Yes
Firm-Cohort FE	Yes	Yes
Year-Cohort FE	Yes	Yes
State-Cohort FE	Yes	Yes

Panel B: Implicit Measurement of RPE

	Enforceability Up		Enforceability Down	
	Ln (CEO Pay)	Ln (VP Pay)	Ln (CEO Pay)	Ln (VP Pay)
	1	2	3	4
NCA Enforceability	0.146***	0.087***	-0.045	0.013
	(0.034)	(0.030)	(0.034)	(0.026)
Systematic Performance	0.118***	0.086***	0.156***	0.116***
	(0.030)	(0.018)	(0.028)	(0.015)
NCA Enforceability × Systematic Performance	-0.005	0.029	0.023	0.049
	(0.056)	(0.035)	(0.037)	(0.035)
Unsystematic Performance	0.141***	0.112***	0.146***	0.116***
	(0.008)	(0.008)	(0.009)	(0.008)
NCA Enforceability × Unsystematic Performance	0.018	-0.024	-0.031	-0.005
	(0.027)	(0.024)	(0.030)	(0.020)
Observations	81,897	81,897	73,730	73,730
R-squared	0.757	0.805	0.765	0.812
Controls from Table 4	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes

TABLE 7

Alternative Mechanism: Retention

This table reports the effect of a state-level change in the enforceability of non-compete agreements (NCAs) on the executive pay gap. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) for the period 1993–2018. The coefficients are estimated using the stacked-regression method by Baker et al. (2022) for a test window of $t \pm 10$ (t is the event year), using firms headquartered in never treated states as "clean" controls. The regressions in Column 2 of Panels A and B in Table 2 are re-estimated by adding interaction terms for the test variable *NCA Enforceability Up (Down)*. *NCA Enforceability Up (Down)* is interacted with measures of the degree of litigation in an industry, market to book ratios, and skilled worker reliance (Qiu and Wang (2021)). *Litigation Industry* equals one if the firm is in one of the following industries: pharmaceutical/biotechnology (SIC codes 2833–2826, 8731–8734), computer (3570–3577, 7370–7374), electronics (3600–3674), or retail (5200–5961) (Cheng and Warfield (2005)); *High Mkt to Book* equals one if the *Mkt to Book Ratio* is above the sample annual median in the same industry; *Low Skill Reliance* equals one if the skilled labor risk is below the annual median. The dependent variable is *Ln (Total Gap)*, measured in year $t + 1$. *NCA Enforceability Up (Down)* is measured in year t and is a binary variable that is set to one if a firm is headquartered in a state that has experienced an increase (decrease) in NCA enforceability over the current or previous years. All models include firm-cohort, year-cohort, and headquarter-state-cohort fixed effects. Detailed variable definitions can be found in the Appendix. The standard errors in parentheses are clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to significance levels of 1%, 5%, and 10%, respectively.

Table 7 (Continued)

	Ln (Total Gap)					
	Enforceability Up			Enforceability Down		
	1	2	3	4	5	6
NCA Enforceability	0.205***	0.186***	0.214***	-0.069	-0.079*	-0.124
	(0.057)	(0.054)	(0.072)	(0.043)	(0.044)	(0.090)
NCA Enforceability × Litigation Industry	-0.062			-0.068		
	(0.124)			(0.096)		
High Mkt to Book		0.232***			0.212***	
		(0.029)			(0.032)	
NCA Enforceability × High Mkt to Book		0.003			-0.038	
		(0.033)			(0.030)	
Low Skill Reliance			0.037			0.018
			(0.036)			(0.027)
NCA Enforceability × Low Skill Reliance			-0.017			-0.050
			(0.032)			(0.139)
Observations	83,849	82,989	64,648	74,710	73,933	56,336
R-squared	0.634	0.638	0.644	0.645	0.650	0.659
Controls from Table 2	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

TABLE 8

Firm Value Implications

This table documents the firm value implications of a state-level increase in the enforceability of non-compete agreements (NCAs) conditional on the change in the executive pay gap. Panel A (B) considers the change in the pay gap from $t-1$ to t ($t+1$). Observations for all treated firms are retained starting from the year following the increase in NCA enforceability to build portfolios of treated stocks. In each monthly portfolio, we take a long (short) position in the subsample with increases (non-increases) in the pay gap. The monthly portfolio returns are computed as equally weighted (Column 1) or market-capitalization-weighted (market capitalization measured at the end of the previous month) (Column 2) average monthly stock returns. The reported alphas are from time-series regressions over several sample periods of portfolio returns on the Fama-French five factors. The standard errors in parentheses are computed using the Newey-West estimators for 4 lags. ***, **, * correspond to significance levels of 1%, 5%, and 10%, respectively.

Panel A: Pay Gap Change from Year $t-1$ to Year t

	Equally Weighted Portfolio	Weighted Average Portfolio
	1	2
Alpha for holding 12 months	1.861%*	1.334%*
	(0.010)	(0.007)
Alpha for holding 24 months	1.128%**	1.246%**
	(0.006)	(0.005)
Alpha for holding 36 months	1.178%***	1.191%***
	(0.004)	(0.004)
Observations	264	

Panel B: Pay Gap Change from Year $t-1$ to Year $t+1$

	Equally Weighted Portfolio	Weighted Average Portfolio
	1	2
Alpha for holding 12 months	0.694%	1.651%*
	(0.007)	(0.009)
Alpha for holding 24 months	1.093%**	1.254%*
	(0.005)	(0.006)
Alpha for holding 36 months	1.199%***	1.254%***
	(0.004)	(0.004)
Observations	252	

Appendix: Variable Definitions

Variable	Definition
Compensation Variables	
Total Gap	The difference between the CEO's total compensation and the average compensation of the VPs. To mitigate the data inconsistency caused by the introduction of FAS 123R, total compensation data is adjusted following Coles et al. (2013) and Kini and Williams (2012). (*Source*: ExecuComp)
ST Gap	The difference between the CEO's short-term (ST) compensation and the average ST compensation of the VPs. ST compensation is the sum of salary and bonus. (*Source*: ExecuComp)
LT Gap	The difference between the CEO's long-term (LT) compensation and the average LT compensation of the VPs. Before the FAS 123R, LT compensation is the total value of the long-term incentive plan, restricted stock granted, and options granted. After the FAS 123R, LT compensation is the total value of the non-equity incentive pay, stocks granted, and estimated value of options granted (we hypothesize that the cash payment of formulaic multiyear plans in the non-equity incentive pay is non-negligible). (*Source*: ExecuComp)
Median Industry Gap	The median *Total Gap* for firms in the same Fama-French 30 industry, year, and quartile of total assets (excluding the focal firm). (*Source*: ExecuComp)
Median Industry ST Gap	The median *ST Gap* for firms in the same Fama-French 30 industry, year, and quartile of total assets (excluding the focal firm). (*Source*: ExecuComp)
Median Industry LT Gap	The median *LT Gap* for firms in the same Fama-French 30 industry, year, and quartile of total assets (excluding the focal firm). (*Source*: ExecuComp)
Ln (Total Gap)	Natural logarithm of *Total Gap*.
Ln (ST Gap)	Natural logarithm of *ST Gap*.
Ln (LT Gap)	Natural logarithm of *LT Gap*.
Ln (Median Industry Gap)	Natural logarithm of *Median Industry Gap*.
Ln (Median Industry ST Gap)	Natural logarithm of *Median Industry ST Gap*.
Ln (Median Industry LT Gap)	Natural logarithm of *Median Industry LT Gap*.
Ln (CEO Pay)	Natural logarithm of the total compensation of the CEO. (*Source*: ExecuComp)
Ln (CEO ST Pay)	Natural logarithm of the ST compensation of the CEO. ST compensation is the sum of salary and bonus. (*Source*: ExecuComp)
Ln (CEO LT Pay)	Natural logarithm of the LT compensation of the CEO. Before the FAS 123R, LT compensation is the total value of the long-term incentive plan, restricted stock granted, and options granted. After the FAS 123R, LT compensation is the total value of the non-equity incentive pay, stocks granted, and estimated value of options granted. (*Source*: ExecuComp)
Ln (VP Pay)	Natural logarithm of the average total compensation of the VPs. (*Source*: ExecuComp)
Ln (VP ST Pay)	Natural logarithm of the average ST compensation of the VPs. ST compensation is the sum of salary and bonus. (*Source*: ExecuComp)
Ln (VP LT Pay)	Natural logarithm of the average LT compensation of the VPs. Before the FAS 123R, LT compensation is the total value of the long-term incentive plan, restricted stock granted, and options granted. After the FAS 123R, LT compensation is the total value of the non-equity incentive pay, stocks granted, and estimated value of options granted. (*Source*: ExecuComp)
Ln (Median Industry CEO Pay)	Natural logarithm of *Median Industry CEO Pay*, which is the median *CEO Pay* for firms in the same Fama-French 30 industry, year, and quartile of total assets (excluding the focal firm). (*Source*: ExecuComp)

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Variable	Definition
Ln (Median Industry CEO ST Pay)	Natural logarithm of *Median Industry CEO ST Pay*, which is the median *CEO ST Pay* for firms in the same Fama-French 30 industry, year, and quartile of total assets (excluding the focal firm). (*Source*: ExecuComp)
Ln (Median Industry CEO LT Pay)	Natural logarithm of *Median Industry CEO LT Pay*, which is the median *CEO LT Pay* for firms in the same Fama-French 30 industry, year, and quartile of total assets (excluding the focal firm). (*Source*: ExecuComp)
Ln (Median Industry VP Pay)	Natural logarithm of *Median Industry VP Pay*, which is the median *VP Pay* for firms in the same Fama-French 30 industry, year, and quartile of total assets (excluding the focal firm). (*Source*: ExecuComp)
Ln (Median Industry VP ST Pay)	Natural logarithm of *Median Industry VP ST Pay*, which is the median *VP ST Pay* for firms in the same Fama-French 30 industry, year, and quartile of total assets (excluding the focal firm). (*Source*: ExecuComp)
Ln (Median Industry VP LT Pay)	Natural logarithm of *Median Industry VP LT Pay*, which is the median *VP LT Pay* for firms in the same Fama-French 30 industry, year, and quartile of total assets (excluding the focal firm). (*Source*: ExecuComp)
Executive Characteristics	
CEO Age	Age of CEO. (*Source*: ExecuComp)
CEO Tenure	Number of years as CEO. (*Source*: ExecuComp, BoardEx)
CEO Is Insider	Dummy that equals one if the CEO has worked in the firm for at least 12 months prior to becoming CEO, zero otherwise. (*Source*: ExecuComp, BoardEx)
CFO Is VP	Dummy that equals one if one of the VPs is the CFO, zero otherwise. An executive is identified to be a CFO according to the Execucomp variable "CFO annual flag" (CFOANN = CFO). Considering that the variable often has missing values prior to 2007, we follow Kubick and Masli (2016) and classify an executive as CFO if the executive's job title includes any variants of "CFO," "financial," "finance," "treasurer," or "controller." (*Source*: ExecuComp)
Duality	Dummy that equals one if the CEO is also chairman in the same firm, zero otherwise. (*Source*: ExecuComp)
Ln (CEO Age)	Natural logarithm of *CEO Age*.
Ln (CEO Tenure)	Natural logarithm of *CEO Tenure*.
New CEO	Dummy that equals one if the CEO is in her first year in the role, zero otherwise. (*Source*: ExecuComp)
No. of VPs	Number of VPs. (*Source*: ExecuComp)
Propensity of Relay Succession	Dummy that equals one if a president or COO is distinct from the CEO or chair, zero otherwise, following Naveen (2006). (*Source*: ExecuComp)
Retiring CEO	Dummy that equals one if *CEO Age* is 65 or more, zero otherwise. (*Source*: ExecuComp)
Firm/Industry Characteristics	
Industry Homogeneity	Mean partial correlations between the returns on firms from a particular industry and the return on an equally weighted index constructed by using all the firms from the same industry, holding the market return constant (Parrino (1997)). Estimated based on 36 monthly returns. Industry is defined as the Fama-French 30 industry. (*Source*: CRSP)
Ln (Total Asset)	Natural logarithm of total assets. (*Source*: Compustat)
Stk. Return Volatility	Volatility of past 36 monthly returns. (*Source*: CRSP)
No. of Segments	Number of business segments in which a firm operates. (*Source*: Compustat)
Cash Ratio	Cash over total assets. (*Source*: Compustat)
Dividend Payer	Dummy that equals one if the firm has paid dividends in a year, zero otherwise. (*Source*: Compustat)

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Variable	Definition
ROA	Net income over total assets. (*Source*: Compustat)
Leverage	Long-term debt plus debt in current liabilities, scaled by total assets. (*Source*: Compustat)
Mkt to Book Ratio	Total assets minus the book value of equity plus the market value of equity, scaled by total assets. Book value of equity is book common equity minus preferred stock and plus deferred taxes. (*Source*: Compustat)
RPE	Dummy that equals one if the firm includes performance-vesting provisions in executive compensation in a year, zero otherwise. (*Source*: ISS Incentive Lab)
Systematic Performance	The predicted value of regressing firm stock return on peer stock return, which is measured as the equal-weighted portfolio return of firms in the same industry and size quartile (excluding the focal firm). (*Source*: CRSP, Compustat)
Unsystematic Performance	The residual of regressing firm stock return on peer stock return, which is measured as the equal-weighted portfolio return of firms in the same industry and size quartile (excluding the focal firm). (*Source*: CRSP, Compustat)
Conditional Variables	
Young VPs	Dummy that equals one if the average age of VPs is below the sample annual median, zero otherwise. (*Source*: ExecuComp, BoardEx, 10-K files, and company's official websites)
High Ability	Dummy that equals one if managerial ability is above the sample annual median in a same industry, zero otherwise. Managerial ability data is from Demerjian et al. (2012).
Low HHI	Dummy that equals one if the HHI is below the annual median in a same industry, zero otherwise. Product market HHI data is from Hoberg and Phillips (2016).
Litigation Industry	Dummy that equals one if the firm is in the following industries: pharmaceutical/biotechnology (SIC codes 2833–2826, 8731–8734), computer (3570–3577, 7370–7374), electronics (3600–3674), or retail (5200–5961), zero otherwise, following Cheng and Warfield (2005). (Source: Compustat)
High Mkt to Book	Dummy that equals one if the *Mkt to Book Ratio* is above the sample annual median in a same industry, zero otherwise.
Low Skill Reliance	Dummy that equals one if the skilled labor risk is below the annual median, zero otherwise. Skilled worker risk data is from Qiu and Wang (2021).

Online Appendix to

"Outside Employment Opportunities and Tournament Incentives"

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March 2025

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OA.A Discussions about Garmaise's (2011) Methodology

1 Questions and Thresholds

Garmaise (2011) considers twelve questions analyzed by Malsberger (1996) to evaluate the change of non-compete status in each jurisdiction. The following are Garmaise's questions and respective thresholds. When relevant events, including court cases or legislative actions, cause the level of the state-level NCA enforcement to move above or below the threshold, we conclude that NCA enforceability increases or decreases accordingly.

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

Threshold 1. States that enforce non-competition agreements outside a sale-of-business context are above the threshold.

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, are above the threshold.

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.

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 state-wide restrictions have been upheld are above the threshold.

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 narrower 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 is above the threshold. States that explicitly exclude consideration of the reasonableness of the contract from the calculation of damages are also above the threshold.

2 Events that Led to Variations in NCA Enforceability

The following table summarizes the court cases or legislative actions that led to variations in NCA enforceability during our study period, 1993 to 2018. The events are identified based on the thresholds and questions by Garmaise (2011). Specific details are in Section OA.A.1.

Note that we code that Illinois experienced decreased enforceability in 2013 but do not follow Ewens and Marx (2018), who label Illinois as a state with increased enforceability in 2011. As mentioned in Ewens and Marx (2018), in *Fire Equipment v. Arredondo et al.*, the Supreme Court of Illinois altered its definition of "protectable legitimate business interests" in deciding on NCA enforcement in 2011. Previously, Illinois had recognized fixed conditions to identify such

Events that led to the increase in NCA enforceability		
Florida	1996	Florida Legislature
Georgia	2004	*Bellsouth Corporation v. Forsee*
Ohio	2004	*Lake Land v. Columber*
Vermont	2005	*Summits 7 v. Kelly*
Idaho	2008	Idaho Legislature
Wisconsin	2009	*Star Direct v. Dal Pra*
Georgia	2010	Georgia Legislature
Colorado	2011	*Lucht's Concrete Pumping, Inc. v. Horner*
Texas	2011	*Marsh v. Cook*
Virginia	2013	*Assurance Data Inc. v. Malyevac*

Events that led to the decrease in NCA enforceability		
Texas	1994	*Light v. Centel Cellular Co.*
Louisiana	2001	*Shreveport Bossier, Inc. v. Bond* (Undone in 2003)
Oregon	2008	Oregon Legislature
New Hampshire	2012	New Hampshire Legislature
Illinois	2013	*Fifield v. Premier Dealership Servs.*
Kentucky	2014	*Creech v. Brown*

business interests (for instance, *Outsource Int'l, Inc. v. Barton* 1999), but starting in 2011 the restrictions were relaxed through the introduction of more flexible standards. The Court also said it "expressly observe(s) that appellate court precedent for the past three decades remains intact."

As indicated by Garmaise's 12 questions, the second question to evaluate the change of NCA enforceability is "What is an employer's protectable interest and how is it defined?" States that meet the threshold should be "states in which the employer can prevent the employee from future independent dealings with all the firm's customers." In Illinois, employers could already do this before 2011. After the 2011 Supreme Court ruling, the practical enforceability in Illinois exceeded the threshold even more. Importantly, although we argue that Illinois did not substantially increase its state-level enforceability in 2011, our main results are robust to the inclusion of this event. More details can be found in Section OA.H.1.

In identifying variations in NCA enforceability, we notice some other inconclusive opinions that have been raised by recent studies, although the methodologies are all consistent with

Garmaise (2011). In *Poynter Investments v. Century Builders of Piedmont* 2010, for example, the Supreme Court of South Carolina ruled against the use of "blue pencil" provisions whereby a court can soften the terms of an unenforceable non-compete, yet made it easier for firms to obtain a preliminary injunction against former employees. Given the unclear implications of the decision, we follow Ewens and Marx (2018) and treat South Carolina as a control state in the baseline models. In addition, Bai, Eldemire, and Serfling (2024) and Ewens and Marx (2018) denote that Oregon experienced decreased enforceability in 2008, while Kini, Williams, and Yin (2021) claim a change in the opposite direction from the same event. Untabulated results show that our main results are not affected by treating the non-compete status in South Carolina or Oregon as increase, decrease, or no-change.

OA.B Price Effects of NCA Enforceability Shocks

If NCA enforceability shocks indeed have substantial implications for treated firms, we would expect these shocks to generate significant stock market reactions, which could motivate firms to adjust executive pay. We next examine stock price reactions around the dates when courts decide on changes in enforceability or when NCA regulations are signed into law (event dates). The test window comprises a pre-event period (trading days [-11, -4] and [-3, -2]) and an event period (trading days [-1, 3]), with $t = 0$ representing the event date. We restrict the sample to firms headquartered in states affected by these events and calculate cumulative abnormal returns (CARs) for the test window using either the market model (equally weighted CRSP market portfolio) or the Fama-French five-factor model. Because all firms in a state experience the same event date, standard errors may be biased due to cross-sectional correlation. Kolari and Pynnönen (2010)

propose a new test statistic to address this issue and show that it performs better than other popular estimators. Thus, we use Kolari's statistics to evaluate the statistical significance of abnormal return estimates.

In Panel A of Table OA.2, we find that abnormal returns during the window from day -1 to day +3 are only significant for Enforceability Up events. The reactions to such events are positive, consistent with the prevailing notion that NCAs typically favor employers over employees. These findings are robust across two different estimation methods, and no anticipation effect is evident during the pre-event period from day -11 to day -2. However, in Panel B, no significant abnormal returns are reported for Enforceability Down events, suggesting that market participants do not expect these shocks to significantly impact firms.

OA.C Mobility Effects of NCA Enforceability Shocks

Our experiment relies on the notion that a variation in NCA enforceability generates or removes barriers to top executive mobility. Though existing studies note that executives are restrained by the prevalent use of NCAs and enhanced NCA enforceability (Garmaise (2011), Graham, Kim, and Kim (2020)), we believe it is necessary to check whether the shifts in enforcement constitute valid shocks to VP mobility in our studied sample window. To test this, we start with the firms covered in ExecuComp during 1993 to 2018. Since VPs are our primary focus, we exclude CEO information from the sample. The mobility indicator takes the value of one if the focal executive is no longer at the same firm in year $t+1$ relative to t. We do not require an executive to reappear at another sample firm, because they may move to firms outside S&P (without coverage in ExecuComp). We exclude the departures of executives above age 65, which are likely to be

caused by retirements.

Consistent with our main identification strategy, we adopt the stacked-regression method of Baker, Larcker, and Wang (2022) for a test window of $t \pm 10$, using firms headquartered in never-treated states as "clean" controls. In our sample, around 15% of VPs terminate their role at the focal firm in the following year. We use a linear probability model to gauge the effects of increases or decreases in NCA enforceability on executive turnover in separate samples. In Table OA.3, we find that changes in NCA enforceability affect corporate executives' mobility in different ways. Stronger enforceability decreases mobility by about 32.6% ($= -0.049/0.150$); thus, executives' ability to join external tournaments is indeed affected by the upward enforceability shocks. However, we do not find statistically significant effects of weaker enforcement on the mobility of VPs working for treated firms.

OA.D Multiple Hypothesis Testing Problem

The many studies involving NCA enforceability shocks have shown that the shocks affect multiple outcomes, which leads to a multiple hypothesis testing problem. Heath, Ringgenberg, Samadi, and Werner (2023) highlight that such a problem requires corrections that make statistically significant estimates harder to obtain. They suggest evaluating results using appropriately corrected standard errors, as reported in their Table AI (Panel B for staggered introductions). In Panel A of Table 2, the statistical significance of our baseline estimates for Up events in Columns 1 and 2 (t-values = 3.88 and 3.75) passes even the most stringent adjusted critical values suggested by Heath et al. (2023), which are based on an overall number of potential outcomes as large as 293 (where the t-value would be 3.69). In Panel B, the results for Down events in Columns 1 and

2 (t-values = 2.13 and 1.89) fail to meet these thresholds. These results are again consistent with the asymmetric findings we report throughout the paper. Overall, we do not view the multiple hypothesis testing problem as a threat to our inferences.

OA.E Changes to Baseline Specifications

1 Alternative Proxies

We follow prior research and use several alternative measures as proxies for the executive pay gap: pay gap as the difference between the total pay of the CEO and the total pay of the median (*Ln (Total Gap Median VP*) or highest-paid VP in a firm-year (*Ln (Total Gap Max VP*); Gini coefficient as the income dispersion among the whole top executive team (*Gini Coefficient*); and relative annual rank of total gap (*Rank (Total Gap)*).

In the main analysis, we follow the approach by Coles, Daniel, and Naveen (2013) and Kini and Williams (2012) to adjust the post-FAS 123R compensation data and get the proxy of *Ln (Total Gap)*. Here, we replicate the procedures by Walker (2011) and Gabaix, Landier, and Sauvagnat (2014) to adjust the pre-FAS 123R period data and build an adjusted version of the executive pay gap (*Ln (Total Gap) Adj.*). Specifically, we first subtract the amount paid for long-term incentive plans (ExecuComp item: LTIP) from TDC1 and add the product of the number of performance shares granted (ExecuComp item: SHRTARG) and the stock price at the fiscal year-end. SHRTARG is replaced by zero if missing (Gabaix et al. (2014)). We use the TDC1 data as-is for the period after FAS 123R. The proxies of *Median Industry Gap* are all re-calculated in line with their respective dependent variables. Table OA.4 indicates that our main finding does not differ when we use the alternative measures of the executive pay gap in the Enforceability

Up sample. The estimations on *NCA Enforceability Down* remain only marginally significant or insignificant when the alternative measures are used.

2 Alternative Specifications

In Columns 1 and 2 of Table OA.5, we restrict the window to $t \pm 3$ or $t \pm 5$ around the event year t for the treated companies. Compared to the window of $t \pm 10$ used in the main analysis, a shorter test window is helpful in further excluding confounding factors correlated with shifts in NCA enforcement. In Column 3, we change to using "not yet treated" firms as clean comparisons. Unlike the "never treated" group, the "not yet treated" group includes observations for firms that will be treated in the future. We report the results of the regressions estimated in the generalized DID and with the Callaway and Sant'Anna (2021) estimator in Columns 4 and 5, respectively. The new results are consistent with the baseline findings, reaffirming the asymmetric pattern shown in our main results.

OA.F Retention Considerations and Executive Pay Levels

In Section V.C, we argue that retention considerations by firms may counteract the tournament incentives observed in our findings. Inconsistent with this argument, we show that variables reflecting retention costs do not help us explain changes in executive pay gaps following NCA enforceability shocks. However, given that pay levels affect the likelihood of executive retention, it remains unclear whether revisions in executive pay levels following enforceability shocks are consistent with a retention mechanism driving executive pay gap adjustments. We next conduct tests to examine CEO and VP pay separately to address these questions.

Specifically, we re-examine the set of conditional variables used in the main analysis that

capture variations in retention costs. These variables include *Fraction of Insider CEOs*, *Skewness of CEO Pay*, *Young VPs*, *High Ability*, *Low HHI* (Section V.A), *Litigation Industry*, *High Mkt to Book*, and *Low Skill Reliance* (Section V.C). In Table OA.6, we report how retention costs drive the effects of an increase (Panels A and B) and decrease in enforceability (Panels C and D) on CEO and VP pay levels. In Panels A and B, we find that firms with higher retention costs for executives increase their pay more in only 6 out of 16 cases when enforceability increases. In Panels C and D, we show that firms increase VP pay levels more when VPs are more able or willing to move to another firm in only 1 case.[1] For the remaining tests across all 4 Panels, CEOs and VPs in firms facing high retention costs experience either a smaller increase (2 cases out of 32 in total) or a similar change (23 cases out of 32) in pay levels when NCA enforceability changes. Overall, these results indicate limited evidence in support of the retention mechanism.

OA.G Does the Executive Pay Gap Capture Tournament Incentives?

In the main body of the paper, we assumed that the executive pay gap creates a tournament prize that is meant to incentivize and reward VPs and possibly compensate them for lost outside employment opportunities following the Up shocks. Nonetheless, other theories or explanations have been put forth for the existence of a pay gap. Below, we provide some evidence that supports our contention that the pay gap likely captures tournament incentives in our context. We only focus on the Enforceability Up sample in this part, as the Enforceability Down results remain largely insignificant throughout.

[1] We note that in Columns 7 and 8 of Panel C in Table OA.6, *NCA Enforceability Down* has a positive impact on VP pay for firms in the top tercile of CEO pay skewness. However, this difference is not statistically significant when compared to the bottom tercile.

1 CEO Power

Shocks that impose mobility restrictions likely hinder the efficiency of talent allocation (e.g., Anand, Hasan, Sharma, and Wang (2018), He and Wintoki (2023)) and shrink the talent pool while increasing firms' challenges in replacing incumbent CEOs. Powerful CEOs can exploit such shocks to boost their pay, widening the pay gap (Bebchuk, Cremers, and Peyer (2011)). If CEO power plays a prominent role in the pay gap, then CEOs should merely set a higher pay for themselves and hardly compensate other executives. In this case, we expect to see an insignificant or even negative impact on the level of average VP compensation following a shock that restrains mobility. In Section IV.C, however, we show that both CEOs and VPs receive higher compensation after such shocks, which is at odds with the CEO power prediction. We next report more-direct tests that help us evaluate whether a CEO power mechanism can explain our findings.

Different Proxies CEO pay slice is an indicator broadly used by existing studies to capture managerial power via executive compensation (Bebchuk et al. (2011)). We compute the pay slice as the ratio of the CEO pay scaled by the aggregated pay of the five highest-paid executives. In our sample, the correlation between the pay slice and the total pay gap among the top five executives is 0.56, which appears to be lower than expected.[2] We use the pay slice as the dependent variable, re-

[2]Assume the average VP's pay is P and the CEO's pay is αP ($\alpha > 1, P > 0$) at time 0. The one-period increase rates in their compensations are δ and $\beta\delta$, respectively. Thus, at time 1, an average VP's pay is $(1+\delta)P$ and the CEO pay is $(1+\beta\delta)\alpha P$. It is straightforward to show that the measures of pay gap and pay slice do not always synchronously change, but depend on the specific relations among δ, α, and β. For example, if $\delta > 0$ and $\frac{1}{\alpha} < \beta < 1$, the pay gap increases but the pay slice decreases. This may be one reason why the correlation between the two proxies is relatively low.

run the baseline regressions, and report the results in Columns 1 and 2 of Panel A, Table OA.7. The coefficient on *NCA Enforceability Up* is not significant once controls are included. This suggests that CEOs do not manage to increase their pay slice when facing NCA shocks.

We further regress our pay gap measure on a set of variables directly associated with CEO power and build a dependent variable that captures the residuals from this regression. After purging the components related to CEO power, the residual is likely to be the part of the pay gap that is set through efficient contracting. Specifically, in the first stage, similar to Hasan, Navone, To, and Wu (2020), we regress the pay gap on dummies for CEO-chair duality, highly tenured CEOs, and founder CEOs. We add industry and year FEs at this stage to capture potential shocks for different sectors and time-varying trends that may relate to CEO power. In the second stage, we use the residual from stage 1 as the dependent variable and re-estimate the baseline specifications. In Columns 3 and 4, we continue to find a significant positive impact of increased NCA enforceability on the pay gap.

Cross-sectional Variation We focus on one potential cross-sectional variation in terms of CEO power. Bertrand and Mullainathan (2003) show that CEO power relates to institutional factors, such as deterrents to takeovers. We use the takeover index developed by Cain, McKeon, and Solomon (2017) for the presence and extent of hostile takeovers. A larger value of the variable is potentially associated with less entrenched CEOs due to high market discipline. *Low Takeover Index* equals one if the takeover index is below the annual sample median in a state. The interaction between *Low Takeover Index* and the enforceability test variable has a negative and statistically significant coefficient in Panel B of Table OA.7. Thus, when CEOs are likely to be more powerful and entrenched, the effect of an NCA shock on the pay gap becomes weaker, a result inconsistent

with the explanation that powerful CEOs extract excessive compensation.

2 Talent Differentials

The dispersion among executive pay can also be set to reflect talent differentials (Mueller, Ouimet, and Simintzi (2017), Terviö (2008)). Consequently, a larger pay gap may simply capture CEO talent or skill hierarchies rather than being set to incentivize executives.

However, assuming the composition of the top management team remains unchanged, the observed post-shock variation in compensation theoretically should not reflect time-varying talent differentials for two reasons. First, since the NCA shocks are sudden and exogenous, it is unlikely that the incumbent management team will be able to acquire new skills quickly following the reforms, which could affect the distribution of talent across executives. Second, studies suggest that firms perform worse when experiencing strengthened enforceability (Anand et al. (2018), He and Wintoki (2023)). If managerial skills are evaluated based on firm performance and if talent differentials drive pay gaps, we should probably not observe a post-shock increase in pay gap, given that the CEO is ultimately responsible for poor performance. To validate these conjectures, we follow the previous literature and consider three time-varying ability proxies: a managerial ability score, adjusted ROA, and adjusted stock return (Demerjian, Lev, and McVay (2012), Rajgopal, Shevlin, and Zamora (2006)). ROA and stock return are adjusted by subtracting the median value in each industry-year. The three extra variables are significantly associated with the pay gap measure, but our main results are not affected by their inclusion in Panel A of Table OA.8.

The composition of top executives, however, often changes in practice, and this can drive variations in the pay gap. For example, replacing a particularly talented CEO with an average-ability one could lead to a reduced pay gap. To capture the talent differences between an individual

VP and a CEO, we consider executive FEs to best describe the executives' latent traits and skills (Graham, Li, and Qiu (2012)). Specifically, we construct a pay gap proxy at the VP level instead of the firm level. That is, we compute the compensation gap between each focal VP and her CEO in the focal firm. The large and rich VP-year sample provides us with the flexibility to incorporate either CEO-VP pair FEs or CEO-VP-firm FEs. The former allow us to control for the time-invariant difference within each CEO-VP pair, while the latter reflect the fact that such a difference could vary across firms. In Panel B, we include these FEs and re-estimate our baseline regression models using the new dependent variable measured at the VP level. The R-squared across different models is high (77%). We observe that the positive impact of increased NCA enforceability is still positive and significant.

3 CEO Bargaining Power

Kini et al. (2021) note that the presence of an NCA is the outcome of a bargaining game between the firm and the executive. The firm bargains to include the provision and protect its economic interests upon the possible departure of the executive, and the executive argues for more considerations to compensate them for the incurred personal costs. As a result, executives can obtain higher compensation (Kini et al. (2021)). We indeed find that the effects of the NCA shocks on the pay gap are more pronounced when the top management members have more ex ante opportunities on the external job market. This finding lends itself to an alternative explanation: If NCAs become more enforceable, executives (especially those with significant bargaining power) will accept them only in return for substantially increased pay. If the disutility attached to an NCA or the bargaining power is larger for CEOs than for VPs, this bargaining mechanism could lead to a post-shock widening of the pay gap that is unrelated to tournament incentives.

We consider a set of variables that are associated with a CEO's outside job opportunities or bargaining power and that are plausibly unrelated to the VPs'. If the bargaining activities of the CEO primarily drive the pay gap changes we report, we expect these variables to drive substantial heterogeneity in our main results.

First, a retiring CEO is less sensitive to the loss of future career opportunities because her career concerns are lower (Gibbons and Murphy (1992)). Second, a generalist CEO with rich and diverse experiences should have more valuable future opportunities because her skills are more transferrable beyond certain firms or industries (Custódio, Ferreira, and Matos (2013)). A CEO is defined as a generalist if the value of the generalist index is higher than the sample annual median.[3] Third, a more productive CEO likely has stronger bargaining power. We construct a CEO productivity factor by performing a principal component analysis based on the following variables: CEO duality dummy, tenure, age, number of outside directorships, and average compensation growth over the prior three years, in line with Masulis and Zhang (2014). We retain the two orthogonal factors with eigenvalues greater than one. A CEO is productive if both factors are above their respective sample medians.

In addition, assuming a CEO often departs to take the same job at another firm that offers better compensation, the gap between her pay and that of the top-paid CEO in a given year is helpful in capturing the focal CEO's external tournament incentives. Following Coles, Li, and Wang (2018), we calculate the pay differential between the focal CEO and the second-highest-paid CEO in a specific industry. We do this because the highest compensation is often caused by transitory events, such as the exercise of accumulated options. To account for labor market

[3]We thank Cláudia Custódio for sharing the data of CEO generalist index.

segmentation, we also calculate a local pay gap, that is, a proxy for the focal CEO and the second-highest-paid CEO in a specific firm's headquarters state (Ma, Pan, and Stubben (2020)). The concept is further generalized by computing the pay gap between the focal CEO and the universal second-highest-paid CEO in each sample year. The CEOs whose pay gaps are higher than the respective sample medians are those with more opportunities for external promotion.

Last, synergy theory posits that a CEO's activities covering a broad range of firm business have enormous synergy potential to reduce other colleagues' marginal efforts (Edmans, Goldstein, and Zhu (2013)). Because only a CEO can have such potential, she may have stronger power to bargain for higher pay than subordinate executives. We identify the less synergistic firms as those with a diversified business (i.e., more than one business segment) where the different divisions likely operate independently (Edmans et al. (2013)). CEOs could have more synergy impacts in non-diversified firms, and also larger bargaining power.

In Table OA.9, we find that the effects of increased NCA enforceability on the pay gap are virtually always statistically significant, consistent with prior findings. However, the effects become significantly weaker when firms are non-diversified (Shi, Connelly, and Sanders (2016)), which runs counter to the notion that CEOs get compensated for their larger bargaining power due to larger synergy potentials. No other proxies for bargaining power lead to significant differences. Overall, these findings contradict the argument that the expanded pay gaps we observe are driven by CEOs' bargaining power.

OA.H Other Robustness Checks

We present a series of tests that establish the robustness of our inference, particularly for the non-compete shocks that increase the enforceability. All tests are informed by the extant literature. First, our results are not sensitive to using different coding schemes to capture shifts in enforceability, to the choice of sample window, or to potential expectations of future legislative actions driven by corporate lobbying activities. Second, the relevance of the internal horse race varies over a CEO's tenure in that a new CEO is less likely to be dislodged (Coles et al. (2018)). We test whether—and verify that—the effects of enforceability on the pay gap, in general, get stronger after excluding the years just after CEO turnover events. Furthermore, we show that our results are still significant after accounting for a set of extra control variables, including tournament possibility, compensation structure, firm strategy, and several accounting variables. Fourth, we control for additional FEs to alleviate concerns about other sources of omitted variables that may relate to the pay gap. Specifically, we consider FEs that capture the time-varying heterogeneity across industries, firm size quartiles, census tract regions, and states of incorporation; FEs for firm age; and CEO-firm FEs. Our results hold. Fifth, and last, our results are robust if we make adjustments to the regression sample.

1 Different Coding Schemes

In Table OA.10, we alter the coding schemes for enhanced NCA enforceability in four ways.

First, as discussed in OA.A.2, in our baseline tests, we do not treat Illinois as a treated state in 2011 because we argue that, according to Garmaise's 12 questions, the relevant court case

does not move state-level enforceability above the threshold and substantially increase it. Here, we modify the coding by labeling Illinois as a state that did enhance its enforcement in 2011. After 2013, the enforceability again decreases, so we exclude the sample after this event. In Column 1, our results persist with this coding modification.

Second, in our baseline model we focus on the first occurrence of variations in non-compete status, but this approach does not capture the fact that Georgia further increases its enforceability in 2010. Therefore, we split our sample period into two parts to study both of the shocks affecting Georgia. We select 2006 as the dividing point so that the lengths of the two subsamples are balanced. Considering that 2006 is also the year that changed the reporting formats of executive compensation, we do not arbitrarily divide the sample according to the time variable but divide it according to the data reporting format (a small number of ExecuComp firms report in the old format in 2006). A side benefit to this is that we can use the raw data provided in ExecuComp to estimate the models separately and avoid the potential biases due to adjustments to the original compensation data. For the more recent subsample, we classify the firms headquartered in Georgia after 2010 as treated firms. In Columns 2 and 3 the effect of NCA enforceability is still significantly positive in the two windows.

Third, in our studied period, legislative actions caused two states, Florida and Idaho, to change their NCA enforceability. These occurrences may be associated with state-level political conditions or lobbying activities. Here, we only retain the treated states where the enhanced NCA enforceability is caused by court rulings, which firms are less likely to anticipate. In Column 4, we drop the observations for Florida and Idaho, and our results are not affected.

Finally, we directly use the NCA Index from Bai et al. (2024). We use Bai et al.'s version of the NCA enforcement score because their data period overlaps with ours. When, in Column 5,

we use the discrete NCA Index as the test variable, the model is not set as a DID, but the results remain qualitatively similar.

2 CEO Turnover

In Table OA.11, we verify whether the effect of enforceability on the pay gap is still present when the existing CEO departs. We remove the observations that coincide with CEO turnovers at year t (Column 1), in the two years since a turnover (Column 2), or in the five years since a turnover (Column 3). We also distinguish between the categories of CEO departures and follow Jenter and Kanaan (2015) and Peters and Wagner (2014) to identify forced CEO turnovers. Following these classifications, we remove firms that have ever fired CEOs (Column 4). The coefficients of interest are always highly significant and remain positive.

3 Omitted Variables

We discuss here some potential sources of omitted variables that may confound our results and control for sets of such variables in Table OA.12.

First, to mitigate omitted variable concerns, we include all conditional variables used in previous cross-sectional tests and rerun our baseline model. The results in Column 1 indicate that our findings remain qualitatively similar after controlling for the following variables: the continuous fraction of insider CEOs, skewness of CEO pay, VP age, managerial ability score (Demerjian et al. (2012)), product market HHI (Hoberg and Phillips (2016)), and the dummy variable for founder CEOs.

Second, the expected utility for each VP from a tournament depends on the prize size and the VP's likelihood of prevailing in the tournament (Kale, Reis, and Venkateswaran (2009)). When

outside opportunities decline for an average VP, a related concern is whether the firm inherently favors particular candidates. In a biased tournament, the promotion possibility decreases to some degree. Rational managers would always weigh the benefits and costs when deciding whether to engage in the contest, and thus force firms to expand the tournament prize to rebalance the utility. As a result, the observed change in the pay gap may be a direct response to variations in tournament opportunities for VPs instead of the non-compete regulation. Similarly, Kini et al. (2021) find that delta and vega in executive compensation are affected by the increase in NCA enforceability. We follow Kale et al. (2009) and calculate the standard deviation of VP compensation as the proxy of tournament opportunities for an average VP. We also follow Core and Guay (2002) and Coles, Daniel, and Naveen (2006) to compute delta and vega. These variables are included as controls in Columns 2, 3, and 4, and the findings for *NCA Enforceability Up* remain robust.

Third, Abernethy, Dong, Kuang, Qin, and Yang (2024) argue that the executive pay gap can also be explained by variations in a firm's time-varying strategies. According to their classification, a firm can be either a prospector or a defender. Prospectors prefer a competitive strategy that focuses on product innovation and emphasize the ability to seize new market opportunities, while defenders concentrate on existing products in the current niche industry and have less demand for quick decision-making (Abernethy et al. (2024)). Prospector-type firms need CEOs to be more authoritative and dominating and to make more timely decisions. Therefore, those firms would like to award more compensation and also larger executive pay gaps to their CEOs. This dimension could confound our findings. To account for it, we follow Abernethy et al. and construct an indicator of a firm's strategic priority, with a higher value of the measure representing a higher propensity to pursue prospector-type strategies. The measure is included in the regression of Column 5. We observe that our main findings are unaffected.

Finally, in Column 6 we control for a set of accounting variables that are frequently examined as determinants of executive pay level in existing studies e.g., Kini et al. (2021), including *ROA*, *Mkt to Book Ratio*, *Leverage*, *Cash Ratio*, and *Dividend Payer*, while in Column 7 we consider a specification in which all the additional controls mentioned above are included. Overall, we find that the explanatory power of non-compete shocks is robust after controlling for all the additional variables.

4 Additional FEs

In Table OA.13, we control for a set of high-dimensional FEs to further mitigate concerns of omitted variables. We consider the time-varying heterogeneity across industries (Column 1), firm size quartiles (Column 2), four census tract regions (Column 3), and states of incorporation (Column 4). We also add firm age fixed effects to rule out the possibility that the design of the pay gap may correlate with the firm's business cycle (Column 5). We last control for the CEO-firm FEs (Column 6). Some of the control variables in our baseline model are absorbed due to collinearity, but our variable of interest, *NCA Enforceability Up*, is significantly positive in all the cases.

5 Additional Robustness Tests

In Table OA.14, we examine a battery of additional tests in which we (i) exclude observations for the financial crisis period 2008-2009 (Column 1); (ii) exclude the sample of CEOs identified through BoardEx (Column 2); (iii) exclude relocated firms (Column 3); (iv) exclude states that have experienced changes in NCA enforceability during the period from 1980 to 1993 (Columns 4, 5, and 6); and (v) construct industry-level financials based on the SIC-2 or NAICS-3 industry classifications (Columns 7 and 8). Our estimations on *NCA Enforceability Up* are always

significant and similar to our baseline results.

OA.I Footnote Tests

In Table OA.15, we report the results of the tests mentioned in the footnotes of our manuscript. In Panel A, we restrict the sample to firms with more than one VP and find results similar to our baseline results. Panel B presents results showing that the findings are robust after adjusting for negative values of the pay gaps and including them in the regression sample. Panels C and D show the insignificant effects of changes in state-level non-compete enforcement on the promotion preferences of insider CEOs and the inclusion of explicit RPE in executive compensation contracts, respectively.

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Table OA.1: Variables Used in Online Appendix

Variable	Definition
VP Mobility	Dummy that equals one if the focal executive is not at the same firm at year $t+1$ compared to year t, zero otherwise. (*Source*: ExecuComp)
Ln (Firm Age)	Natural logarithm of the difference between the current year and the first year that the focal firm appears in Compustat. (*Source*: Compustat)
GDP Growth	The state-level annual GDP growth rate. (*Source*: Bureau of Economic Analysis)
Unemployment	The state-level annual unemployment rate. (*Source*: Bureau of Labor Statistics)
Ln (Total Gap Median VP)	Natural logarithm of the difference between the CEO's total compensation and that of the median VP. (*Source*: ExecuComp)
Ln (Total Gap Max VP)	Natural logarithm of the difference between the CEO's total compensation and the maximum total compensation among VPs. (*Source*: ExecuComp)
Gini Coefficient	$1+\frac{1}{n}-\frac{2}{n^2\bar{y}}(y_1+2y_2+...+ny_n)$, where n is the number of executives (including the CEO), $y_1...y_n$ are the executives' total compensations in decreasing order of size, and $\bar{y}$ is the average compensation. (*Source*: ExecuComp)
Rank (Total Gap)	Rank of the firm's *Total Gap* minus one for a particular year, scaled by the total number of firms minus one in the same year. (*Source*: ExecuComp)
Ln (Total Gap) Adj.	Natural logarithm of the difference between the CEO's total compensation and the average compensation of the VPs. To mitigate the data inconsistency caused by the introduction of FAS 123R, total compensation is adjusted following Walker (2011) and Gabaix et al. (2014). (*Source*: ExecuComp)
CEO Pay Slice	The total compensation of the CEO over the aggregated compensation of the top five most highly paid executives. (*Source*: ExecuComp)
Gap Residuals	The residuals from the regression of *Ln (Total Gap)* on dummies for CEO-chair duality, highly tenured CEOs, founder CEOs, and industry and year fixed effects. Highly tenured CEOs is a dummy that equals one if *CEO Tenure* is in the top quartile of the distribution in the same industry-year, zero otherwise. (*Source*: ExecuComp, BoardEx)
Ln (VP Total Gap)	Natural logarithm of the difference between the focal VP's total compensation and that of the CEO in the focal firm. (*Source*: ExecuComp)
Low Takeover Index	Dummy that equals one if the hostile takeover index is below the annual median in a same state, zero otherwise. Hostile takeover index is from Cain et al. (2017).
Generalist CEO	Dummy that equals one if the generalist score is above the sample annual median in a same industry, zero otherwise. The generalist score is from Custódio et al. (2013).
Able CEO	Dummy that equals one if the two orthogonal factors of a principal component analysis with eigenvalues greater than one are above their respective sample medians, zero otherwise. The analysis is on the following variables: CEO duality dummy, tenure, age, number of outside directorships, and the average compensation growth over prior three years, following Masulis and Zhang (2014). (*Source*: ExecuComp, BoardEx, ISS)
High CEO Industry Tourn.	Dummy that equals one if the CEO pay gap between the focal CEO and the second-top-paid CEO in a specific industry is above the sample median, zero otherwise. (*Source*: ExecuComp)
High CEO State Tourn.	Dummy that equals one if the CEO pay gap between the focal CEO and the second-top-paid CEO in a specific state is above the sample median, zero otherwise. (*Source*: ExecuComp)
High CEO Tourn.	Dummy that equals one if the CEO pay gap between the focal CEO and the annual second-top-paid CEO is above the sample median, zero otherwise. (*Source*: ExecuComp)
Non-diversified	Dummy that equals one if a firm only has one business segment, zero otherwise. (*Source*: Compustat)

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Variable	Definition
Managerial Ability Score	Managerial ability data is from Demerjian et al. (2012).
Product Market HHI	Product market HHI data is from Hoberg and Phillips (2016).
Industry-adjusted ROA	The adjusted value of *ROA* subtracting the median value in the same industry.
Industry-adjusted Stock Return	The adjusted value of annual stock return subtracting the median value in the same industry. (*Source*: Compustat)
State-level insider CEOs	The fraction of insider CEOs in each state and industry. (*Source*: ExecuComp, BoardEx)
State-level CEO Pay Skewness	The skewness of CEO pay in a focal firm's headquarter state in a given year (excluding the focal firm). (*Source*: ExecuComp)
Founder CEO	Dummy that equals one if the CEO is a founder but is not interim, and zero otherwise. (*Source*: ExecuComp)
Average VP Age	Average age of VPs. (*Source*: ExecuComp, BoardEx, 10-K files, and company's official websites)
Ln (σ VP Pay)	Natural logarithm of the standard deviation of VP compensation in a same firm. (*Source*: ExecuComp)
Ln (CEO Delta)	Natural logarithm of the compensation delta of CEO, following Core and Guay (2002) and Coles et al. (2006).
Ln (VP Delta)	Natural logarithm of the compensation delta of average VP, following Core and Guay (2002) and Coles et al. (2006).
Ln (CEO Vega)	Natural logarithm of the compensation vega of CEO, following Core and Guay (2002) and Coles et al. (2006).
Ln (VP Vega)	Natural logarithm of the compensation vega of average VP, following Core and Guay (2002) and Coles et al. (2006).
Strategy	The following six accounting numbers are averaged for a two-year rolling window and then ranked into quintiles in each industry-year: R&D expenses (over sales), marketing expense (over sales), one-year sales growth, net PPE (over total assets) (multiplied by -1), total employees (over total assets), and the standard deviation of total employees over the past five years. A measure of a firm's strategy is built by summing all the ranks, following Abernethy et al. (2024). (*Source*: Compustat)
Ln (Total Gap) Cons.	Natural logarithm of *Total Gap*, where we monotonically add a constant to make all the negative values of pay gap positive. (*Source*: ExecuComp)
Ln (Total Gap) Med. Gap	Natural logarithm of *Total Gap*, where we replace the negative values of pay gap with *Median Industry Gap*. (*Source*: ExecuComp)
Ln (Total Gap) Med. CEO Pay	Natural logarithm of *Total Gap*, where we recalculate the values by replacing the CEO's pay with *Median Industry CEO Pay* if the values of pay gap are negative. (*Source*: ExecuComp)

Table OA.2: CARs around the Events

This table reports company stock price reactions to the state-level changes in the enforceability of non-compete agreements (NCAs). The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) in states that have experienced changes in NCA enforceability for the period 1993–2018. Cumulative abnormal returns (CARs) are calculated over the pre-event windows (trading days [-11, -4] and [-3, -2]) and the event window (trading days [-1, 3]), where $t = 0$ is the date the court decides on the change in enforceability or the effective date of NCA regulations. Abnormal returns are calculated using the market model with CRSP equally weighted market returns (Column 1), or from the Fama-French five factors (Column 2). The *p*-values in parentheses are computed using the Kolari statistics (Kolari and Pynnönen (2010)) to address cross-correlation issues. ***, **, * correspond to significance levels of 1%, 5%, and 10%, respectively.

Panel A: Enforceability Up

	Market Model CARs	5-Factor CARs
	1	2
CAR [-11;-4]	0.124%	0.204%
	(0.607)	(0.436)
CAR [-3;-2]	-0.036%	-0.136%
	(0.804)	(0.640)
CAR [-1;3]	1.465%***	0.861%**
	(0.002)	(0.013)
Observations	259	

Panel B: Enforceability Down

	Market Model CARs	5-Factor CARs
	1	2
CAR [-11;-4]	0.080%	-0.850%
	(0.716)	(0.328)
CAR [-3;-2]	0.105%	-0.200%
	(0.708)	(0.667)
CAR [-1;3]	0.422%	-0.025%
	(0.473)	(0.894)
Observations	199	

Table OA.3: Executive Mobility

This table reports the effect of a state-level change in the enforceability of non-compete agreements (NCAs) on VP mobility. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) for the period 1993–2018. The coefficients are estimated using the stacked regression method by Baker et al. (2022) for a test window of $t \pm 10$ (t is the event year), using firms headquartered in never treated states as "clean" controls. The dependent variable is a mobility indicator, which takes the value of one if the focal executive is no longer with the same firm at year $t+1$ relative to t. Departures of executives aged above 65, likely due to retirement, are excluded. *NCA Enforceability Up (Down)* is measured in year t and is a binary variable that is set to one if a firm is headquartered in a state that has experienced an increase (decrease) in NCA enforceability over the current or previous years. All models include executive-cohort, industry-year-cohort, and region-year-cohort fixed effects. Detailed variable definitions can be found in the Appendix and Table OA.1. The standard errors in parentheses are clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to significance levels of 1%, 5%, and 10%, respectively.

	VP Mobility	
	1	2
NCA Enforceability Up	-0.049**	
	(0.023)	
NCA Enforceability Down		0.029
		(0.023)
No. of VPs	0.033***	0.032***
	(0.002)	(0.002)
Propensity of Relay Succession	0.003	0.003
	(0.006)	(0.005)
ROA	-0.032	-0.018
	(0.032)	(0.029)
Leverage	0.097***	0.088***
	(0.019)	(0.020)
Mkt to Book Ratio	-0.017***	-0.017***
	(0.005)	(0.005)
Cash Ratio	-0.007	-0.007
	(0.029)	(0.031)
Stk. Return Volatility	0.031*	0.028*
	(0.016)	(0.015)
Ln (Total Asset)	-0.060***	-0.064***
	(0.012)	(0.011)
Ln (Firm Age)	0.138***	0.134***
	(0.043)	(0.041)
GDP Growth	-0.138	-0.172
	(0.140)	(0.137)
Unemployment	0.005	0.004
	(0.004)	(0.004)
Observations	361,370	322,658
R-squared	0.399	0.406
Executive-Cohort FE	Yes	Yes
Industry-Year-Cohort FE	Yes	Yes
Region-Year-Cohort FE	Yes	Yes

Table OA.4: Alternative Measures

This table reports the effect of a state-level change in the enforceability of non-compete agreements (NCAs) on the executive pay gap. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) for the period 1993–2018. The coefficients are estimated using the stacked-regression method by Baker et al. (2022) for a test window of $t \pm 10$ (t is the event year), using firms headquartered in never treated states as "clean" controls. The regressions in Column 2 of Panels A and B in Table 2 are re-estimated by using alternative dependent variables, all measured in year $t+1$. *NCA Enforceability Up (Down)* is measured in year t and is a binary variable that is set to one if a firm is headquartered in a state that has experienced an increase (decrease) in NCA enforceability over the current or previous years. All models include firm-cohort, year-cohort, and headquarter-state-cohort fixed effects. Detailed variable definitions can be found in the Appendix and Table OA.1. The standard errors in parentheses are clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to significance levels of 1%, 5%, and 10%, respectively.

Panel A: Enforceability Up

	Ln (Total Gap Median VP)	Ln (Total Gap Max VP)	Gini Coefficient	Rank (Total Gap)	Ln (Total Gap) Adj.
	1	2	3	4	5
NCA Enforceability Up	0.193***	0.228***	0.016***	0.038***	0.203***
	(0.047)	(0.056)	(0.006)	(0.010)	(0.043)
Observations	83,648	75,303	83,849	83,849	83,589
R-squared	0.673	0.625	0.353	0.646	0.658
Controls from Table 2	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	Yes

Panel B: Enforceability Down

	Ln (Total Gap Median VP)	Ln (Total Gap Max VP)	Gini Coefficient	Rank (Total Gap)	Ln (Total Gap) Adj.
	1	2	3	4	5
NCA Enforceability Down	-0.055	-0.052	-0.003	-0.016	-0.108**
	(0.047)	(0.074)	(0.005)	(0.010)	(0.043)
Observations	75,186	67,666	75,381	75,381	75,134
R-squared	0.683	0.638	0.363	0.654	0.668
Controls from Table 2	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	Yes

Table OA.5: Different Specifications

This table reports the effect of a state-level change in the enforceability of non-compete agreements (NCAs) on the executive pay gap. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) for the period 1993–2018. The regressions in Column 2 of Panels A and B in Table 2 are re-estimated by using alternative specifications. The dependent variable is *Ln (Total Gap)*, measured in year $t+1$. *NCA Enforceability Up (Down)* is measured in year t and is a binary variable that is set to one if a firm is headquartered in a state that has experienced an increase (decrease) in NCA enforceability over the current or previous years. Detailed variable definitions can be found in the Appendix and Table OA.1. The standard errors in parentheses are clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to significance levels of 1%, 5%, and 10%, respectively.

Panel A: Enforceability Up

	Ln (Total Gap)				
	Test window: $t \pm 3$	Test window: $t \pm 5$	Not yet treated as controls	Generalized DID	Callaway and Sant'Anna (2021) estimator
	1	2	3	4	5
NCA Enforceability Up	0.133***	0.169**	0.185***	0.180***	0.153**
	(0.048)	(0.064)	(0.047)	(0.053)	(0.077)
Observations	33,515	50,818	101,176	22,091	-
R-squared	0.715	0.676	0.637	0.617	-
Controls from Table 2	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	No

Panel B: Enforceability Down

	Ln (Total Gap)				
	Test window: $t \pm 3$	Test window: $t \pm 5$	Not yet treated as controls	Generalized DID	Callaway and Sant'Anna (2021) estimator
	1	2	3	4	5
NCA Enforceability Down	-0.015	-0.032	-0.077*	-0.085*	0.032
	(0.055)	(0.041)	(0.044)	(0.045)	(0.080)
Observations	30,835	45,965	88,065	20,770	-
R-squared	0.726	0.691	0.655	0.622	-
Controls from Table 2	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	No

Table OA.6: Retention Mechanism for Executive Pay Levels

This table reports the effect of a state-level change in the enforceability of non-compete agreements (NCAs) on CEO and VP pay levels. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) for the period 1993–2018. The coefficients are estimated using the stacked-regression method by Baker et al. (2022) for a test window of $t \pm 10$ (t is the event year), using firms headquartered in never treated states as "clean" controls. The regressions in Column 1 and 4 of Panels A and B in Table 4 are re-estimated by using sub-sets of observations and adding interaction terms for the test variable *NCA Enforceability Up (Down)*. Columns 1, 2, 5, and 6 of Panels A and C report the results for the top and bottom terciles of the fraction of insider CEOs in the same industry as the firm's state. Columns 3, 4, 7, and 8 of Panels A and C report the results for the top and bottom terciles of the skewness of CEO pay for the state the firm belongs to. Panels A and C also report the p-values of F-statistics testing the differences in coefficients on *NCA Enforceability Up* (*Down*) between the respective partitioned subsamples. In Panels B and D, *NCA Enforceability Up (Down)* is interacted with measures of average VP age, managerial ability score (Demerjian et al. (2012)), product market HHI (Hoberg and Phillips (2016)), the degree of litigation in an industry, market to book ratios, and skilled worker reliance (Qiu and Wang (2021)). *Young VPs* equals one if the average age of VPs is below the sample annual median; *High Ability* equals one if managerial ability is above the sample annual median in the same industry; *Low HHI* equals one if the HHI is below the annual median in the same industry; *Litigation Industry* equals one if the firm is in one of the following industries: pharmaceutical/biotechnology (SIC codes 2833–2826, 8731–8734), computer (3570–3577, 7370–7374), electronics (3600–3674), or retail (5200–5961) (Cheng and Warfield (2005)); *High Mkt to Book* equals one if the *Mkt to Book Ratio* is above the sample annual median in the same industry; *Low Skill Reliance* equals one if the skilled labor risk is below the annual median. The dependent variables are *Ln (CEO Pay)* and *Ln (VP Pay)*, measured in year $t+1$. *NCA Enforceability Up* (*Down*) is measured in year t and is a binary variable that is set to one if a firm is headquartered in a state that has experienced an increase (decrease) in NCA enforceability over the current or previous years. All models include firm-cohort, year-cohort, and headquarter-state-cohort fixed effects. Detailed variable definitions can be found in the Appendix and Table OA.1. The standard errors in parentheses are clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to significance levels of 1%, 5%, and 10%, respectively.

Panel A: Effects of Enforceability Up on CEO and VP Pay: State Insider and Pay Skewness

	Ln (CEO Pay)				Ln (VP Pay)			
	Fraction of insider CEOs		Skewness of CEO pay		Fraction of insider CEOs		Skewness of CEO pay	
	Top tercile	Bottom tercile	Top tercile	Bottom tercile	Top tercile	Bottom tercile	Top tercile	Bottom tercile
	1	2	3	4	5	6	7	8
NCA Enforceability Up	0.120***	0.283**	0.309***	0.104**	0.129***	0.142**	0.206***	0.081***
	(0.041)	(0.104)	(0.047)	(0.045)	(0.038)	(0.060)	(0.041)	(0.030)
p-value	(0.475)		(0.001)		(0.272)		(0.020)	
Observations	24,135	23,237	22,649	22,106	24,135	23,237	22,649	22,106
R-squared	0.789	0.745	0.766	0.805	0.830	0.792	0.803	0.841
Controls from Table 4	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table OA.6 (Continued)

Panel B: Effects of Enforceability Up on CEO and VP Pay: Cross-Sectional Tests

	Ln (CEO pay)						Ln (VP pay)					
	1	2	3	4	5	6	7	8	9	10	11	12
NCA Enforceability Up	0.137***	0.117***	0.119***	0.155***	0.147***	0.187***	0.093***	0.067**	0.063**	0.096***	0.090***	0.116***
	(0.033)	(0.036)	(0.035)	(0.041)	(0.034)	(0.050)	(0.030)	(0.031)	(0.030)	(0.032)	(0.029)	(0.042)
Young VPs	0.017						0.004					
	(0.012)						(0.007)					
NCA Enforceability Up × Young VPs	0.020						-0.023					
	(0.019)						(0.018)					
High Ability		0.004						0.006				
		(0.011)						(0.009)				
NCA Enforceability Up × High Ability		0.066***						0.042**				
		(0.024)						(0.017)				
Low HHI			-0.008						-0.005			
			(0.012)						(0.010)			
NCA Enforceability Up × Low HHI			0.059**						0.045*			
			(0.024)						(0.024)			
NCA Enforceability Up × Litigation Industry				-0.032						-0.050		
				(0.079)						(0.038)		
High Mkt to Book					0.075***						0.056***	
					(0.016)						(0.015)	
NCA Enforceability Up × High Mkt to Book					0.007						-0.011	
					(0.022)						(0.036)	
Low Skill Reliance						0.021						0.006
						(0.031)						(0.019)
NCA Enforceability Up × Low Skill Reliance						-0.046*						-0.032
						(0.027)						(0.034)
Observations	82,180	80,980	81,147	82,426	82,426	63,941	82,180	80,980	81,147	82,426	82,426	63,941
R-squared	0.755	0.752	0.755	0.755	0.755	0.759	0.803	0.802	0.803	0.803	0.804	0.807
Controls from Table 4	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table OA.6 (Continued)

Panel C: Effects of Enforceability Down on CEO and VP Pay: State Insider and Pay Skewness

	Ln (CEO pay)				Ln (VP pay)			
	Fraction of insider CEOs		Skewness of CEO pay		Fraction of insider CEOs		Skewness of CEO pay	
	Top tercile	Bottom tercile	Top tercile	Bottom tercile	Top tercile	Bottom tercile	Top tercile	Bottom tercile
	1	2	3	4	5	6	7	8
NCA Enforceability Down	-0.064	-0.054	0.012	-0.078	-0.012	-0.002	0.084***	0.033
	(0.083)	(0.113)	(0.036)	(0.065)	(0.057)	(0.079)	(0.021)	(0.056)
p-value	(0.111)		(0.164)		(0.161)		(0.860)	
Observations	21,766	20,895	20,283	19,977	21,766	20,895	20,283	19,977
R-squared	0.796	0.751	0.778	0.802	0.834	0.796	0.812	0.839
Controls from Table 4	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table OA.6 (Continued)

Panel D: Effects of Enforceability Down on CEO and VP Pay: Cross-Sectional Tests

	Ln (CEO Pay)						Ln (VP Pay)					
	1	2	3	4	5	6	7	8	9	10	11	12
NCA Enforceability Down	-0.028	-0.032	-0.055	-0.025	-0.046	-0.140**	0.015	0.012	-0.023	0.038	0.008	-0.070*
	(0.025)	(0.035)	(0.039)	(0.047)	(0.038)	(0.054)	(0.026)	(0.031)	(0.032)	(0.040)	(0.038)	(0.038)
Young VPs	0.018						0.004					
	(0.012)						(0.006)					
NCA Enforceability Down × Young VPs	-0.038						-0.012					
	(0.044)						(0.014)					
High Ability		0.012						0.017				
		(0.011)						(0.011)				
NCA Enforceability Down × High Ability		-0.023						-0.003				
		(0.066)						(0.060)				
Low HHI			-0.008						-0.007			
			(0.012)						(0.008)			
NCA Enforceability Down × Low HHI			0.002						0.058**			
			(0.027)						(0.027)			
NCA Enforceability Down × Litigation Industry				-0.095						-0.135*		
				(0.099)						(0.069)		
High Mkt to Book					0.074***						0.059***	
					(0.017)						(0.015)	
NCA Enforceability Down × High Mkt to Book					-0.001						0.003	
					(0.056)						(0.075)	
Low Skill Reliance						0.011						0.000
						(0.019)						(0.015)
NCA Enforceability Down × Low Skill Reliance						-0.024						-0.009
						(0.082)						(0.067)
Observations	73,952	72,887	73,004	74,180	74,180	56,319	73,952	72,887	73,004	74,180	74,180	56,319
R-squared	0.762	0.759	0.762	0.762	0.762	0.768	0.809	0.807	0.809	0.809	0.809	0.813
Controls from Table 4	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table OA.7: Alternative Mechanisms: CEO Power

This table reports the effect of a state-level increase in the enforceability of non-compete agreements (NCAs) on alternative measures of the executive pay gap, and on the pay gap conditional on the likelihood of hostile takeovers. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) for the period 1993–2018. The coefficients are estimated using the stacked-regression method by Baker et al. (2022) for a test window of $t \pm 10$ (t is the event year), using firms headquartered in never treated states as "clean" controls. In Panel A, we consider alternative measures of the executive pay gap (*CEO Pay Slice* and *Gap Residuals*), both of which are measured in year t+1. In Panel B, the dependent variable is *Ln (Total Gap)*, measured in year $t+1$. The regression in Column 2 of Panel A in Table 2 is re-estimated by adding the interaction term between *NCA Enforceability Up* and *Low Takeover Index*, which equals one if the hostile takeover index (Cain et al. (2017)) is below the annual median in the same state. *NCA Enforceability Up* is measured in year t and is a binary variable that is set to one if a firm is headquartered in a state that has experienced an increase in NCA enforceability over the current or previous years. All models include firm-cohort, year-cohort, and headquarter-state-cohort fixed effects. Detailed variable definitions can be found in the Appendix and Table OA.1. The standard errors in parentheses are clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to significance levels of 1%, 5%, and 10%, respectively.

Panel A. Alternative Pay Gap Measures

	CEO Pay Slice		Gap Residuals	
	1	2	3	4
NCA Enforceability Up	0.009*	0.008	0.159***	0.169***
	(0.005)	(0.005)	(0.040)	(0.044)
Observations	81,487	81,487	83,815	83,815
R-squared	0.371	0.387	0.579	0.609
Controls from Table 2	No	Yes	No	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes

Panel B. Hostile Takeovers

	Ln (Total Gap)
NCA Enforceability Up	0.254***
	(0.053)
Low Takeover Index	-0.034
	(0.033)
NCA Enforceability Up × Low Takeover Index	-0.139**
	(0.066)
Observations	71,864
R-squared	0.647
Controls from Table 2	Yes
Firm-Cohort FE	Yes
Year-Cohort FE	Yes
State-Cohort FE	Yes

Table OA.8: Alternative Mechanisms: Talent Differentials

This table reports the effect of a state-level increase in the enforceability of non-compete agreements (NCAs) on the overall executive pay gap while controlling for managerial ability and on the pay gap for each individual VP. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) for the period 1993–2018. The coefficients are estimated using the stacked-regression method by Baker et al. (2022) for a test window of $t \pm 10$ (t is the event year), using firms headquartered in never treated states as "clean" controls. We re-estimate the specification in Column 2 of Panel A in Table 2 with some variations. In Panel A, the dependent variable is *Ln (Total Gap)*, measured in year $t+1$. We add controls for managerial ability, including the managerial ability score (Demerjian et al. (2012)), the industry-adjusted ROA, and the industry-adjusted stock return. In Panel B, we replace the overall firm-level executive pay gap *Ln (Total Gap)* with a similar pay gap at the firm-VP-level for each individual VP (*Ln (VP Total Gap)*) in year $t+1$. *NCA Enforceability Up* is measured in year t and is a binary variable that is set to one if a firm is headquartered in a state that has experienced an increase in NCA enforceability over the current or previous years. We control for combinations of firm-cohort, year-cohort, state-cohort, and either CEO-VP-cohort or CEO-VP-firm-cohort fixed effects. Detailed variable definitions can be found in the Appendix and Table OA.1. The standard errors in parentheses are clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to significance levels of 1%, 5%, and 10%, respectively.

Panel A: Managerial Ability

	Ln (Total Gap)			
	1	2	3	4
NCA Enforceability Up	0.186***	0.190***	0.190***	0.187***
	(0.051)	(0.050)	(0.051)	(0.049)
Managerial Ability Score	0.347***			0.225**
	(0.114)			(0.106)
Adj. ROA		0.697***		0.536***
		(0.084)		(0.093)
Adj. Stk. Return			0.165***	0.122***
			(0.020)	(0.018)
Observations	82,176	83,849	83,849	82,176
R-squared	0.631	0.636	0.635	0.633
Controls from Table 2	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes

Table OA.8 (Continued)

Panel B: Pay Gap at the Firm-VP-Level

	Ln (VP Total Gap)			
	1	2	3	4
NCA Enforceability Up	0.054*	0.058*	0.055*	0.058*
	(0.029)	(0.032)	(0.029)	(0.032)
Observations	267,993	267,993	267,860	267,860
R-squared	0.761	0.764	0.761	0.764
Controls from Table 2	No	Yes	No	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	No	Yes
CEO-VP-Cohort FE	Yes	Yes	No	No
Firm-Cohort FE	Yes	Yes	Yes	No
CEO-VP-Firm-Cohort FE	No	No	Yes	Yes

Table OA.9: Alternative Mechanisms: CEO Bargaining Power

This table reports the effect of a state-level increase in the enforceability of non-compete agreements (NCAs) on the executive pay gap conditional on several variables. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) for the period 1993–2018. The coefficients are estimated using the stacked-regression method by Baker et al. (2022) for a test window of $t \pm 10$ (t is the event year), using firms headquartered in never treated states as "clean" controls. The regression in Column 2 of Panel A in Table 2 is re-estimated by adding interaction terms for the test variable *NCA Enforceability Up*. *NCA Enforceability Up* is interacted with measures of whether the CEO is retiring, a generalist CEO (Custódio et al. (2013)), or a productive CEO (Masulis and Zhang (2014)), as well as measures of CEO tournament and whether the CEO's firm is a non-diversified business. *Retiring CEO* equals one if the CEO is over 65 years old; *Generalist CEO* equals one if the generalist score is above the sample annual median in the same industry; *Productive CEO* equals one if the two orthogonal factors of a principal component analysis with eigenvalues greater than one are above their respective sample medians; *High CEO Industry Tourn.* equals one if the CEO pay gap between the focal CEO and the second-top-paid CEO in a specific industry is above the sample median; *High CEO State Tourn.* equals one if the CEO pay gap between the focal CEO and the second-top-paid CEO in a specific state is above the sample median; *High CEO Tourn.* equals one if the CEO pay gap between the focal CEO and the annual second-top-paid CEO is above the sample median; *Non-diversified* equals one if a firm only has one business segment. The dependent variable is *Ln (Total Gap)*, measured in year $t+1$. *NCA Enforceability Up* is measured in year t and is a binary variable that is set to one if a firm is headquartered in a state that has experienced an increase in NCA enforceability over the current or previous years. All models include firm-cohort, year-cohort, and headquarter-state-cohort fixed effects. Detailed variable definitions can be found in the Appendix and Table OA.1. The standard errors in parentheses are clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to significance levels of 1%, 5%, and 10%, respectively.

Table OA.9 (Continued)

	Ln (Total Gap)						
	1	2	3	4	5	6	7
NCA Enforceability Up	0.177***	0.190***	0.231***	0.154**	0.186***	0.171***	0.244***
	(0.054)	(0.048)	(0.080)	(0.058)	(0.062)	(0.059)	(0.057)
NCA Enforceability Up	0.128						
× Retiring CEO	(0.186)						
Generalist CEO		0.108***					
		(0.023)					
NCA Enforceability Up		-0.037					
× Generalist CEO		(0.061)					
Productive CEO			0.009				
			(0.052)				
NCA Enforceability Up			-0.104				
× Productive CEO			(0.109)				
High CEO Industry Tourn.				-0.080***			
				(0.029)			
NCA Enforceability Up				0.077			
× High CEO Industry Tourn.				(0.050)			
High CEO State Tourn.					-0.089***		
					(0.022)		
NCA Enforceability Up					0.051		
× High CEO State Tourn.					(0.039)		
High CEO Tourn.						-0.149***	
						(0.024)	
NCA Enforceability Up						0.035	
× High CEO Tourn.						(0.030)	
Non-diversified							0.061*
							(0.036)
NCA Enforceability Up							-0.148*
× Non-diversified							(0.080)
Observations	83,849	78,117	39,898	78,839	66,458	82,717	83,849
R-squared	0.634	0.637	0.701	0.623	0.622	0.632	0.634
Controls from Table 2	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table OA.10: Different Coding Schemes

This table reports the effect of a state-level increase in the enforceability of non-compete agreements (NCAs) on the executive pay gap. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) for the period 1993–2018. The regression in Column 2 of Panel A in Table 2 is re-estimated by using different coding schemes of the variations in NCA enforcement. The dependent variable is *Ln (Total Gap)*, measured in year $t+1$. All models include firm-cohort, year-cohort, and headquarter-state-cohort fixed effects. Detailed variable definitions can be found in the Appendix and Table OA.1. The standard errors in parentheses are clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to significance levels of 1%, 5%, and 10%, respectively.

	Ln (Total Gap)				
	Coding Illinois as treated	Pre-FAS 123R	Post-FAS 123R	Exclude Florida & Idaho	NCA Index
	1	2	3	4	5
NCA Enforceability Up	0.166***	0.222***	0.117*	0.186***	
	(0.052)	(0.052)	(0.067)	(0.056)	
NCA Index					0.052*
					(0.027)
Observations	84,689	18,468	49,111	58,669	28,449
R-squared	0.634	0.631	0.710	0.636	0.617
Controls from Table 2	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	Yes

Table OA.11: Exclusion of CEO Turnovers

This table reports the effect of a state-level increase in the enforceability of non-compete agreements (NCAs) on the executive pay gap. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) for the period 1993–2018. The coefficients are estimated using the stacked-regression method by Baker et al. (2022) for a test window of $t \pm 10$ (t is the event year), using firms headquartered in never treated states as "clean" controls. The regression in Column 2 of Panel A in Table 2 is re-estimated by excluding CEO turnover events. We remove observations that coincide with CEO turnovers in year t (Column 1), the two years following a turnover (Column 2), or the five years following a turnover (Column 3). We also follow Jenter and Kanaan (2015) and Peters and Wagner (2014) to identify forced CEO turnovers. Based on these classifications, we remove firms that have ever fired a CEO (Column 4). The dependent variable is *Ln (Total Gap)*, measured in year $t+1$. *NCA Enforceability Up* is measured in year t and is a binary variable that is set to one if a firm is headquartered in a state that has experienced an increase in NCA enforceability over the current or previous years. All models include firm-cohort, year-cohort, and headquarter-state-cohort fixed effects. Detailed variable definitions can be found in the Appendix and Table OA.1. The standard errors in parentheses are clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to significance levels of 1%, 5%, and 10%, respectively.

	Ln (Total Gap)			
	Exclude turnover year	Exclude two years since turnover	Exclude five years since turnover	Exclude firms with fired CEOs
	1	2	3	4
NCA Enforceability Up	0.177***	0.193***	0.278**	0.212***
	(0.059)	(0.058)	(0.104)	(0.068)
Observations	75,188	66,622	36,443	53,024
R-squared	0.653	0.665	0.706	0.648
Controls from Table 2	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes

Table OA.12: Omitted Variables

This table reports the effect of a state-level increase in the enforceability of non-compete agreements (NCAs) on the executive pay gap. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) for the period 1993–2018. The coefficients are estimated using the stacked-regression method by Baker et al. (2022) for a test window of $t \pm 10$ (t is the event year), using firms headquartered in never treated states as "clean" controls. The regression in Column 2 of Panel A in Table 2 is re-estimated by controlling for additional variables, including conditional variables (state-level insider CEOs, state-level CEO pay skewness, founder CEO, average VP age, managerial ability score (Demerjian et al. (2012)), and product market HHI (Hoberg and Phillips (2016)), pay-related variables (*Ln (σ VP Pay)*, *Ln (CEO Delta)*, *Ln (VP Delta)*, *Ln (CEO Vega)*, *Ln (VP Vega)*), firm strategy (Abernethy et al. (2024)), and accounting variables (*ROA*, *Mkt to Book Ratio*, *Leverage*, *Cash Ratio*, and *Dividend Payer*). The dependent variable is *Ln (Total Gap)*, measured in year $t+1$. *NCA Enforceability Up* is measured in year t and is a binary variable that is set to one if a firm is headquartered in a state that has experienced an increase in NCA enforceability over the current or previous years. All models include firm-cohort, year-cohort, and headquarter-state-cohort fixed effects. Detailed variable definitions can be found in the Appendix and Table OA.1. The standard errors in parentheses are clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to significance levels of 1%, 5%, and 10%, respectively.

	Ln (Total Gap)						
	Conditional Vars.	σ VP Pay	CEO and VP Delta	CEO and VP Vega	Strategy	Accounting Vars.	All
	1	2	3	4	5	6	7
NCA Enforceability Up	0.163***	0.164***	0.133***	0.171***	0.201***	0.174***	0.113***
	(0.044)	(0.047)	(0.039)	(0.043)	(0.051)	(0.046)	(0.033)
Observations	79,492	83,849	76,850	69,790	69,898	82,625	56,245
R-squared	0.632	0.650	0.655	0.639	0.650	0.643	0.684
Controls from Table 2	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table OA.13: Additional FEs

This table reports the effect of a state-level increase in the enforceability of non-compete agreements (NCAs) on the executive pay gap. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) for the period 1993–2018. The coefficients are estimated using the stacked-regression method by Baker et al. (2022) for a test window of $t \pm 10$ (t is the event year), using firms headquartered in never treated states as "clean" controls. The regression in Column 2 of Panel A in Table 2 is re-estimated by using different fixed effects. The dependent variable is *Ln (Total Gap)*, measured in year $t+1$. *NCA Enforceability Up* is measured in year t and is a binary variable that is set to one if a firm is headquartered in a state that has experienced an increase in NCA enforceability over the current or previous years. All models include firm-cohort, year-cohort, and headquarter-state-cohort fixed effects. Detailed variable definitions can be found in the Appendix and Table OA.1. The standard errors in parentheses are clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to significance levels of 1%, 5%, and 10%, respectively.

	Ln (Total Gap)					
	1	2	3	4	5	6
NCA Enforceability Up	0.155***	0.192***	0.111**	0.135**	0.187***	0.100***
	(0.039)	(0.050)	(0.045)	(0.066)	(0.049)	(0.037)
Observations	83,776	83,849	83,849	82,983	83,849	81,271
R-squared	0.649	0.637	0.637	0.645	0.636	0.696
Controls from Table 2	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	No	No	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	No	No	No	No	No
Size-Year FE	No	Yes	No	No	No	No
Region-Year FE	No	No	Yes	No	No	No
State Inc.-Year FE	No	No	No	Yes	No	No
Firm Age FE	No	No	No	No	Yes	No
CEO-Firm FE	No	No	No	No	No	Yes

Table OA.14: Additional Robustness Tests

This table reports the effect of a state-level increase in the enforceability of non-compete agreements (NCAs) on the executive pay gap. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) for the period 1993–2018. The coefficients are estimated using the stacked-regression method by Baker et al. (2022) for a test window of $t \pm 10$ (t is the event year), using firms headquartered in never treated states as "clean" controls. The regression in Column 2 of Panel A in Table 2 is re-estimated by using alternative samples or alternative industry classifications. The dependent variable is *Ln (Total Gap)*, measured in year $t+1$. *NCA Enforceability Up* is measured in year t and is a binary variable that is set to one if a firm is headquartered in a state that has experienced an increase in NCA enforceability over the current or previous years. All models include firm-cohort, year-cohort, and headquarter-state-cohort fixed effects. Detailed variable definitions can be found in the Appendix and Table OA.1. The standard errors in parentheses are clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to significance levels of 1%, 5%, and 10%, respectively.

	Ln (Total Gap)							
	Exclude Crisis	ExecuComp Sample	Exclude Reloca-tion	Decrease Before	Increase Before	Change before	SIC-2 Industry	NAICS-3 Industry
	1	2	3	4	5	6	7	8
NCA Enforceability Up	0.183***	0.186***	0.200***	0.216***	0.165**	0.217***	0.208***	0.183***
	(0.050)	(0.052)	(0.053)	(0.050)	(0.072)	(0.070)	(0.048)	(0.043)
Observations	74,495	81,939	71,751	68,472	53,489	39,374	83,397	82,767
R-squared	0.634	0.637	0.633	0.630	0.634	0.628	0.636	0.636
Controls from Table 2	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table OA.15: Footnote Tests

This table reports the effect of a state-level change in the enforceability of non-compete agreements (NCAs) on the executive pay gap, the likelihood of promoting insider CEOs, or the use of explicit relative performance evaluation (RPE). The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) for the period 1993–2018. The coefficients are estimated using the stacked-regression method by Baker et al. (2022) for a test window of $t \pm 10$ (t is the event year), using firms headquartered in never treated states as "clean" controls. *NCA Enforceability Up (Down)* is measured in year t and is a binary variable that is set to one if a firm is headquartered in a state that has experienced an increase (decrease) in NCA enforceability over the current or previous years. All models include firm-cohort, year-cohort, and headquarter-state-cohort fixed effects. Detailed variable definitions can be found in the Appendix and Table OA.1. The standard errors in parentheses are clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to significance levels of 1%, 5%, and 10%, respectively.

Panel A: Requiring Firms to Have Multiple VPs

	Ln (Total Gap)			
	At Least Two VPs	At Least Three VPs	At Least Two VPs	At Least Three VPs
	1	2	3	4
NCA Enforceability Up	0.183***	0.173***		
	(0.049)	(0.047)		
NCA Enforceability Down			-0.083*	-0.092**
			(0.043)	(0.040)
Observations	83,462	81,127	75,040	72,941
R-squared	0.632	0.628	0.650	0.646
Controls from Table 2	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes

Panel B: Negative Pay Gaps

	Enforceability Up			Enforceability Down		
	Ln (Total Gap) Cons.	Ln (Total Gap) Med. Gap	Ln (Total Gap) Med. CEO Pay	Ln (Total Gap) Cons.	Ln (Total Gap) Med. Gap	Ln (Total Gap) Med. CEO Pay
	1	2	3	4	5	6
NCA Enforceability	0.052**	0.184***	0.187***	-0.022	-0.070*	-0.069
	(0.024)	(0.046)	(0.043)	(0.023)	(0.035)	(0.048)
Observations	89,767	89,767	89,632	80,682	80,682	80,588
R-squared	0.520	0.632	0.623	0.532	0.641	0.632
Controls from Table 2	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Table OA.15 (Continued)

Panel C: Promotion of Insider CEO

	CEO Is Insider	
	1	2
NCA Enforceability Up	-0.022	
	(0.023)	
NCA Enforceability Down		0.033
		(0.041)
Observations	82,426	74,180
R-squared	0.688	0.708
Controls from Table 4	Yes	Yes
Firm-Cohort FE	Yes	Yes
Year-Cohort FE	Yes	Yes
State-Cohort FE	Yes	Yes

Panel D: Inclusion of Explicit RPE

	RPE	
	1	2
NCA Enforceability Up	-0.055	
	(0.042)	
NCA Enforceability Down		0.006
		(0.035)
Observations	42,751	37,810
R-squared	0.589	0.606
Controls from Table 4	Yes	Yes
Firm-Cohort FE	Yes	Yes
Year-Cohort FE	Yes	Yes
State-Cohort FE	Yes	Yes