Outside employment opportunities and tournament incentives

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Abstract

We present robust evidence that firms enlarge the executive pay gap when executive mobility is constrained by the enhanced enforceability of 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 and in years close to CEO-dismissal events in which tournament-incentive mechanisms are more prevalent. Following restrictions to executive mobility, equity portfolios that long (short) firms that (do not) boost the executive pay gap generate positive alphas. We rule out CEO power, managerial talent differentials, and CEO bargaining power as alternative explanations driving our results. Our findings suggest that there is a substitution effect between external (outside employment opportunities) and internal (pay gap) tournament incentives, hence provide novel evidence on a new executive pay gap determinant.

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

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1 Introduction

The compensation gap across the ordinal rank of workers in an organization is commonly proposed as 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 that is associated with increased pecuniary wealth and social reputation. Thus, a pay gap, like a tournament prize, provides motivation for existing employees to work harder.

In modern corporations, promotion-driven pay increases are particularly significant among top executives (Belzil and Bognanno, 2008), especially those who get the top prize, that is, the coveted Chief Executive Officer (CEO) position. Therefore, tournament-incentive effects are likely to greatly affect the behavior of the firm's vice presidents (VPs). There is significant evidence that tournament incentives induce VPs to exert effort and boost their willingness to take risk (e.g., Kale, Reis, and Venkateswaran, 2009; Kini and Williams, 2012; Lazear and Rosen, 1981), thus, shape various corporate outcomes.[1]

While various findings are about the real effects of rank-order tournament incentives, the evidence on their determinants is scant.[2] 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 VPs face in their current workplace. In other words, we aim to address the following question: do firms adjust their VPs' internal tournament incentives following changes to the VPs' external job opportunities?

Internal promotions (vertical moves), and the resulting pay increases, are important incentive devices for VPs but, in a mobile market, VPs are also incentivized by employment

[1]For example, existing studies show the significant impact of tournament incentives on firm performance and valuation (Burns, Minnick, and Starks, 2017; Kale et al., 2009), financial policies (Hasan, Navone, To, and Wu, 2020; Kini and Williams, 2012; Kubick and Masli, 2016; Phan, Simpson, and Nguyen, 2017), as well as the incidence of corporate lawsuits or fraud (Haβ, Mller, and Vergauwe, 2015; Shi, Connelly, and Sanders, 2016). Tournament incentives also affect companies' Initial Public Offerings, for example, their failure risk (Colak, Gounopoulos, Loukopoulos, and Loukopoulos, 2021). See Lazear (2018) for a relevant recent review.

[2]Earlier economic research concludes that pay differentials are greater when promotion rates are lower (e.g., Bognanno, 2001; Main, O'Reilly, and Wade, 1993). Kale et al. (2009) document the importance of several other dimensions and provide related evidence (see Section 3.1 for details). Henderson and Fredrickson (2001) argue that considerations around management coordination are associated with pay gap design. Burns et al. (2017) show that cultural factors are helpful in explaining cross-country variation in the executive pay gap.

opportunities outside the firm. They can strive for the CEO position in other firms (diagonal move) or join another firm on a similar job title but with better pay and status (horizontal move). Horizontal and diagonal moves are particularly common. Graham, Kim, and Kim (2020) document that 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 term external tournament incentives those associated with a VP's opportunity to leave for an outside job, which would motivate the VP to perform better.[3] The concept of an external tournament is broad, as it relates to all the prospective job opportunities in the horizontal and/or diagonal directions, and implies multiple outcomes of the prize size in various external horse races. As a result, it is obviously hard to develop a proxy for the external tournament incentives.[4] However, we argue that the most straightforward factor associated with such incentives is a VP's potential job mobility. If a VP's ability to join another firm is low, the tournament incentives arising from the external executive market, in any form, are limited, and vice versa.

Based on this argument, and similar to Ewens and Marx (2018) and Kini, Williams, and Yin (2021), we use the variations in the state-level enforceability of non-compete agreements (NCAs) between firms and their employees as quasi-natural experiments associated with changes to VPs' external tournament incentives. NCAs are legal clauses that are frequently embedded in employment contracts to restrict employees' ability to join other firms. In particular, more than 70% of top executives are constrained by NCAs (Garmaise, 2011; Kini et al., 2021).[5] The actual numbers are likely higher because firms do not necessarily report such em-

[3]We recognize that a CEO also has the potential to join external tournaments, for example to be the CEO of a larger or more prestigious firm (Coles, Li, and Wang, 2018). However, recent evidence suggests that CEO reallocation across firms is rare (Cziraki and Jenter, 2022). In Section 5.3, we find that differences in the external tournament opportunities between a firm's CEO and VPs cannot explain our findings.

[4]In addition, because non-pecuniary factors, such as corporate social reputation (Focke, Maug, and Niessen-Ruenzi, 2017), also characterize an individual's external tournament incentives, a VP may not always choose a career destination with the largest foreseeable pay increase, which further complicates the design of an empirical proxy.

[5]A widely cited survey by Starr, Prescott, and Bishara (2021) finds that NCAs are commonly used not only for executives, but also for less-strategic workers. Such clauses are estimated to affect at least 18% of U.S. labor participants. Several states, such as Oregon, Nevada, and Illinois, have amended their laws prohibiting the use of non-compete clauses for low-paid earners (see, for example, "Oregon, Nevada and Illinois further limit restrictive covenants", JDSUPRA, June 9, 2021). The Federal Trade Commission (FTC) recently suggested a rule to forbid employers from imposing non-competes on their workers (see, "FTC proposes rule

ployment contracts (Lin, Peters, and Seo, 2022). Due to the segmentation in executive labor markets (e.g., Ma, Pan, and Stubben, 2020; Yonker, 2017) and assuming that the tendency to include NCAs for VPs is a persistent firm-level feature (Chen, Jung, Peng, and Zhang, 2022), 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). Notably, these state-level rules are unlikely to be influenced by the actions of a particular firm and can, therefore, be deemed exogenous to a firm's executive pay system.[6] We identify nine (six) states that have been through increases (decreases) in NCA enforceability from 1993 to 2018.[7] Exploiting the changes in enforceability in a difference-in-differences (DID) framework, we examine how labor market frictions shape internal tournaments and, specifically, the executive pay gap, i.e., the pay gap between the CEO and executive directors.

We expect an increase in NCA enforceability to lead to a larger executive pay gap for two main reasons. First, the outside opportunities that function as implicit incentives for executives are less effective when NCAs become more enforceable and executive mobility is reduced. Internal promotion becomes the only way for executives to boost their utility. As a consequence, firms are incentivized to restructure executive compensation contracts and widen the gap between the pay of their CEOs and that of the VPs. A larger pay gap enhances the utility associated with future promotions (i.e., tournament prize) and makes up for the lost external opportunities (Gibbons and Murphy, 1992). Second, if executive mobility is reduced on the labor market, firms might struggle to search for capable candidates externally, therefore selecting and promoting well-performing current employees becomes crucial. A more competitive internal tournament generates information on and signals about the quality of executives, which are useful to find ideal candidates for promotion. In short, the internal pay gap is expected to increase as a substitute for VPs' weaker external tournament incentives.

to ban noncompete clauses", FTC, January 5, 2023). However, our focus throughout this paper is on the top management team, which is not the target of these initiatives.

[6] Bai, Eldemire, and Serfling (2023) and Johnson, Lavetti, and Lipsitz (2021) 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 (2022) also report 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.

[7] The events analyzed here are comparable to those in studies with similar sample windows, such as Bai et al. (2023), Ewens and Marx (2018), and Kini et al. (2021). A few discrepancies are discussed in detail and motivated in Appendix A.

A counterargument, however, is that firms might be less motivated to increase tournament incentives for their executives in the presence of stricter NCA enforcement. First, fewer outside employment opportunities mean that VPs are motivated to perform better in their current jobs since potential dismissal could result in costly and delayed reentry to the executive labor market. Second, prior studies find that executives face higher dismissal risks under stricter NCA enforcement since firms now encounter lower costs of competitive harm from forced executive turnover decisions (Kini et al., 2021; Lin et al., 2022). Therefore, the threat of dismissal is rather powerful in this case and could motivate VPs even in the absence of greater incentives. Overall, it is an empirical question whether firms will increase internal tournament incentives when there is a reduction in executives' outside employment opportunities.

As a preliminary step to our empirical analysis, we show that more (less) restrictive state-level rules on NCAs lead to a significant reduction (growth) in executive mobility. In the main DID tests, we find a statistically significant enlargement in the executive pay gap following a state-level increase in NCA enforceability, after controlling for a comprehensive set of other variables as in Kale et al.'s (2009) study. In these tests, the executive pay gap (*Total Gap*) is computed as the difference between a CEO's total pay and the average total pay of the VPs from the CEO's firm. On average, a firm affected by the shock (a treated firm) approximately has a \$0.70 million larger pay gap than 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 \$3.05 million. Therefore, the impact of stronger NCA enforcement is economically meaningful as it is almost a quarter of that of firm size. We extend our regression analysis in a dynamic setting and find the effects on the pay gap, which are persistent over time, materialize after one year following an increase in NCA enforceability. Also, consistent with the parallel trends assumption, pre-shock trends in the pay gap are similar between treated and non-treated firms.

Though our central focus is on *Total Gap*, we decompose total executive compensation into the short- (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. The baseline finding of a positive effect of increased NCA enforceability on *Total Gap* also extends to both the *ST Gap* and *LT Gap*. However, the change in *ST Gap* is observed mainly in the first two years after the shock with the *LT Gap* playing a dominant role in explaining the long-lasting effects. The differences in timings are likely attributable to the nature of the specific components in the executive pay package.

We utilize several other specifications to help mitigate econometric concerns that may bias the baseline results. We first use a propensity score matched sample in our DID model and find similar results, showing that differences in observable characteristics between treated and non-treated firms do not drive our findings. Next, recent studies have criticized the staggered DID method for using already-treated units as controls, leading to potentially biased estimations due to heterogeneous treatments (Baker, Larcker, and Wang, 2022; Goodman-Bacon, 2021). Following the extant studies, we use both the Callaway and Sant'Anna (2021) estimator and the stacked regression method suggested by Baker et al. (2022), and find our results to be robust. Last, we construct a set of counterfactual events by keeping the time distribution of event years and randomly matching them with the states that have never been affected by the shock. The distribution of coefficients from 1,000 runs of the falsification tests shows that our findings are unlikely to be observed by chance.

In cross-sectional tests, we evaluate our argument that reduced executive mobility is the mechanism behind the observed increase in pay gap 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 the subsample of firms belonging to states that tend to rely on CEOs hired from other firms or with higher skewness of CEO pay levels, or firms with founder CEOs. In these firms, VPs value outside employment options more and have a high ex ante tendency for mobility because within-firm promotion expectations are limited or across-firm movements are more rewarding. Further, the impact of enhanced enforceability is more pronounced in firms in which VPs have more significant outside job opportunities, i.e., in firms with younger VPs, more able managers, and in industries with lower product market concentration.

Running a rank-order tournament to find the best VP is not the only way for firms to select internal candidates for the CEO position. Pass-the-baton practices are another typical pattern.

In a pass-the-baton model, a firm can designate a person as the CEO's successor in advance and promote her when the incumbent CEO leaves (Vancil, 1987). Having a succession plan in place contributes to smoother CEO switches by reducing the possibility of forced turnover (Cvijanovic, Gantchev, and Hwang, 2022; Naveen, 2006) and impairs the value of competition among VPs. We find that the effects of increases in NCA enforceability are more pronounced in years right before CEO dismissals, when firms are more likely to adopt the tournament model rather than the pass-the-baton model.

We next switch our focus to the opposite change pattern, i.e., a reduction in NCA enforceability, to check whether the effect is symmetric. We note that we have no priors as to whether the effect should be symmetric or not. We find a negative but weaker effect on executive pay gaps, implying an asymmetric impact of changes in NCA enforceability. This would be consistent with prior evidence that firms are reluctant to reduce CEO pay (Dittmann, Maug, and Zhang, 2011) as well as recent findings that relate CEO pay arrangements to issues beyond incentives, such as, perception of fairness (Edmans, Gosling, and Jenter, 2022).

One important question to address is whether adjustments to executive pay gaps driven by shocks to NCA enforceability have implications on 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 et al., 2023; He and Wintoki, 2020; Shi, 2023). Arguably, firms can counteract these adverse effects by adjusting internal tournament incentives. We provide stock-market-based evidence which shows 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.

We interpret our findings through the framework of rank-order tournament incentives, but at first sight other explanations could also hold. One alternative interpretation is that an expanded pay gap is a manifestation of the excessive compensation extracted by a powerful CEO (Bebchuk, Cremers, and Peyer, 2011).[8] Stringent restrictions to executive labor mobility

[8]Entrenched CEOs may also be inclined to pursue a quiet life by, for example, offering more pecuniary benefits to VPs, which would make them more loyal followers. Therefore, CEOs can derive private benefits from

prevent firms from recruiting candidates whose skillsets best match their needs and this may give the incumbent CEO more power. Another common view is that the pay gap reflects talent differentials across corporate hierarchies rather than being an incentive tool (Mueller, Ouimet, and Simintzi, 2017; Terviö, 2008). If NCA shocks lead firms to implement changes in the composition of their executive teams, talent differentials among executives will also be affected. The last alternative explanation we consider relates to the CEO bargaining power argument (Kini et al., 2021). The intuition behind this argument is that affected CEOs may have stronger bargaining power compared to the VPs and claim more consideration for themselves than other executives, which creates a differential in the effects of non-compete enforcement shocks that favors CEOs. We run a battery of additional tests to rule out all the above competing explanations. Taken together, the executive pay gap changes following shifts in non-compete enforcement are likely to capture variations in tournament incentives.

To the best of our knowledge, our study is the first to provide direct evidence showing 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 analyzing the interplay between VPs' internal promotion and external employment opportunities is key to enhancing 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). Previous studies document that the executive pay gap substantially impacts real corporate outcomes (Kale et al., 2009; Kini and Williams, 2012). Therefore, it is important to understand its determinants better.

We also contribute to the related topic on the effects of labor mobility on managerial compensation (e.g., Chen et al., 2022; Kini et al., 2021). Kini et al. (2021) argue that CEOs receive higher compensation because NCAs raise their unemployment risks. In this paper,

saving effort in wage bargaining and improving social relations (Bertrand and Mullainathan, 2003; Cronqvist, Heyman, Nilsson, Svaleryd, and Vlachos, 2009). However, this prediction is at odds with our main findings.

however, we identify an alternative explanation for this increase in CEO compensation, which is based on VPs tournament incentives.

Lastly, a recent study by Johnson et al. (2021) finds that the strengthening of NCA enforceability exacerbates gender and racial salary gaps. We, instead, 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 firm value implications of this practice, complementing other studies on non-compete enforceability that provide inconsistent results regarding its effect on firm value (Anand et al., 2018; Bai et al., 2023; Garmaise, 2011; He and Wintoki, 2020). Our findings help reconcile the conflicting prior evidence concerning the real effects of executive mobility and provide insights for the ongoing debate about pay inequality.

2 Institutional background

Non-compete clauses embedded in a firm's employment contracts restrict signers from joining competitors if that could infringe upon the legitimate business of the firm 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 can 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 laws, 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. It is not only individuals suspected of violating NCAs who are vulnerable to lawsuits from their former employers, but also the new employers may be liable for possible monetary damages. At worst, the new employment relation could be ceased forcefully (Bai et al., 2023; Vanko, 2002).

Therefore, non-compete limitations for top executives can be far-reaching and not necessarily within the same industry or state (Bishara et al., 2015). Nonetheless, the real extent of

non-compete restrictiveness is determined by state-level enforceability, which is further determined by legislative actions or prior court rulings. Although non-compete laws vary considerably across different states and individuals may escape local regulations through cross-state movements (Marx, Singh, and Fleming, 2015), this appears not to be an optimal solution for executives' career developments, because many studies find that the 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. It is critical for firms to grasp the 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., 2023; He and Wintoki, 2020; Kini et al., 2021; Lin et al., 2022) consider answers to twelve questions to evaluate the level of NCA enforceability in a particular state and track substantial changes in enforceability. We follow the same empirical strategy and find that, during our sample period, the following nine states have experienced increases in NCA enforceability: 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 also record a weakening in enforceability in Texas in 1994, Louisiana in 2001, Oregon in 2008, New Hampshire in 2012, Illinois in 2013, and Kentucky in 2014. We present the empirical strategy and summary of the events in Appendix A.

Though some studies propose using a discrete enforceability index based on Garmaise's (2011) twelve questions as a test variable (e.g., counting the number of positive answers), we instead 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 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 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 multiple changes are in opposite directions, we set our test variable to one when the first event happens and re-set it to zero following the second event (Klasa, Ortiz-Molina, Serfling, and Srinivasan, 2018). In our sample, the average rate of VP turnover is 14.8% and, in Table OA.1, we observe that stronger (weaker) enforceability decreases (increases) mobility by about 29.7% (19.1%), indicating that the shifts in the NCA enforcement have similar impacts in terms of magnitude. We primarily focus on upward shifts in enforceability but also discuss the effects of downward shifts.

3 Methodology

3.1 Empirical methodology

To examine the impact of variations in NCA enforceability on the executive pay gap, we primarily estimate the following DID specification:

$$Ln\ (Pay\ Gap)_{i;t+1} = \quad + \quad NCA\ Enforceability_{t;s} + \quad \mathbf{X} + \ _i + \ _t + \ _s + " \qquad (1)$$

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

The dependent variable describes 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*, and also use alternative proxies and transformations in robustness tests. We measure all the dependent variables at $t+1$ to ease reverse causality

problems and in recognition that adjustments to executive pay are not instantaneous and inevitably happen with a lag.

The main test variable, $NCA\ Enforceability_{t;s}$, is a binary variable that equals one if state-level NCA enforceability has changed in the current or previous years, and zero otherwise. We follow Kale et al. (2009) and include a vector of controls, $\mathbf{X}$, related to CEO, firm, and industry characteristics that are expected to determine the size of the *Pay Gap*. Specifically, firm size, median industry-level 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 utility for VPs in each internal tournament depends on the size of the prize they obtain in case of promotion and the 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 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 Appendix B. We measure all the executive characteristics at year $t+1$ and firm financials or industry characteristics at year t. We do not discuss the expected signs of these controls here for brevity.

The baseline models include firm ($_i$) and year ($_t$) fixed effects (FEs) to control for time-invariant omitted firm characteristics and time-specific factors. Firm FE can also help capture the firm-level variations in NCA usage since this should be persistent over time (Chen et al., 2022). In addition, we control for the time-invariant state attributes with state FE ($_s$) because firms can 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.

3.2 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 any CEO or indicate the existence of multiple CEOs. BoardEx database helps us to fill or correct 42.2% of those observations. In particular, we identify CEOs by their "role name", considering variants of the term "CEO". Other non-CEO corporate executives 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. However, requiring firms to have at least two or three VPs does not affect our results.

ExecuComp modified its reporting format of executive compensation after the passage of FAS 123R, making the comparability between pre- and post-FAS 123R problematic. As discussed by Walker (2011), prior to FAS 123R, TDC1 fails to measure the ex-ante value of performance shares. In our main analysis, following prior studies (e.g., Focke et al., 2017; Gabaix et al., 2014), we adjust the pre-regulation data to make compensation data consistent across our whole sample period. That is, we firstly 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.

We are aware that the above method to adjust ExecuComp data is imperfect. An obvious limitation is that SHRTARG is often missing (Gabaix et al., 2014), which risks biasing our estimations. However, as discussed in Coles, Daniel, and Naveen (2014), all TDC1 components in ExecuComp except SALARY have changed definitions since the regulation change. Eliminating the differences between two reporting regimes is rather difficult to achieve. In the Online Appendix, we consider two alternative approaches to ease concerns. We follow the procedures by Kini and Williams (2012) to recalculate the values of stock option awards using the Black-Scholes model for the post-FAS 123R period and substitute the ExecuComp item,

OPTION_AWARDS_FV, in the same period. We also split the full sample depending on the data reporting formats to guarantee within-group comparability. The results are unaffected by these changes.

We compute the *Total Gap*, *ST Gap*, and *LT Gap* as previously discussed and exclude non-positive values from the baseline regression.[9,10] When *Total Gap* values are excluded, we argue that VPs are not substantially incentivized in terms of monetary compensation and also discard the same observations for both the *ST Gap* and the *LT Gap*.

Employment laws are typically applicable to the states where employees are working. Executives and managers are likely to locate in the states of their firms' headquarters (Bai et al., 2020; Dang, De Cesari, and Phan, 2021). Since Compustat only provides the latest data on firms' headquarters locations, we extract historical 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.[11] 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 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 1^{st} and 99^{th} percentiles.

Table 1 summarizes the descriptive statistics for the main variables used in our baseline models. The sample consists of 28,449 firm-year observations with 2,368 unique firms. The mean (median) value of the *Total Gap* is \$3.44 (\$1.97) million, which is larger than that reported in prior related studies (e.g., Kini and Williams, 2012; Kubick and Masli, 2016).

[9]Kini and Williams (2012) find that instances of negative executive pay gap are mostly because CEOs are also founders, who receive low compensation but have large equity stakes. A negative gap is also possible when the former CEO becomes a VP while continuing to receive a high compensation that is larger 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; (3) we replace the CEO's pay when smaller than VPs' 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.

[10]In around 5.7% of the observations the *LT Gap* is equal to zero, because ExecuComp reports that all executives get zero or the same amount of long-term incentives. These observations are less likely to reflect tournament dynamics and are, therefore, excluded.

[11]The information in 10-X Header Data comes from SEC EDGAR, which formally starts in 1994. For the period preceding this date, Bai et al. (2020) collect the headquarter location data from Moody's Manuals and Dun & Bradstreet's Million Dollar Directory. Over the period 1994 to 2003, there is a very significant overlap between the two datasets. We note that around 12.1% of firms have at least once relocated their headquarters to other states in our sample. The results are robust when excluding all the observations for these firms.

However, this pattern is not surprising considering the rapid growth of CEO pay in recent years. The distributions of variables are comparable to prior research (Acharya, Gabarro, and Volpin, 2021; Hasan et al., 2020; Hayes, Lemmon, and Qiu, 2012; Kale et al., 2009).

4 Empirical Results

4.1 Main results

4.1.1 Graphical analysis

Our baseline model employs a DID specification, so it is necessary to check if it satisfies the underlying assumption of parallel trends. We include leads and lags around the year of enhanced NCA enforceability and estimate the equation:

$$Ln(Pay\ Gap)_{i;t+1} = \quad + \sum_{k=\ 20;\ k\neq\ -1}^{k=21} \quad _k \quad D^k_{t;s} + \quad _i + \quad _t + \quad _s + \ ''$$ (2)

where dummy variables $D^k_{t;s}$ capture periods preceding, following, and contemporaneous to an event of increased NCA enforceability, and $_k$ is the parameter of interest in the graphical analysis of this section. The definitions of the other components in Eq. (2) are identical to those in Eq. (1). The vector of control variables is not included to mitigate the effects of the possible endogeneity of any control variable (Baker et al., 2022). The firm FEs $_i$ is included in the estimation, so that $_k$ reflects average within-firm variations over time.

Since our sample period is from 1993 to 2017 and given the timings of the studied events, we have a maximum of 20 years preceding and 21 years following any event. We do not bin or truncate any relative-time indicators to avoid contamination in the event-study specifications (Baker et al., 2022; Sun and Abraham, 2021). We omit the dummy indicating year -1 from Eq. (2) to avoid collinearity (Borusyak, Jaravel, and Spiess, 2022) and, therefore, take this period as the reference year.

Figure 1 plots the coefficients estimated from Eq. (2) for a 21-year-window around the shock. Consistent with the parallel trends assumption, the figure shows that the pay gap is not statistically different between treated and control firms before an enhanced enforceability

event. The non-existing pre-treatment effects indicate that differences in treated and control firms' inherent preferences cannot be the drivers of our findings.

4.1.2 Baseline results

Table 2 presents results for the baseline model of Eq. (1). All regressions are estimated using OLS with firm, year, and state fixed effects (FEs). Column (1) reports results for specifications on how enhanced enforceability affects the executive pay gap without time-varying covariates. In Column (2), we further add a comprehensive set of other variables following Kale et al. (2009). The results from both columns show that the positive coefficients on *NCA Enforceability Up* are highly significant, 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 amounting to 20.3% (= exp(0:185) 1), corresponding to an absolute increase of about $0.70 million (= $3:44 million 20:3%). The effects of other variables in Column (2) are generally in line with the empirical results or theoretical predictions by Kale et al. (2009) and, in particular, confirm the statistical and economic significance of firm size (Gabaix and Landier, 2008; Gabaix et al., 2014). Given that the interquartile change in *Ln (Total Asset)* leads to a $3.05 million (= $3:44 million [exp(0:286 2:22) 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 the *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, an average treated firm experiences an increase of about 5.7% in the *ST Gap* and about 20.6% in the *LT Gap*. Thus, the *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 about long-term incentives.

In the Online Appendix, we follow previous research and use several alternative measures of the executive pay gap. Table OA.2 indicates that our main finding does not differ for any of those measures. Overall, we posit that the enhanced NCA enforceability expands the relative pay gap between the CEO and the VPs.

4.1.3 Dynamic effects

We extend our analysis by examining the dynamic effects of an increase in NCA enforceability. In particular, we decompose the periods preceding and following this 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) in either three or four years in the future (*NCA Enforceability Up*$^{-3,-4}$); (ii) in either one or two years in the future (*NCA Enforceability Up*$^{-1,-2}$); (iii) in either the current or the previous year (*NCA Enforceability Up*$^{0,+1}$); (iv) either two or three years ago (*NCA Enforceability Up*$^{+2,+3}$); (v) at least four years ago (*NCA Enforceability Up*$^{>=+4}$). The regressions of Columns (2), (4), and (6) of Table 2 are re-estimated by replacing the test variable *NCA Enforceability Up* with this set of binary variables.

The results reported in Column (1) of Table 3 are generally consistent with the plotted trends in Figure 1, which show that the assumption of parallel trends holds, and the treated effects on the overall pay gap get stronger over time. We further examine the *ST Gap* and the *LT Gap*. The statistically significant effect of the increased NCA enforceability on the *ST Gap* is relatively transient and mainly exists in the first two periods and then becomes marginal. By contrast, the impact of the treatment on the *LT Gap* is persistent. The differences in timing are likely attributable to the nature of the different components in the executive pay package. Changes in long-term pay, consisting of stock and option grants, usually take longer to materialize and produce significant effects relative to changes in short-term pay, that is, salary and bonus. Consistent with Table 2, variations in the *LT Gap* are the main drivers of the enlarged *Total Gap* that follows an enhanced enforceability in NCA.

4.2 Econometric concerns

4.2.1 Matched sample

Treated and control firms should be similar along dimensions that pertain to executive pay. The treatment effects may otherwise suffer from biases caused by omitted or unobserved factors. We consider a propensity score matching approach to match treated observations with control observations and reduce the systematic differences across observable factors.

Recall that in Figure 1, we show that the enhanced NCA enforceability has lasting effects on the executive pay gap until year +10. Therefore, we set a window from t 10 to $t + 10$ around each treatment event in year t for the treated firms. These firms are required not to change their home states during the window to avoid opportunistic relocations (Bai et al., 2020). The treated group consists of firms headquartered in states that have undergone an increase in enforceability in year t. The control group comprises firm-years of (i) firms that are never treated, or (ii) firms that are ever treated in a year that is outside of the event window.[12] A matched sample is identified based on one-to-one matching (with replacement) between treated and control groups. For all candidate firms, propensity scores are estimated using the covariates in Eq. (1) for the year prior to the increase in enforceability in logit models. In the matched sample, the control firm for a particular treated firm is the non-treated firm from the same year as the treated one with the most similar propensity score. We apply a caliper of 5‰ to exclude matches with scores that are not close enough. We retain firms with available data for at least one year in both the pre- and post-treatment periods. If an ever-treated firm is matched as a control firm, the firm-years that fall into the period of t 10 around its own treatment are excluded. Overall, we obtain 191 matched pairs. Panel A of Table 4 demonstrates that the means of observable characteristics for the treated and control groups are quite similar after matching. Using the matched sample, in Panel B, we continue to find a significant growth of pay gap for the treated relative to control firms following an increase in NCA enforceability.

[12] In untabulated analysis, we find that the results using propensity score matching are robust to restricting the control group to consist of only never-treated firms.

4.2.2 Alternative specifications

Staggered DID applications have been questioned in recent years owing to the frequent use of already-treated units as control firm-years. The comparison between an earlier- and later-treated group potentially suffers from an estimation bias when treatment effects are heterogeneous, for example in relation to the time the treatment effect takes to affect the outcome variable (Baker et al., 2022; Goodman-Bacon, 2021; Sun and Abraham, 2021). This issue is relevant in our context since the timing of the treatment effect of enhanced enforceability might vary across a long sample period. In the baseline regression, however, we note that 81.7% of observations are never treated, helping reduce the likelihood of bias (Baker et al., 2022).

We provide two more pieces of evidence to alleviate any possible concerns. First, in Table OA.3, we put a restriction on the window, $t \quad 1$, $t \quad 3$, $t \quad 5$, or $t \quad 10$ around the treatment for the treated firms and show that our results persist. By truncating the treatment effects to vary in a relatively short timeframe, there is a reduction in the likelihood that previously-treated firms act as future controls and lead to biased estimations. Second, we follow Baker et al. (2022) and use both the Callaway and Sant'Anna (2021) (CS) estimator and stacked specifications to evaluate our baseline results. Consistent with the event-study estimates in Figure 1, the test window is set from $t \quad 10$ to $t + 10$ for each specific event in year t. We use "not yet treated" or "never treated" firms as clean comparisons and consider firm- and year-cohort FEs in the estimation.[13] Unlike the "never treated" group, the "not yet treated" group also includes observations for firms that will be treated in the future. Last, we stack the events together and calculate an average effect for all the events. Panels C and D of Table 4 report the results of regressions with the CS estimator and the stacked regression, respectively. The coefficients estimated from the alternative methodologies are similar to those in the baseline regression, confirming the robustness of our main findings.

[13] In the CS estimator, we do not include state FEs as in Eq. (1) to avoid perfect collinearity. We thank Fernando Rios-Avila for clarifications on this issue. In the stacked regression, we find similar results when additionally controlling for state-cohort FEs.

4.2.3 Falsification tests

A remaining concern, however, is that our results may be driven by other unobserved shocks that coincide with the increase in NCA enforceability. We follow Dang et al. (2021) and implement 1,000 runs of a placebo test based on a counterfactual increase in enforceability. In each iteration, we keep the actual distribution of the event years while randomly assigning to them the states that have never been through the enhanced enforceability during our sample period (without replacement), and then re-estimate Eq. (1). Given the random assignment of the treatments, the 1,000 estimated coefficients are expected not to be significant in most cases. In Figure 2, the distribution of the probability density of the placebo estimates is centered around zero, and the coefficients are far from the true estimated value.[14] Thus, the results in the baseline regressions are unlikely to be driven by chance.

4.2.4 Other robustness checks

In the Online Appendix, we present a series of tests that establish the robustness of our inference. All tests are informed by the extant literature; however, we explain the rationale behind each test only in the Online Appendix to conserve space here. First, our results are not sensitive to using different coding schemes to capture shifts in enforceability; to the choice of sample window; or to the existence of potential expectations as to future legislative actions driven by corporate lobbying activities (Table OA.4). Second, 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 (Table OA.5). Furthermore, we control for additional FEs to alleviate concerns for other sources of omitted variables that may relate to the pay gap. Specifically, we consider FEs to capture the time-varying heterogeneity across industries, firm size quartiles, census tract regions, or states of incorporation, and also firm age FEs (Bai et al., 2020; Dang et al., 2021) (Table OA.6). Fourth, 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 and verify that the

[14] In untabulated results, we repeat the procedure several times, implementing trials for up to 2,000 times. Because the placebo states are randomly assigned, the estimated results vary each time. However, the fraction of the placebo coefficients that are larger than the actual coefficient is consistently smaller than 2%.

effects of enforceability on the pay gap, in general, get stronger after excluding the years just after CEO turnover events (Table OA.7). Fifth, and last, our results are robust if we (i) examine the full sample including financial and utility firms; (ii) exclude observations for the financial crisis period 2008-2009; (iii) restrict the sample to companies with at least two or three VPs; (iv) exclude the sample of CEOs identified through BoardEx; (v) examine only the sample with available historical headquarter data from 10-X Header Data (from 1994 to 2018); (vi) exclude relocated firms; (vii) exclude states that have experienced changes in NCA enforceability during the period from 1980 to 1993; (viii) exclude firms headquartered in Florida and/or Ohio that constitute 46.6% of the treated sample; (ix) construct industry-level financials (*Industry Homogeneity* and *Median Industry Gap*) based on the SIC-2 or NAICS-3 industry classification (Table OA.8).

4.3 Mechanisms underlying the baseline findings

4.3.1 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 impact of shocks to NCA enforceability on the pay gap should depend on the VPs' external opportunities and incentives pre-shock, also in relation to internal promotion prospects. Adjustments to the pay gap should be less significant when the 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 level since executive mobility often happens within the same state (Ma et al., 2020; Yonker, 2017). We expect firms belonging to states in which CEOs tend to be promoted internally to rely less on the external labor market and thus be less sensitive to mobility shocks.[15] We calculate the fraction of insider CEOs at year t in each state and trisect the sample. As shown in Columns (1) and (2) of Panel A,

[15] A potential concern is that the likelihood of promoting insider CEOs is boosted by the changes in NCA enforceability that make external hiring expensive. However, existing evidence shows that firms' hiring of CEOs is not affected by NCA frictions (Chen et al., 2022; Cziraki and Jenter, 2022). In our sample, we find similar results (untabulated), that is, either an increase or decrease in enforceability does not affect CEO hirings.

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

Similarly, a VP can become a CEO in the 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 indifferent to changes in NCA enforceability. We calculate the skewness of CEO pay at year 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. In this test, we exclude the ever-relocated firms that are subject to opportunistic actions, however, the results are not sensitive to this restriction. 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 when the enforceability gets stronger than the bottom tercile sample.[16]

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 the founder. Founder CEOs are rarely forced to leave their firm given their significant voting power on average. 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. CEOs older than 65 years old or with interim positions are excluded in this test because they are likely to depart shortly. In Columns (5) and (6) of Panel A, Table 5, we find that the increase in pay gap for firms with founder CEOs is significantly larger than that for the non-founder sample, showing that VPs working for a founder require more internal tournament incentives to compensate them for the lost outside opportunities.

We next consider the cross-sectional variation in three variables that reflect outside job opportunities. First, career concerns are associated with the age of executives. Younger VPs usually have more career prospects and are more sensitive to the lost outside options caused by the new rules. Career concerns are weaker when a VP is older because the short prospective career decreases the value of opportunities in the external job market (Gibbons and Murphy,

[16] We are interested in the state-level variations in the proportion of insider CEOs and skewness of CEO pay because the executive labor market is known to be geographically segmented. In untabulated results, however, we find that our results are qualitatively similar when we compute the above two variables at the industry level and use them as alternative conditional variables.

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, when working in less monopolistic product markets, executives can easily transfer their existing industry-specific skills to the 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.[17] *High Ability* equals one if managerial ability is above the sample median. As it is practically difficult to distinguish the ability of an individual executive, we use the proxy from Demerjian, Lev, and McVay (2012) that models the ability of a whole top management team.[18] *Low HHI* equals one if the HHI of sales within an industry is below the sample median. We access the HHI data developed through textual analysis of company filings by Hoberg and Phillips (2016).[19]

In this analysis, we add state-year FEs to remove potential time-varying omitted variables at the state level. We focus on the interaction term because the enforceability binary variable is absorbed. Panel B of Table 5 shows 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.

4.3.2 Tournament incentives versus pass-the-baton mechanisms

The purpose for a company to run a rank-order tournament is to select an internal talent as the future CEO. In contrast, pass-the-baton is an approach in which a successor is designated in advance and can get promoted immediately when the incumbent CEO leaves (Vancil, 1987). While the tournament implies an intensified competition among VPs, pass-the-baton reflects a stylized schedule. In our baseline regressions, we control for *Propensity of Relay Succession* and observe its negative impact on the executive pay gap. A potential reason is that firms

[17] We identify an individual VP's age using various sources, including ExecuComp, BoardEx, 10-K files, and company's official websites.

[18] The dataset is available on Peter Demerjian's personal website.

[19] The dataset is available on Hoberg-Phillips Data Library.

do not need to spend time selecting "winners" from an internal tournament when there are already designated heirs.

An implication of the above argument is that the impact of an NCA shock on the executive pay gap should be larger in firms that tend to adopt a tournament model rather than a pass-the-baton mechanism. Distinguishing between these two types is challenging since a succession plan may not be made public (Cvijanovic et al., 2022). Also, CEO succession plans can lead to alternative outcomes since they do not necessarily end up with an internal succession and can result in external talent searching activities.

However, previous studies suggest that having a succession plan in place is helpful to reduce the possibility of forced CEO turnover and signals a smoother management transition (Cvijanovic et al., 2022; Naveen, 2006). In the spirit of this argument, we select a sub-sample of observations for the year of or before a CEO's turnover. We use the dataset compiled by Jenter and Kanaan (2015) and Peters and Wagner (2014) to split this set of observations into forced and voluntary turnovers.[20] In Panel C of Table 5, we note that the positive impact of *NCA Enforceability Up* on the pay gap is considerably larger right before forced turnovers compared to voluntary ones. This result is consistent with our argument that firms that rely on tournament incentives to motivate VPs and internally select future CEO candidates are particularly likely to enlarge the executive pay gap when VPs' potential labor mobility declines.

4.4 Asymmetric effects

We next turn our focus to declines in NCA enforceability. We replicate the baseline regressions in Table 2 and add the binary variable *NCA Enforceability Down* to capture state-level shocks that weaken NCA enforcement. In Panel A of Table 6, we observe a statistically significant negative effect of this test variable on the executive pay gap. However, the economic impacts on *Total Gap* and *LT Gap* caused by the downward shocks are generally smaller than the upward ones, suggesting asymmetric effects from variations in state-level enforceability.

In addition, we repeat the propensity score matching procedures as in Section 4.2.1 for the reduced enforceability shocks and obtain 140 matched pairs. Panel B of Table 6 shows that

[20] The dataset is available on WRDS.

the means of covariates for the matched sample are very close. After matching, the coefficients on *NCA Enforceability Down* are still negative but no longer distinguishable from zero. This finding is consistent with firms being reluctant to reduce CEO pay (Dittmann et al., 2011) as well as recent findings that issues beyond incentives, such as, perception of fairness, affect CEO pay levels (Edmans et al., 2022).

4.5 Firm value implications

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, 2020; Shi, 2023). At the same time, a properly designed compensation system helps develop managerial talents in the internal executive market and create better performance (Kale et al., 2009; Lazear and Rosen, 1981). Such effects are potentially more evident in less-mobile labor markets in which executives are more likely to be active participants in rank-order tournaments. Thus, a trade-off emerges, and the impact of the change in the executive pay gap may counteract the negative impact in firm value from 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 negative variations in pay gap. To remove some common trends, we adjust a year's pay gap by subtracting the median value in that year. We consider the change in the pay gap either from year $t \quad 1$ to t or from year $t \quad 1$ to $t + 1$. Since 1996 is the earliest treated year in our sample, the newly constructed dataset of CRSP monthly returns starts from 1997. In each 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 one in those with reduced 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.[21] As shown in Table 7, the portfolios often generate significant positive alphas across different holding

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

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 perform significantly better on the stock market.

5 Does pay gap capture tournament incentives?

So far, we have assumed that the executive pay gap creates a tournament prize that is meant to incentivize and reward VPs and possibly compensate them for any lost outside employment opportunity. Nonetheless, other theories, mechanisms, and explanations have been put forward 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.

5.1 CEO power

Shocks that impose mobility restrictions likely hinder the efficiency of talent allocation (Anand et al., 2018; Marx et al., 2015), and shrink the talent pool while increasing firms' challenges to replace incumbent CEOs. Such shocks can be exploited by powerful CEOs to boost their own pay which will result in a widened pay gap (Bebchuk et al., 2011). We next list some tests that help us evaluate whether a CEO power mechanism can explain our findings.

5.1.1 Channels

First, we examine the effects of increased NCA enforceability on CEO and VP pay separately. Though we find that the executive pay gap expands, it may imply three scenarios: (1) increased pay for CEO and VPs (to different degrees); (2) increased pay for CEO but decreased pay for VPs; (3) decreased pay for CEO and VPs (to different degrees). Existing studies show mixed findings on the relation between NCA enforceability and the level of CEO pay (Garmaise, 2011; Kini et al., 2021). More importantly, if CEO power plays a prominent role in driving the pay gap, CEOs should merely set a higher pay for themselves, while other executives might hardly get compensated. In this case, we expect to see an insignificant, or even negative, impact on the level of average VP compensation.

Because the determinants of executive pay level and pay gap may not be the same, we follow the model settings in recent studies that specifically examine the executive pay level (e.g., Focke et al., 2017) and replace some control variables in the baseline model. Panel A of Table 8 corroborates the findings of Kini et al. (2021) who argue that the affected CEOs ask for higher compensations to reflect higher unemployment risks. We further find that VP pay levels also benefit from tightened mobility in the labor market, though VP pay growth is lower than that of the CEO. The evidence indicates that CEOs are not the only beneficiaries of restricted mobility in terms of higher compensation.

5.1.2 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 top five most highly-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.[22] We use the pay slice as the dependent variable, re-run the baseline regressions, and report the results in Columns (1) and (2) of Panel B, Table 8. The coefficient on *NCA Enforceability Up* is not significant if controls are included. Therefore, 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 et al. (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

[22] Assume the average VP's pay is P and CEO's pay is P ($> 1; P > 0$) at time 0. The one-period increase rates in their compensations are and respectively. Thus, at time 1, an average VP's pay is $(1+)P$ and CEO pay is $(1+) P$. 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 , , and . For example, if > 0 and $\frac{1}{} < < 1$, the pay gap increases but the pay slice decreases. This may be one reason why the correlation between the two proxies is relatively low.

stage, we use the residual from stage 1 as the dependent variable and re-estimate the baseline specifications. In Columns (3) and (4) of Panel B, Table 8 we continue to find a significant positive impact of increased NCA enforceability on the pay gap.

5.1.3 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 factors that deter takeovers. We use the takeover index developed by Cain, McKeon, and Solomon (2017) for the presence and extent of hostile takeovers.[23] We take the inverse of the original index so that a larger value of the variable is potentially associated with more entrenched CEOs due to lack of market discipline. The interaction between the transformed index and the enforceability test variable has a negative and statistically significant coefficient in Panel C of Table 8. Thus, when CEOs are likely to be more powerful and entrenched, the effect of an NCA shock on the pay gap actually becomes weaker, a result at odds with the explanation that powerful CEOs extract excessive compensation.

5.2 Talent differentials

The dispersion among executive pay can also be set to reflect talent differentials (Mueller et al., 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 is able to acquire new skills quickly following the reforms, which could affect the distribution of talent across executives. Second, studies suggest that firms have worse performance when experiencing a strengthened enforceability (Anand et al., 2018; He and Wintoki, 2020). If managerial skills are evaluated based on firm performance and talent differentials drive pay gaps, we should probably not observe a post-

[23]The dataset is available on Stephen B. McKeon's personal website.

shock increase in pay gap given that the CEO is ultimately responsible for poor performance. To validate the conjectures, we follow the previous literature and consider three time-varying ability proxies: managerial ability score, adjusted ROA, and adjusted stock return (Demerjian et al., 2012; Rajgopal et al., 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 9.

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 manager FEs to best describe the managers' latent traits and skills (Graham, Li, and Qiu, 2012). Specifically, we construct a pay gap proxy at the VP's 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 FEs allow us to control for the time-invariant difference within each CEO-VP pair, while the inclusion of the latter reflects the fact that such difference could vary across firms. In Panel B of Table 9, 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.

5.3 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 would argue for more considerations to compensate them for the incurred personal costs. As a result, executives can obtain higher compensation (Kini et al., 2021). In previous sections, we indeed find that the effects of the NCA shocks on the pay gap are more pronounced when

the top management members have more 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) are willing to accept them only in return for substantially increased pay. If the disutility attached to an NCA and/or the bargaining power are 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 that are plausibly unrelated to the VPs'. If the bargaining activities of the CEO primarily drive the pay gap changes we report, we expect substantial heterogeneity in our main results that depend on these variables.

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 generalist if the value of a generalist index is higher than the sample annual median.[24] 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 the average compensation growth over 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 that a CEO often departs to take the same job in another firm offering a better compensation, the gap between her pay and that of the top-paid CEO in a given year is helpful to capture the focal CEO's external tournament incentives. Following Coles et al. (2018), we calculate the pay differential between the focal CEO and the second-top-paid CEO in a specific industry. We do this because the first-top-paid compensation is often caused by certain transitory events, such as the exercise of accumulated options. To account for labor market segmentation, we also calculate a local pay gap, that is, a proxy for

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

the focal CEO and the second-top-paid CEO in a specific firm's headquarters state (Ma et al., 2020). The concept is further generalized by computing the pay gap between the focal CEO and the universal second-top-paid one in each sample year. CEOs whose pay gaps are higher than the respective sample medians are those with more opportunities for external promotion.

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 a higher pay than subordinate executives. We identity the less synergistic firms as those with a diversified business (with more than one business segments), where different divisions likely operate independently (Edmans et al., 2013). CEOs could have more synergy impacts in non-diversified firms, and also a larger bargaining power.

Last, as documented by Kini et al. (2021), a new CEO coming from outside the firm or after the dismissal of the former CEO is in a more advantageous position to ask for more benefits from her employer. Thus, such CEO should receive a comparatively higher pay, which could result in a larger pay gap. Like before, we identify if the new CEO is an outsider according to ExecuComp and BoardEx, and if the prior CEO is fired following Jenter and Kanaan (2015) and Peters and Wagner (2014).

In untabulated analyses, we find that the effect of increased NCA enforceability on the pay gap is stronger when the CEOs are close to retirement, consistent with the notion that tournament incentives are very much needed in these cases (Kale et al., 2009). Such effect becomes significantly weaker when firms are non-diversified (Shi et al., 2016), in contrast to the possibility that CEOs get compensated for their larger bargaining power due to larger synergy potentials. Besides, no other instances present significant differences. These findings are not consistent with the argument that the expanded pay gaps we observe are driven by the bargaining power of CEOs.

6 Conclusion

We study how exogeneous 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 turnover and, more importantly, an increase in the pay gap. The positive effect can be observed irrespective of whether short-term or long-term pay components are considered, but the lasting and persistent changes are mainly found for long-term pay. 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, a more skewed distribution of CEO pay, for firms with founder CEOs, younger VPs, and more capable top executives, as well as in industries with lower product market concentration. VPs in firms with these characteristics are expected to value external job opportunities more, and when these are curtailed, they require more significant internal tournament incentives.

We document that the potential adverse effects on firm performance due to increases in NCA enforceability vary significantly between firms with or 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 managerial labor market.

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Figures and Tables

Figure 1: The effect of increases in NCA enforceability on executive pay gaps

This figure shows the effect of a state-level increase in the enforceability of non-compete agreements (NCAs) on the executive pay gap. We report the lags and leads of a binary variable capturing an increase in NCA enforceability (*NCA Enforceability Up*) over a 21-year period around the event. The graph presents the coefficients from year -10 to year +10 based on a regression of executive pay gaps (*Ln (Total Gap)* measured at $t+1$) on such lags and leads, controlling for firm, year, and headquarter state fixed effects. The dummy for year -1 is omitted in the regression, as in Eq.(2). The *y*-axis plots coefficients for each dummy variable. The *x*-axis shows the year relative to the increase in NCA enforceability. The dashed lines are the 90% confidence intervals of the estimated coefficients, which are calculated from standard errors clustered by headquarter state.

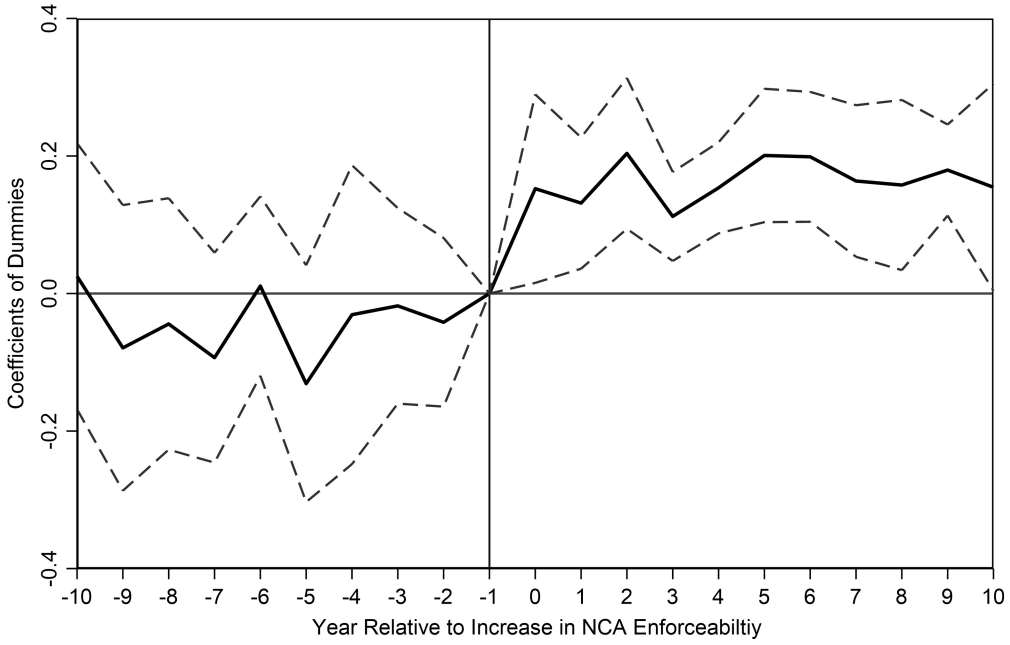

Figure 2: Distribution of estimated coefficients from placebo tests

This figure shows the effect of a state-level increase in the enforceability of non-compete agreements (NCAs) on the executive pay gap in falsification tests. The graph presents the probability density of the estimated coefficients on the pseudo increase in NCA enforceability (*NCA Enforceability Up*) from 1,000 placebo tests. In each iteration, the real distribution of the event years is kept, and the never-treated states are randomly assigned into each of those years (without replacement). The vertical line represents the true coefficient from the baseline regression (the coefficient of *NCA Enforceability Up* of Column (2) in Table 2).

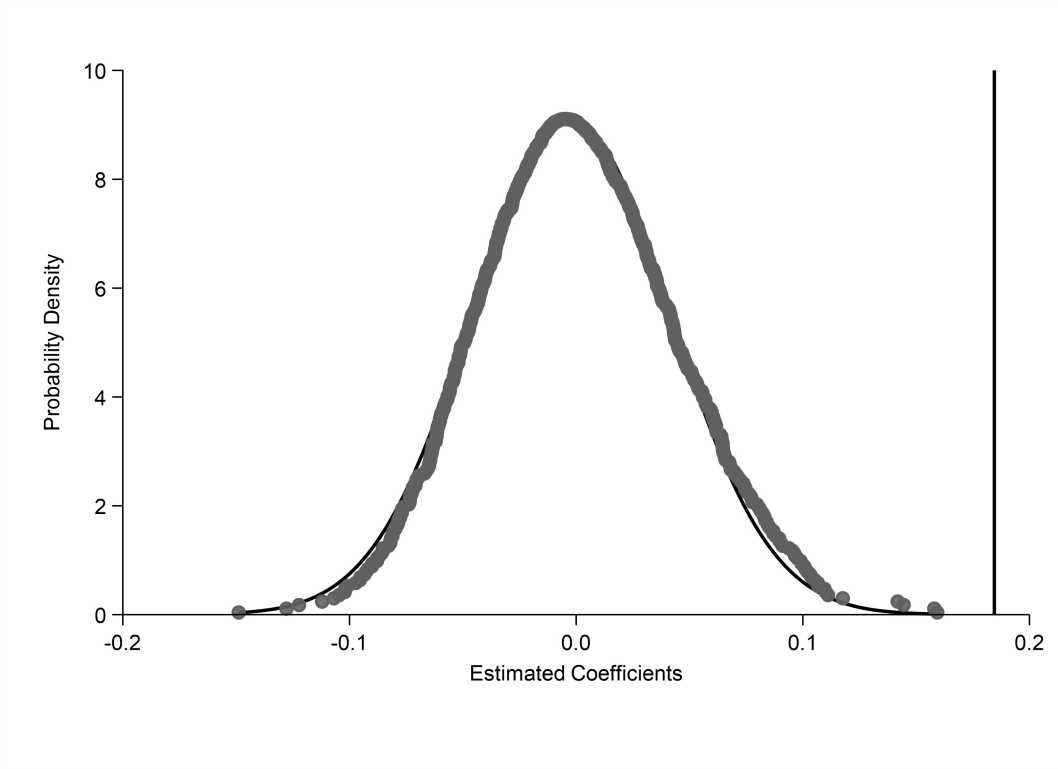

Table 1: Summary statistics

This table reports the descriptive statistics (including number of 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) and is from 1993-2018. Detailed variable definitions can be found in Appendix B.

	Obs.	Mean	Lower Quartile	Median	Upper Quartile
Compensation variables					
Total Gap (in 000s)	28,449	3,441.16	786.08	1,966.32	4,361.88
ST Gap (in 000s)	27,822	625.83	269.13	445.49	701.37
LT Gap (in 000s)	24,597	2,885.18	604.29	1,692.89	3,900.65
Median Industry Gap (in 000s)	28,449	2,528.21	824.65	1,741.98	3,566.68
Median Industry ST Gap (in 000s)	27,822	520.10	297.42	436.43	596.16
Median Industry LT Gap (in 000s)	24,597	2,097.91	564.21	1,400.93	3,096.13
Executive characteristics					
CEO Age	28,449	55.72	51.00	56.00	60.00
CEO Tenure	28,449	7.07	2.00	5.00	10.00
New CEO	28,449	0.10	0.00	0.00	0.00
CEO Is Insider	28,449	0.67	0.00	1.00	1.00
Retiring CEO	28,449	0.10	0.00	0.00	0.00
Duality	28,449	0.55	0.00	1.00	1.00
No. of VPs	28,449	4.77	4.00	5.00	5.00
Propensity of Relay Succession	28,449	0.54	0.00	1.00	1.00
CFO Is VP	28,449	0.89	1.00	1.00	1.00
Firm/Industry Characteristics					
Ln (Total Asset)	28,449	7.26	6.11	7.15	8.33
Industry Homogeneity	28,449	0.11	0.07	0.10	0.14
Stk. Return Volatility	28,449	0.45	0.30	0.40	0.54
No. of Segments	28,449	2.53	1.00	2.00	4.00

Table 2: Baseline regressions

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) and is from 1993-2018. The dependent variables reflect alternative definitions of the pay gap and are 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. 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)*, *Ln (CEO Tenure)*) are recorded in year $t+1$ while other controls in year t. All models include firm, year, and headquarter-state fixed effects. Detailed variable definitions can be found in Appendix B. The t-stats in parentheses are computed using standard errors clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to the significant levels of 1%, 5%, and 10% respectively.

	Ln (Total Gap)		Ln (ST Gap)		Ln (LT Gap)	
	(1)	(2)	(3)	(4)	(5)	(6)
NCA Enforceability Up	0.184***	0.185***	0.057**	0.053**	0.214***	0.198***
	(5.06)	(5.13)	(2.18)	(2.31)	(3.20)	(2.85)
New CEO		0.056		-0.082***		0.113***
		(1.37)		(-4.52)		(3.23)
CEO Is Insider		-0.168***		-0.142***		-0.195***
		(-5.89)		(-7.74)		(-5.70)
Industry Homogeneity		0.168*		-0.177*		0.275**
		(1.76)		(-1.87)		(2.22)
Retiring CEO		-0.095**		-0.047*		-0.089*
		(-2.32)		(-1.88)		(-1.81)
Duality		0.097***		0.095***		0.128***
		(4.15)		(4.54)		(3.94)
No. of VPs		0.022***		0.026***		0.024***
		(2.83)		(3.85)		(3.18)
Propensity of Relay Succession		-0.054***		-0.058***		-0.052***
		(-3.68)		(-4.92)		(-3.21)
Ln (Median Industry Gap)		0.074***				
		(3.80)				
Ln (Median Industry ST Gap)				0.218***		
				(7.53)		
Ln (Median Industry LT Gap)						0.081***
						(5.99)
CFO Is VP		0.043		0.027		0.030
		(1.47)		(1.15)		(0.88)
Ln (CEO Age)		-0.291***		0.310***		-0.511***
		(-3.06)		(2.80)		(-4.62)
Ln (CEO Tenure)		0.065***		0.052***		0.048***
		(3.77)		(4.45)		(2.73)
Ln (Total Asset)		0.286***		0.079***		0.299***
		(15.42)		(5.49)		(10.07)
Stk. Return Volatility		0.049		-0.070*		0.209***
		(0.93)		(-1.69)		(3.17)
No. of Segments		-0.005		0.005		-0.003
		(-0.55)		(0.85)		(-0.32)
Observations	28,449	28,449	27,822	27,822	24,597	24,597
R-squared	0.620	0.637	0.595	0.619	0.627	0.643
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 3: Dynamic effects

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) and is from 1993-2018. The regressions of Columns (2), (4), and (6) of Table 2 are re-estimated by replacing the test variable *NCA Enforceability Up* with a set of binary variables that identify periods preceding and following an increase in NCA enforceability. These binary variables are equal to one if the firm is headquartered in a state that (i) experiences an increase in enforceability in either three or four years in the future (*NCA Enforceability Up*$^{-3,-4}$); (ii) experiences an increase in enforceability in either one or two years in the future (*NCA Enforceability Up*$^{-1,-2}$); (iii) has experienced an increase in enforceability in either the current or previous year (*NCA Enforceability Up*$^{0,+1}$); (iv) has experienced an increase in enforceability either two or three years ago (*NCA Enforceability Up*$^{+2,+3}$); (v) has experienced an increase in enforceability four or more years ago (*NCA Enforceability Up*$^{>=+4}$). All models include firm, year, and headquarter-state fixed effects. Detailed variable definitions can be found in Appendix B. The *t*-stats in parentheses are computed using standard errors clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to the significant levels of 1%, 5%, and 10% respectively.

	Ln (Total Gap)	Ln (ST Gap)	Ln (LT Gap)
	(1)	(2)	(3)
NCA Enforceability Up$^{-3,-4}$	0.021	0.030	0.055
	(0.20)	(0.66)	(0.63)
NCA Enforceability Up$^{-1,-2}$	0.023	0.006	0.071
	(0.41)	(0.16)	(0.79)
NCA Enforceability Up$^{0,+1}$	0.177***	0.085**	0.168*
	(2.85)	(2.02)	(2.00)
NCA Enforceability Up$^{+2,+3}$	0.187***	0.057*	0.255***
	(3.95)	(1.99)	(3.67)
NCA Enforceability Up$^{>=+4}$	0.212***	0.050	0.256***
	(4.16)	(1.44)	(2.71)
Observations	28,449	27,822	24,597
R-squared	0.637	0.619	0.643
Controls from Table 2	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes

Table 4: Matched sample and alternative specifications

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) and is from 1993-2018. The regressions of Columns (1) and (2) of Table 2 are re-estimated using different econometric specifications. *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. In Panels A and B, a propensity-score-matching method is used to estimate the coefficient on *NCA Enforceability Up*. A matched sample is obtained based on one-to-one matching (with replacement) between treated firms (firms headquartered in states with an increase in NCA enforceability in year t) and non-treated firms (never-treated firms in the same year t, or treated outside a window of t 10). Treated firms are required not to relocate during the test window. For all treated and non-treated firms, propensity scores are estimated using the values of the covariates in Eq. (1) for the year t 1 prior to a change in NCA enforceability. In the matched sample, the control firm for a particular treated firm is the non-treated firm from the same year t as the control firm with the most similar propensity score (within a caliper of 5‰). Both the treated and control firms are required to have available data in at least one year in both the pre- and post-treatment periods.If an ever-treated firm is matched as a control firm, the firm-years that overlap the period of t 10 around its own treatment are excluded. Panel A reports the means of the covariates for 191 pairs of matched treated and control firms, together with t-stats of tests of equality of means between the two types of firms. Panel B reports regression results based on the matched sample. The dependent variable is *Ln (Total Gap)* measured at $t+1$. The t-stats in parentheses are computed using standard errors clustered at the headquarter-state level. The coefficients on constants are not reported. In Panel C, the dependent variable is *Ln (Total Gap)* measured at $t+1$. The coefficient on *NCA Enforceability Up* is estimated using Callaway and Sant'Anna (2021) estimator for a test window of t 10 (consistent with Figure 1), using never or not-yet-treated observations as "clean" controls and without or with covariates. The regressions with the Callaway and Sant'Anna estimator include firm-cohort and year-cohort fixed effects. The z-stats in parentheses are computed using standard errors clustered at the headquarter-state level. In Panel D, the dependent variable is *Ln (Total Gap)* measured at $t+1$. The coefficient on *NCA Enforceability Up* is estimated using the stacked-regression method by Baker et al. (2022) for a test window of t 10 (consistent with Figure 1), using never or not-yet-treated observations as "clean" controls and without or with covariates. The stacked regressions include firm-cohort and year-cohort fixed effects. The t-stats in parentheses are computed using standard errors clustered at the headquarter-state level. Detailed variable definitions can be found in Appendix B. ***, **, * correspond to the significant levels of 1%, 5%, and 10% respectively.

Panel A: Characteristics of means for matched treated and control firms

	Treated	Control	Dif. (Control-Treated)	t-stats
Propensity Score	0.066	0.066	-0.000	(-0.00)
New CEO	0.110	0.147	0.037	(1.07)
CEO Is Insider	0.691	0.733	0.042	(0.90)
Industry Homogeneity	0.113	0.114	0.001	(0.19)
Retiring CEO	0.105	0.058	-0.047	(-1.69)
Duality	0.555	0.534	-0.021	(-0.41)
No. of VPs	4.838	4.885	0.047	(0.34)
Propensity of Relay Succession	0.581	0.508	-0.073	(-1.44)
Ln (Median Industry Gap)	7.356	7.328	-0.027	(-0.30)
CFO Is VP	0.827	0.869	0.042	(1.14)
Ln (CEO Age)	4.012	3.998	-0.014	(-1.01)
Ln (CEO Tenure)	1.632	1.557	-0.074	(-0.77)
Ln (Total Asset)	7.080	7.048	-0.032	(-0.21)
Stk. Return Volatility	0.479	0.467	-0.012	(-0.51)
No. of Segments	2.419	2.503	0.084	(0.50)

Panel B: Regression using matched sample

	Ln (Total Gap)	
	(1)	(2)
NCA Enforceability Up	0.194***	0.194***
	(3.21)	(3.65)
Observations	4,902	4,902
R-squared	0.631	0.646
Controls from Table 2	No	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	Yes

Panel C: Regression using Callaway and Sant'Anna (2021) estimator

	Ln (Total Gap)			
	Never treated as controls		Not yet treated as controls	
	(1)	(2)	(3)	(4)
NCA Enforceability Up	0.175***	0.184***	0.172***	0.180***
	(2.74)	(2.79)	(2.70)	(2.74)
Controls from Table 2	No	Yes	No	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes

Panel D: Stacked regressions

	Ln (Total Gap)			
	Never treated as controls		Not yet treated as controls	
	(1)	(2)	(3)	(4)
NCA Enforceability Up	0.194***	0.191***	0.191***	0.188***
	(5.91)	(5.66)	(5.83)	(5.50)
Observations	106,213	106,213	109,988	109,988
R-squared	0.642	0.654	0.643	0.655
Controls from Table 2	No	Yes	No	Yes
Firm-Cohort FE	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes

Table 5: Mechanisms underlying baseline findings

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) and is from 1993-2018. The regression of Column (2) of Table 2 is re-estimated by using sub-sets of observations or adding interaction terms for the test variable *NCA Enforceability Up*. The dependent variable is *Ln (Total Gap)* measured at $t+1$. *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. In Panel A, Columns (1) and (2) report the results for the top and the bottom tercile of the fraction of insider CEOs for the state the firm belongs to, Columns (3) and (4) report the results for the top and the bottom tercile 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. The founders are also required not to be retiring or interim CEOs. All the models include firm, year, and state fixed effects. Panel A also reports the p-values of F-stats testing the differences in coefficients on *NCA Enforceability Up* between the respective partitioned subsamples. In Panel B, *NCA Enforceability Up* is interacted with measures of average VP age, managerial ability score (Demerjian et al., 2012), and product market HHI (Hoberg and Phillips, 2014). *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 median; *Low HHI* equals one if the HHI is below the sample median. All the models include firm and state-year fixed effects. In Panel C, Columns (1) and (2) report the results for the year of and the year before CEO forced or voluntary turnover. CEO forced turnover is defined following Jenter and Kanaan (2015) and Peters and Wagner (2014). Panel C also reports the p-values of F-stats testing the differences in coefficients on *NCA Enforceability Up* between the respective partitioned subsamples. Detailed variable definitions can be found in Appendix B. The t-stats in parentheses are computed using standard errors clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to the significant levels of 1%, 5%, and 10% respectively.

Panel A: State insider, skewness of CEO pay, and founder CEOs

	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	0.083**	0.278***	0.416***	0.098**	0.404***	0.177***
	(2.54)	(3.69)	(6.29)	(2.37)	(3.47)	(3.51)
p-value (equal coefficient estimates)	(0.01)		(0.00)		(0.02)	
Observations	7,810	8,045	7,827	7,966	1,495	23,709
R-squared	0.704	0.640	0.656	0.705	0.673	0.639
Controls from Table 2	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Average age of VPs, managerial ability, and product market HHI

		Ln (Total Gap)		
		(1)	(2)	(3)
NCA Enforceability Up	Young VPs	0.052**		
		(2.40)		
Young VPs		0.005		
		(0.34)		
NCA Enforceability Up	High Ability		0.040**	
			(2.04)	
High Ability			0.050***	
			(3.39)	
NCA Enforceability Up	Low HHI			0.070**
				(2.35)
Low HHI				-0.012
				(-0.64)
Observations		28,244	27,937	27,641
R-squared		0.654	0.653	0.655
Controls from Table 2		Yes	Yes	Yes
Firm FE		Yes	Yes	Yes
State-Year FE		Yes	Yes	Yes

Panel C: Sample before CEO turnover

	Ln (Total Gap)		
	Forced Turnover		Voluntary Turnover
	(1)		(2)
NCA Enforceability Up	0.642**		0.290***
	(2.59)		(2.89)
p-value (equal coefficient estimates)		(0.05)	
Observations	1,124		4,398
R-squared	0.726		0.664
Controls from Table 2	Yes		Yes
Firm FE	Yes		Yes
Year FE	Yes		Yes
State FE	Yes		Yes

Table 6: Asymmetric effects

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) and is from 1993-2018. *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. In Panel A, the dependent variables reflect alternative definitions of the pay gap and are measured in year $t+1$. Panel A also reports the p-values of F-stats testing whether the sum of the coefficients on *NCA Enforceability Up* and *NCA Enforceability Down* is significantly different from zero in the same model. In Panels B and C, a propensity-score-matching method is used to estimate the coefficient on *NCA Enforceability Down*. A matched sample is obtained based on one-to-one matching (with replacement) between treated firms (firms headquartered in states with a decrease in NCA enforceability in year t) and non-treated firms (never-treated firms in the same year t, or treated outside a window of t 10). Treated firms are required not to relocate during the test window. For all treated and non-treated firms, propensity scores are estimated using the values of the covariates in Eq. (1) for the year t 1 prior to a change in NCA enforceability. In the matched sample, the control firm for a particular treated firm is the non-treated firm from the same year t as the control firm with the most similar propensity score (within a caliper of 5‰). Both the treated and control firms are required to have available data for at least one year in both the pre- and post-treatment periods. If an ever-treated firm is matched as a control firm, the firm-years that overlap the period of t 10 around its own treatment are excluded. Panel B reports the means of the covariates for 140 pairs of matched treated and control firms, together with t-stats of tests of equality of means between the two types of firms. Panel C reports regression results based on the matched sample. The dependent variable is *Ln (Total Gap)* measured at $t+1$. Detailed variable definitions can be found in Appendix B. The t-stats in parentheses are computed using standard errors clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to the significant levels of 1%, 5%, and 10% respectively.

Panel A: Asymmetric effects

	Ln (Total Gap)		Ln (ST Gap)		Ln (LT Gap)	
	(1)	(2)	(3)	(4)	(5)	(6)
NCA Enforceability Up (a)	0.185***	0.185***	0.058**	0.053**	0.214***	0.198***
	(5.09)	(5.10)	(2.28)	(2.40)	(3.21)	(2.84)
NCA Enforceability Down (b)	-0.082**	-0.060**	-0.056***	-0.049**	-0.077**	-0.049
	(-2.63)	(-2.28)	(-4.05)	(-2.67)	(-2.12)	(-1.49)
p-value ($a+b=0$)	(0.04)	(0.01)	(0.94)	(0.88)	(0.07)	(0.05)
Observations	28,449	28,449	27,822	27,822	24,597	24,597
R-squared	0.620	0.637	0.595	0.619	0.627	0.643
Controls from Table 2	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Characteristics of means for matched treated and control firms

	Treated	Control	Dif. (Control-Treated)	t-stats
Propensity Score	0.074	0.074	0.000	(0.01)
New CEO	0.057	0.071	0.014	(0.49)
CEO Is Insider	0.693	0.736	0.043	(0.79)
Industry Homogeneity	0.097	0.092	-0.005	(-0.78)
Retiring CEO	0.071	0.121	0.050	(1.42)
Duality	0.650	0.650	0.000	(0.00)
No. of VPs	4.793	4.807	0.014	(0.10)
Propensity of Relay Succession	0.493	0.550	0.057	(0.96)
Ln (Median Industry Gap)	7.255	7.404	0.149	(1.27)
CFO Is VP	0.857	0.864	0.007	(0.17)
Ln (CEO Age)	4.023	4.032	0.010	(0.61)
Ln (CEO Tenure)	1.774	1.720	-0.054	(-0.52)
Ln (Total Asset)	7.395	7.666	0.271	(1.31)
Stk. Return Volatility	0.395	0.387	-0.008	(-0.44)
No. of Segments	2.629	2.757	0.129	(0.55)

Panel C: Regression using matched sample

	Ln (Total Gap)	
	(1)	(2)
NCA Enforceability Down	-0.136	-0.108
	(-1.29)	(-1.16)
Observations	3,062	3,062
R-squared	0.673	0.690
Controls from Table 2	No	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	Yes

Table 7: 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. We consider the change in the pay gap net of the annual median value of the gap. Panel A (B) considers the change from t 1 to t ($t+1$). Observations for all the treated firms are retained starting from the year following that of an increase in NCA enforceability to build portfolios of treated stocks. In each monthly portfolio, we take a long (short) position in the subsample with (without) 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 Fama-French five factors. The t-stats in parentheses are computed using the Newey-West estimators for 4 lags. ***, **, * correspond to the significant levels of 1%, 5%, and 10% respectively.

Panel A: Pay gap change from year t 1 to year t

	Equally Weighted Portfolio (1)	Weighted Average Portfolio (2)
Alpha for holding 12 months	1.140%*	1.365%*
	(1.77)	(1.85)
Alpha for holding 24 months	0.907%**	0.931%*
	(2.07)	(1.84)
Alpha for holding 36 months	0.913%***	0.842%**
	(2.61)	(2.09)
Observations	264	

Panel B. Pay gap change from year t 1 to year $t+1$

	Equally Weighted Portfolio (1)	Weighted Average Portfolio (2)
Alpha for holding 12 months	0.837%	1.214%*
	(1.13)	(1.87)
Alpha for holding 24 months	1.015%**	1.081%**
	(2.23)	(1.99)
Alpha for holding 36 months	0.832%**	1.180%***
	(2.59)	(2.83)
Observations	252	

Table 8: Alternative mechanisms: CEO power

This table reports the effect of a state-level increase in the enforceability of non-compete agreements (NCAs) on the CEO and VP compensation, alternative measures of executive pay gap, and on pay gap conditional on the likelihood of hostile takeovers. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) and is from 1993-2018. *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. In Panel A, the dependent variables are CEO compensation (*Ln (CEO Pay)*) and VP compensation (*Ln (VP Pay)*) measured in year $t+1$. All controls are measured in year t. The set of controls include firm, year, and state fixed effects. In Panels B and C, we re-estimate the specification of Column (2) of Table 2 with some variations. In Panel B, we consider alternative measures of executive pay gap (*CEO Pay Slice* and *Gap Residual*) in year $t+1$. In Panel C, the dependent variable is *Ln (Total Gap)* measured at $t+1$. We include the interaction between *NCA Enforceability Up* and the inverse of the hostile takeover index created by Cain et al. (2017), while controlling for the index and firm and state-year fixed effects. Detailed variable definitions can be found in Appendix B. The t-stats in parentheses are computed using standard errors clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to the significant levels of 1%, 5%, and 10% respectively.

Panel A. CEO and VP compensation

	Ln (CEO Pay)		Ln (VP Pay)	
	(1)	(2)	(3)	(4)
NCA Enforceability Up	0.139***	0.143***	0.080***	0.083***
	(4.68)	(5.28)	(2.95)	(3.63)
Industry Homogeneity		0.141*		0.091
		(1.85)		(1.10)
ROA		0.195***		0.166***
		(3.77)		(4.47)
Ln (Total Asset)		0.329***		0.291***
		(24.59)		(33.19)
Market-to-Book		0.127***		0.112***
		(18.23)		(18.38)
Leverage		-0.250***		-0.233***
		(-3.72)		(-4.85)
Ln (Median Industry CEO Pay)		0.051***		
		(3.79)		
Ln (Median Industry VP Pay)				0.055***
				(4.19)
Cash Ratio		-0.014		-0.032
		(-0.17)		(-0.59)
Dividend Payer		-0.056***		-0.031
		(-2.90)		(-1.65)
Stk. Return Volatility		0.053		0.091***
		(1.35)		(3.86)
Observations	26,764	26,764	26,764	26,764
R-squared	0.729	0.758	0.771	0.803
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes

Panel B. Alternative pay gap measures

	CEO Pay Slice		Gap Residual	
	(1)	(2)	(3)	(4)
NCA Enforceability Up	0.008*	0.008	0.154***	0.179***
	(1.71)	(1.62)	(4.22)	(4.92)
Observations	27,570	27,570	28,424	28,424
R-squared	0.341	0.360	0.553	0.581
Controls from Table 2	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes

Panel C. Hostile takeover

		Ln (Total Gap)
NCA Enforceability Up	Inv. Takeover Index	-0.011***
		(-3.72)
Inv. Takeover Index		0.006
		(1.62)
Observations		23,665
R-squared		0.663
Controls from Table 2		Yes
Firm FE		Yes
State-Year FE		Yes

Table 9: 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) and is from 1993-2018. *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. In Panels A and B we re-estimate the specification of Column (2) of Table 2 with some variations. In Panel A, the dependent variable is *Ln (Total Gap)* measured at $t+1$. We add controls for managerial ability: 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 $t+1$. We control for combinations of firm, year, CEO-VP, or CEO-VP-firm fixed effects. Detailed variable definitions can be found in Appendix B. The t-stats in parentheses are computed using standard errors clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to the significant levels of 1%, 5%, and 10% respectively.

Panel A. Managerial ability

	Ln (Total Gap)			
	(1)	(2)	(3)	(4)
NCA Enforceability Up	0.185***	0.183***	0.182***	0.183***
	(5.00)	(5.24)	(5.06)	(5.14)
Managerial Ability Score	0.316***			0.196**
	(3.67)			(2.39)
Adj. ROA		0.656***		0.503***
		(12.33)		(8.46)
Adj. Stk. Return			0.171***	0.133***
			(10.80)	(8.10)
Observations	28,001	28,449	28,449	28,001
R-squared	0.636	0.639	0.639	0.639
Controls from Table 2	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes

Panel B. Pay gap at the firm-VP-level

	Ln (VP Total Gap)			
	(1)	(2)	(3)	(4)
NCA Enforceability Up	0.069**	0.074**	0.070**	0.074**
	(2.21)	(2.48)	(2.24)	(2.51)
Observations	92,640	92,640	92,575	92,575
R-squared	0.765	0.769	0.765	0.768
Controls from Table 2	No	Yes	No	Yes
Firm FE	Yes	Yes	No	No
Year FE	Yes	Yes	Yes	Yes
CEO-VP FE	Yes	Yes	No	No
CEO-VP-Firm FE	No	No	Yes	Yes

Appendix A

A.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.

A.2 Events that led to the 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 Appendix A.1.

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	*Fi eld v. Premier Dealership Servs.*
Kentucky	2014	*Creech v. Brown*

Note that we code that Illinois experienced decreased enforceability in 2013, but do not follow Ewens and Marx (2018) who label Illinois as one 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 business interests (for instance, *Outsource Int'l, Inc. v. Barton* 1999), while starting in 2011 the restrictions were relaxed by introducing more flexible standards. However, the Court also "expressly observe(s) that appellate court precedent for the past three decades remains intact".

As indicated by Garmaise's twelve 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". The answer to this question for Illinois was already "yes" before 2011. As a result of the Supreme Court ruling, the practical enforceability in Illinois exceeded the threshold even more since then. Importantly, even though 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.4.

We also notice some other inconclusive opinions raised in recent studies when identifying the variations of NCA enforceability, although the methodologies are all consistent with Garmaise (2011). In *Poynter Investments v. Century Builders of Piedmont* 2010, 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, but meanwhile, made it easier for firms to obtain a preliminary injunction against former employees. Given the unclear implications of the Supreme Court's decisions, we follow Ewens and Marx (2018) and treat South Carolina as a control state in the baseline models. Besides, Bai et al. (2023) and Ewens and Marx (2018) denote that Oregon experienced a decreased enforceability in 2008, while Kini et al. (2021) claim a change in the opposite direction from the same event. Untabulated results show our main results are not affected by treating the non-compete status in South Carolina, or Oregon, as increase, decrease, or no-change.

Appendix B

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 FAS 123R, total compensation data is adjusted following Walker (2011) and Gabaix et al. (2014). (*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 product of SHRTARG and stock price, restricted stock granted, and options granted. After the FAS 123R, LT compensation is the total value of the non-equity incentive pay, fair value of stocks, and fair value of options granted (we hypothesize that the cash payment of formulaic multiyear plans in the non-equity incentive pay is nonnegligible). (*Source*: ExecuComp)
Median Industry Gap	The median *Total Gap* for firms in the same the Fama-French 30 industry, year, and quartile of *Total Asset* (excluding the focal firm). (*Source*: ExecuComp)
Median ST Industry Gap	The median *ST Gap* for firms in the same the Fama-French 30 industry, year, and quartile of *Total Asset* (excluding the focal firm). (*Source*: ExecuComp)
Median LT Industry Gap	The median *LT Gap* for firms in the same the Fama-French 30 industry, year, and quartile of *Total Asset* (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 ST Industry Gap)	Natural logarithm of *Median ST Industry Gap*.
Ln (Median LT Industry Gap)	Natural logarithm of *Median LT Industry Gap*.
Ln (CEO Pay)	Natural logarithm of the total compensation of the CEO. (*Source*: ExecuComp)
Ln (VP Pay)	Natural logarithm of the average compensation of the VPs. (*Source*: ExecuComp)
Ln (Median Industry CEO Pay)	Natural logarithm of the median *CEO Pay* for firms in the same the Fama-French 30 industry, year, and quartile of *Total Asset* (excluding the focal firm). (*Source*: ExecuComp)
Ln (Median Industry VP Pay)	Natural logarithm of the median *VP Pay* for firms in the same the Fama-French 30 industry, year, and quartile of *Total Asset* (excluding the focal firm). (*Source*: ExecuComp)

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Variable	Definition
CEO Pay Slice	The total compensation of the CEO over the aggregated compensation of the top five most highly-paid executives. (*Source*: ExecuComp)
Gap Residual	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 the *CEO Tenure* is in the distribution of the top quartile in the same industry-year, zero otherwise. Industry is defined as the Fama-French 30 industry. (*Source*: ExecuComp, BoardEx)
Ln (VP Total Gap)	Natural logarithm of the difference between the focal VP's total compensation and that of her CEO in the focal firm. (*Source*: ExecuComp)
Executive characteristics	
CEO Age	Age of CEO. (*Source*: ExecuComp)
CEO Tenure	Number of years as CEO. (*Source*: ExecuComp, BoardEx)
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, 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)
Retiring CEO	Dummy that equals one if CEO Age is 65 or more, zero otherwise. (*Source*: ExecuComp)
Duality	Dummy that equals one if the CEO is also chairman in the same firm, 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)
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 is incomplete 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)
Firm/Industry characteristics	
Ln (Total Asset)	Natural logarithm of total assets. (*Source*: Compustat)
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)
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)
ROA	Net income over total assets. (*Source*: Compustat)

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Variable	Definition
Leverage	Long-term debt plus debt in current liabilities, scaled by total assets. (*Source*: Compustat)
Market-to-Book	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)
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)
Variables used in Online Appendix	
VP mobility	Dummy that equals one if the focal VP 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 the 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	$1 + \frac{1}{n} \quad \frac{2}{n^2\bar{y}}(y_1 + 2y_2 + \cdots + ny_n)$, where n is the number of executives (including the CEO), $y_1 \cdots y_n$ 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 FAS 123R, total compensation is adjusted following Kini and Williams (2012). (*Source*: ExecuComp)

Appendix C Online Appendix

OA.1 Mobility effects of non-compete enforceability

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 et al., 2020), it is necessary to check if the shifts in enforcement constitute valid shocks to VP mobility in our studied sample window. 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 not anymore at the same firm at year $t+1$ relative to t. Similar to Islam, Rahman, Sen, and Zein (2021), we do not require an executive to re-appear at another sample firm because they may move to firms outside S&P (without coverage in ExecuComp). We exclude the departures of executives aged above 65 that are likely to be caused by retirements. In our sample, around 14.8% of VPs terminate their role at the focal firm in the following year.

In Table OA.1, we use a linear probability model to gauge the effects of changes in NCA enforceability on executive turnover. We find that increases and decreases in NCA enforceability lead to effects in opposite directions. Stronger (weaker) enforceability decreases (increases) mobility by about 29.7% (= 0:044=0:148) (18.9% (= 0:028=0:148)). Therefore, we confirm that the ability of executives to join external tournaments is indeed affected by the enforceability shocks.

OA.2 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 that 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 Coe - cient*); relative annual rank of total gap (*Rank (Total Gap)*). We also follow the procedures to recalculate the Black-Scholes values for the stock options in the post-FAS 123R period (details

discussed below) and build an adjusted version of the executive pay gap (*Ln (Total Gap) Adj.*). The proxies of *Median Industry Gap* are all re-calculated in line with the respective dependent variables. Table OA.2 indicates our main finding does not differ when using the alternative measures of the executive pay gap.

In the main text, we follow the approach by Walker (2011) and Gabaix et al. (2014) to adjust the pre-reform data and get the proxy of *Ln (Total Gap)*. We next follow the procedures by Kini and Williams (2012) to recalculate the values of stock option awards using the Black-Scholes model for the post-reform period for an alternative proxy, *Ln (Total Gap) Adj.*. For fiscal years before (after) 2006, all ExecuComp firms report the compensation data using the old (new) reporting format. In 2006, a small fraction of firms still appears with the old format. We also calculate the Black-Scholes values for those firms. The Black-Scholes model to value an executive stock option per share, P, is as follows:

$$P = Se^{\ dT}N(d_1) \quad Xe^{\ rT}N(d_2)$$

$$d_1 = \frac{ln(\frac{S}{X}) + (r \quad d + \frac{^2}{2})T}{p\overline{T}}$$

$$d_2 = \frac{ln(\frac{S}{X}) + (r \quad d \quad \frac{^2}{2})T}{p\overline{T}}$$

Following Coles, Daniel, and Naveen (2006), Hayes et al. (2012), Kini and Williams (2012), and the routines of ExecuComp, we estimate the inputs as follows:

S, underlying stock price, measured as the market price at end of fiscal year.

d, expected future dividend yield, calculated as the average of the dividend yields over the previous three years.

T, 70% of the nominal option term, which is the difference between the actual expiration date and 1st July (assumed grant date) of the grant year.

X, exercise price.

r, risk-free rate, measured as the average yield on a seven-year term U.S. Treasury bond.

, expected stock return volatility, measured as the volatility over the previous five years.

The correlation between our calculated Black-Scholes values and ExecuComp reported val-

ues for options (ExecuComp item: OPTION_AWARDS_FV) is 93.6% pre-regulation, indicating that our procedures of estimation are comparable to ExecuComp's. Therefore, we can ensure the consistency of the re-estimated data across the sample period.

OA.3 Sample surrounding the increase in NCA enforceability

In Table OA.3, we restrict the window to $t \quad 1$, $t \quad 3$, $t \quad 5$, or $t \quad 10$ around the event year t for the treated companies. The test serves two purposes. First, as discussed in the main body of the paper, the estimation of staggered DID effects may be biased because it uses the earlier-treated observations as controls for the later-treated ones (Baker et al., 2022; Goodman-Bacon, 2021; Sun and Abraham, 2021). This issue may be significant in our setting, because the timing of the treatment effect of enhanced enforceability varies across a long sample period. By truncating the treatment effects to vary in a short timeframe, the possibility that the already treated sample acts as future controls is reduced to a large extent. Second, setting a short test window is helpful to exclude confounding factors that are correlated with the shifts in non-compete enforcements. We show that the new results are consistent with the baseline regressions.

OA.4 Different coding schemes

In Table OA.4, we alter the coding schemes for enhanced NCA enforceability in four ways.

First, as discussed in Appendix A, in our baseline tests, we do not treat Illinois as a treated state in 2011 because we argue that, according to Garmaise's twelve questions, the relevant court case does not move state-level enforceability above the threshold and substantially increases it. Here, we modify the coding by labelling Illinois as a state that has enhanced the enforcement in 2011. After 2013, the enforceability again decreases, so the indicator goes back to zero. In Column (1), our results persist with this coding modification.

Second, in our baseline model we focus on the first occurrence of variations of non-compete status, which 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 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). One side benefit 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. Their occurrences may be associated with state-level political conditions or lobbying activities (Klasa et al., 2018). We only retain the treated states where the enhanced NCA enforceability is caused by court rulings, which is unlikely to be anticipated by firms. In Column (4), after dropping the observations for Florida and Idaho, our results are not affected.

Finally, we directly use the NCA Index from Bai et al. (2020). We use Bai et al.'s version of the NCA enforcement score because their data period overlaps with ours. Using the discrete NCA Index as the test variable, the model is not set as a standard DID, but the results remain qualitatively similar in Column (5).

OA.5 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.5.

First, the expected utility for each VP from a tournament depends on the prize size and the likelihood of prevailing in the tournament (Kale et al., 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 et al. (2006) to compute delta and vega. These variables are included as controls in Columns (1), (2), and (3) and the findings for *NCA Enforceability Up* remain robust.

Second, Abernethy, Dong, Kuang, Qin, and Yang (2022) 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., 2022). Prospector-type firms need CEOs to be more authoritative and dominating and make more timely decisions. Therefore, those firms would like to award more compensation and also larger executive pay gaps to their CEOs than the defender-type ones (Abernethy et al., 2022). To account for this dimension that could confound our findings, we follow Abernethy et al. and construct an indicator of a firm's strategic priority through six accounting numbers, including the 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. All the indicators are averaged for a two-year rolling window and then ranked into quintiles in each industry-year. A measure of a firm's strategy is built by summing all the ranks, with higher value of the measure representing a higher propensity to pursue prospector-type strategies. The measure is included in the regression of Column (4) in which we observe that our main findings are unaffected.

Finally, in Column (5) we further control for a set of accounting variables that are frequently examined as determinants of executive pay level in existing studies (e.g., Focke et al., 2017), including *ROA*, *Market-to-Book*, *Leverage*, *Cash Ratio*, and *Dividend Payer*, while in Column (6) we consider a specification in which all the additional controls mentioned above

are included. Overall, in all the columns of Table OA.5, we find that the explanatory power of non-compete shocks is robust.

OA.6 Additional FEs

In Table OA.6, we control for a set of high-dimensional FEs to mitigate concerns for omitted variables that may relate to the executive pay gap. We consider the time-varying heterogeneity across industries (Bai et al., 2020) (Column (1)), firm size quartiles (Column (2)), four census tract regions (Column (3)), or states of incorporation (Dang et al., 2021) (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 finally control for all these FEs (Column (6)). Some of the control variables in Eq. (1) are absorbed due to collinearity, but our variable of interest, *NCA Enforceability Up*, is significantly positive in all cases.

OA.7 CEO turnover

In Table OA.7, we verify if the effect of enforceability on the pay gap is still present when the existing CEO departs. We remove the observations that coincide with CEO turnover at year t (Column (1)), the two years since turnover (Column (2)), or the five years since 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, in general, get stronger after excluding the sample in relation to CEO turnover events.

OA.8 Additional robustness tests

In Table OA.8, we examine a battery of additional tests, including: (i) examine the full sample including financial and utility firms (Column (1)); (ii) exclude observations for the financial crisis period 2008-2009 (Column (2)); (iii) restrict the sample to companies with at least two or three VPs (Columns (3) and (4)); (iv) exclude the sample of CEOs identified

through BoardEx (Column (5)); (v) examine only the sample with available historical headquarter data from 10-X Header Data (from 1994 to 2018) (Column (6)); (vi) exclude relocated firms (Column (7)); (vii) exclude states that have experienced changes in NCA enforceability during the period from 1980 to 1993 (Columns (8), (9), and (10)); (viii) exclude firms headquartered in Florida and/or Ohio that constitute 46.6% of the treated sample (Columns (11), (12), and (13)); (ix) construct industry-level financials based on the SIC-2 or NAICS-3 industry classification (Columns (14) and (15)).

Table OA.1: Executive mobility

This table reports the effect of a state-level increase in the enforceability of non-compete agreements (NCAs) on the VP mobility. The sample covers the S&P ExecuComp firms (financial and utility firms are excluded) and is from 1993-2018. The dependent variable, *VP Mobility*, is a mobility indicator, which takes the value of one if the focal executive is not at the same firm at year $t+1$ relative to t. The departures of executives aged above 65 that are likely to be caused by retirements 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, industry-year, and region-year fixed effects. This table also reports the p-values of F-stats testing whether the sum of the coefficients on *NCA Enforceability Up* and *NCA Enforceability Down* is significantly different from zero in Column (3). Detailed variable definitions can be found in Appendix B. The t-stats in parentheses are computed using standard errors clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to the significant levels of 1%, 5%, and 10% respectively.

	VP Mobility		
	(1)	(2)	(3)
NCA Enforceability Up (a)	-0.044**		-0.044**
	(-2.06)		(-2.03)
NCA Enforceability Down (b)		0.028**	0.027**
		(2.03)	(2.02)
No. of VPs	0.034***	0.034***	0.034***
	(15.86)	(16.03)	(16.00)
Propensity of Relay Succession	0.002	0.002	0.002
	(0.56)	(0.52)	(0.57)
ROA	-0.061**	-0.062**	-0.061**
	(-2.07)	(-2.09)	(-2.07)
Leverage	0.088***	0.088***	0.088***
	(4.22)	(4.27)	(4.28)
Market-to-Book	-0.018***	-0.018***	-0.018***
	(-5.53)	(-5.50)	(-5.53)
Cash Ratio	0.012	0.014	0.012
	(0.61)	(0.69)	(0.59)
Stk. Return Volatility	0.034**	0.033**	0.034**
	(2.66)	(2.64)	(2.67)
Ln (Total Asset)	-0.044***	-0.044***	-0.044***
	(-5.67)	(-5.65)	(-5.67)
Ln (Firm Age)	0.111***	0.111***	0.112***
	(4.57)	(4.58)	(4.59)
GDP Growth	-0.161	-0.147	-0.157
	(-1.67)	(-1.50)	(-1.64)
Unemployment	0.003	0.002	0.003
	(0.68)	(0.57)	(0.70)
p-value ($a+b=0$)			(0.53)
Observations	116,960	116,960	116,960
R-squared	0.362	0.362	0.362
Executive FE	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes
Region-Year FE	Yes	Yes	Yes

Table OA.2: Alternative measures

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) and is from 1993-2018. The regression of Column (2) of Table 2 is re-estimated by using different dependent variables measured at $t+1$. *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, year, and headquarter-state fixed effects. Detailed variable definitions can be found in Appendix B. The t-stats in parentheses are computed using standard errors clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to the significant levels of 1%, 5%, and 10% respectively.

	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	0.176***	0.196***	0.014***	0.035***	0.183***
	(4.93)	(3.97)	(3.21)	(4.35)	(4.49)
Observations	28,392	25,295	28,449	28,449	28,449
R-squared	0.665	0.606	0.414	0.637	0.627
Controls from Table 2	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes

Table OA.3: Sample surrounding the increase in NCA enforceability

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) and is from 1993-2018. The dependent variable is *Ln (Total Gap)* measured at $t+1$. *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. The regression of Column (2) of Table 2 is re-estimated by truncating the windows of 1, 3, 5, or 10 years around the event year t for treated companies. All models include firm, year, and headquarter-state fixed effects. Detailed variable definitions can be found in Appendix B. The t-stats in parentheses are computed using standard errors clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to the significant levels of 1%, 5%, and 10% respectively.

	Ln (Total Gap)			
	t 1	t 3	t 5	t 10
	(1)	(2)	(3)	(4)
NCA Enforceability Up	0.122***	0.140***	0.189***	0.200***
	(2.74)	(4.07)	(3.28)	(5.12)
Observations	23,913	24,799	25,582	27,141
R-squared	0.642	0.641	0.639	0.637
Controls from Table 2	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes

Table OA.4: 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) and is from 1993-2018. The dependent variable is *Ln (Total Gap)* measured at $t+1$. The regression of Column (2) of Table 2 is re-estimated by using different coding schemes of the variations in NCA enforcement. All models include firm, year, and headquarter-state fixed effects. Detailed variable definitions can be found in Appendix B. The t-stats in parentheses are computed using standard errors clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to the significant levels of 1%, 5%, and 10% respectively.

	Ln (Total Gap)				
	Coding Illinois as treated	Pre-FAS 123R	Post-FAS 123R	Excluding Florida & Idaho	NCA Index
	(1)	(2)	(3)	(4)	(5)
NCA Enforceability Up	0.156***	0.240***	0.121**	0.184***	
	(3.52)	(3.27)	(2.28)	(4.78)	
NCA Index					0.060**
					(2.42)
Observations	28,449	12,314	16,042	27,298	28,449
R-squared	0.637	0.648	0.699	0.638	0.637
Controls from Table 2	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes

Table OA.5: 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) and is from 1993-2018. The dependent variable is *Ln (Total Gap)* measured at $t+1$. *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. The regression of Column (2) of Table 2 is re-estimated by controlling for additional variables, including *Ln (VP Pay)*, *Ln (CEO Delta)*, *Ln (VP Delta)*, *Ln (CEO Vega)*, *Ln (VP Vega)* measured at $t+1$, *Strategy* (Abernethy et al., 2022), and accounting variables, including *ROA*, *Market-to-Book*, *Leverage*, *Cash Ratio*, and *Dividend Payer*, measured at t. All models include firm, year, and headquarter-state fixed effects. Detailed variable definitions can be found in Appendix B. The t-stats in parentheses are computed using standard errors clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to the significant levels of 1%, 5%, and 10% respectively.

	Ln (Total Gap)					
	VP Pay	CEO and VP Delta	CEO and VP Vega	Strategy	Accounting variables	All
	(1)	(2)	(3)	(4)	(5)	(6)
NCA Enforceability Up	0.162***	0.139***	0.174***	0.203***	0.179***	0.177***
	(4.88)	(4.16)	(4.95)	(5.09)	(4.93)	(4.67)
Observations	28,449	25,842	23,285	23,575	26,764	18,190
R-squared	0.659	0.658	0.645	0.653	0.656	0.700
Controls from Table 2	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes

Table OA.6: 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) and is from 1993-2018. The dependent variable is *Ln (Total Gap)* measured at $t+1$. *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. The regression of Column (2) of Table 2 is re-estimated using different fixed effects. Detailed variable definitions can be found in Appendix B. The t-stats in parentheses are computed using standard errors clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to the significant levels of 1%, 5%, and 10% respectively.

	Ln (Total Gap)					
	(1)	(2)	(3)	(4)	(5)	(6)
NCA Enforceability Up	0.152***	0.182***	0.143***	0.148***	0.180***	0.110**
	(4.83)	(5.01)	(4.18)	(3.00)	(5.20)	(2.13)
Observations	28,446	28,449	28,449	28,255	28,449	28,252
R-squared	0.650	0.640	0.639	0.649	0.639	0.666
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	No
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	No	No	No	No	Yes
Size-Year FE	No	Yes	No	No	No	Yes
Region-Year FE	No	No	Yes	No	No	Yes
State Inc.-Year FE	No	No	No	Yes	No	Yes
Firm Age FE	No	No	No	No	Yes	Yes

Table OA.7: 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) and is from 1993-2018. The dependent variable is *Ln (Total Gap)* measured at $t+1$. *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 remove the observations that coincide with CEO turnover at year t (Column (1)), the two years since turnover (Column (2)), or the five years since turnover (Column (3)). We also follow Jenter and Kanaan (2015) and Peters and Wagner (2014) to identify forced CEO turnover. Following these classifications, we remove firms that have ever fired CEOs (Column (4)). All models include firm, year, and headquarter-state fixed effects. Detailed variable definitions can be found in Appendix B. The t-stats in parentheses are computed using standard errors clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to the significant 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 ever fired CEOs
	(1)	(2)	(3)	(4)
NCA Enforceability Up	0.173***	0.185***	0.263**	0.195***
	(4.07)	(4.25)	(2.54)	(4.11)
Observations	25,566	22,653	11,582	18,236
R-squared	0.659	0.671	0.717	0.663
Controls from Table 2	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes

Table OA.8: 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) and is from 1993-2018. The dependent variable is *Ln (Total Gap)* measured at $t+1$. *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, year, and headquarter-state fixed effects. Detailed variable definitions can be found in Appendix B. The t-stats in parentheses are computed using standard errors clustered at the headquarter-state level. The coefficients on constants are not reported. ***, **, * correspond to the significant levels of 1%, 5%, and 10% respectively.

	Ln (Total Gap)														
	Full Sample	Exclude Crisis	At Least Two VPs	At Least Three VPs	ExecuComp Sample	10-X Header State	Exclude Relocation	Decrease Before	Increase Before	Change Before	Exclude Florida	Exclude Ohio	Exclude FL & OH	SIC-2 Industry	NAICS-3 Industry
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
NCA Enforceability Up	0.141***	0.167***	0.174***	0.161***	0.180***	0.178***	0.210***	0.203***	0.167***	0.209***	0.182***	0.177***	0.174***	0.199***	0.184***
	(3.67)	(4.79)	(5.04)	(4.68)	(4.80)	(4.39)	(5.90)	(5.39)	(3.30)	(3.95)	(4.77)	(4.15)	(3.74)	(5.80)	(5.50)
Observations	34,585	25,652	28,292	27,462	27,634	25,525	24,448	26,887	26,180	24,618	27,434	27,026	26,011	27,780	27,149
R-squared	0.638	0.636	0.637	0.633	0.639	0.638	0.639	0.637	0.636	0.636	0.639	0.638	0.640	0.639	0.641
Controls from Table 2	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes