DEPARTMENT OF ECONOMICS AND FINANCE

SCHOOL OF BUSINESS AND ECONOMICS

UNIVERSITY OF CANTERBURY

CHRISTCHURCH, NEW ZEALAND

The Wages of Austerity: Executive Pay Caps and the Quiet Life

Lihan Chen
Shaojie Lai
P. Raghavendra Rau
Qing Sophie Wang

WORKING PAPER

No. 5/2026

Department of Economics and Finance
UC Business School
University of Canterbury
Private Bag 4800, Christchurch
New Zealand

WORKING PAPER No. 5/2026

The Wages of Austerity: Executive Pay Caps and the Quiet Life

Lihan Chen[1]
Shaojie Lai[2]
P. Raghavendra Rau[3]
Qing Sophie Wang[4†]

June 2026

Abstract: We study how binding executive pay compression affects managerial behavior and firm outcomes. Using China's 2015 executive pay-cap reform for state-owned enterprises, we compare SOEs with non-SOEs around a policy that sharply reduced executive compensation and limited common substitution channels. Treated firms exhibit lower work effort and CEO engagement, fewer unrelated acquisitions, less risk-related language in disclosure, lower investment, weaker investment efficiency, higher cash holdings, slower cash adjustment, and lower Tobin's Q and total factor productivity. We find little evidence that executives respond by increasing observable perks or tunneling. Instead, the results are more consistent with a passive "quiet life" response, in which weaker monetary incentives reduce managerial initiative and responsiveness. The effects are stronger for older CEOs and in more competitive industries, where performance incentives are likely more salient. CEO turnover rises, especially among high-performing executives, and pay-performance sensitivity declines, suggesting that selection effects and weaker formal incentives reinforce the behavioral response. Treated firms also increase ESG and CSR scores and employment, consistent with greater attention to politically salient outcomes. Overall, the evidence suggests that binding pay compression can generate real efficiency costs even when alternative career, reputational, and political incentives remain in place.

Keywords: Executive compensation, Pay regulation, Managerial incentives, Corporate investment, Firm performance

JEL Categories: G30, G38, J33, M52

Acknowledgements: We acknowledge helpful comments from Chris P. Duk, Ling Feng, Cesare Fracassi, Haoyu Gao, Dragon Tang, Jing Xie, Xiaoxu Yu, Kuo Zhang, and seminar participants at 2025 China Banking and Corporate Finance Young Scholar Forum, 2025 Sydney Banking and Financial Stability Conference, 2025 China International Forum on Finance and Policy Conference, International Research Seminar (IReS) in Finance, Jiangxi University of Finance and Economics - Shanghai University of Finance and Economics Bilateral Finance Forum, University of Cambridge, School of Management at Shandong University.

[1] School of Finance, Shanghai University of Finance and Economics, CHINA
[2] School of Finance, Jiangxi University of Finance and Economics, CHINA
[3] Cambridge Judge Business School, University of Cambridge, UNITED KINGDOM
[4] Department of Economics and Finance, University of Canterbury, NEW ZEALAND.

† Corresponding author: Email: sophie.wang@canterbury.ac.nz

1. Introduction

Should governments cap executive pay? Policymakers have increasingly considered this question in response to concerns about inequality, rent extraction, and the perceived social costs of high executive compensation. The European Union capped bankers' bonuses at 100% of base salary after the financial crisis, [1] the UK Green Party has more recently proposed a 10:1 company pay-ratio limit,[2] and similar CEO-to-worker pay-ratio tax proposals have attracted recurring political attention in the United States,[3] with some local governments having adopted pay-ratio surtaxes.[4] These policies share a common premise: that executive pay can become socially or politically excessive (Murphy, 2013; Cebon and Hermalin, 2015; Murphy and Jensen, 2018; Edmans et al., 2023; Chaigneau et al., 2025), and that regulation can limit these excesses without imposing large costs on firm performance.

The key economic issue is whether this premise is correct. Executive compensation is not only a transfer from shareholders to managers. It is also part of the incentive system that shapes managerial effort, risk-taking, investment, and resource allocation. If observed pay reflects rent extraction, then pay regulation may reduce waste without impairing performance. If, however, high-powered pay partly motivates costly managerial actions, then pay compression may reduce initiative and weaken firm outcomes. The answer is ultimately empirical, but the evidence remains limited because many pay regulations do not generate binding reductions in total incentives.

A central empirical challenge is that regulated firms often adjust around pay restrictions. The U.S. experience with the 1993 Omnibus Budget Reconciliation Act's $1 million deductibility cap shifted compensation toward stock options rather than reducing total executive pay (Rose and Wolfram, 2000, 2002). Banker bonus caps in Europe led many

[1] See for example, MEPs cap bankers' bonuses and step up bank capital requirements, Press Release, European Parliament, 2013.

[2] The UK Green Party proposed to limit pay inequality by introducing a 10:1 pay ratio, meaning the highest-paid person in an organisation could earn no more than ten times the lowest-paid person (See The Green Party's Ill-Considered Policy to Cap CEOs' Pay", The Economist, June 4, 2026).

[3] The "Tax Excessive CEO Pay Act" would raise corporate tax rates for companies with CEO-to-median-worker pay ratios ≥50:1, with surtaxes scaling up to +5% for ≥500:1 (see Ewall-Wice, S. 2021, Democratic lawmakers want companies to pay up for massive CEO compensation, CBS News, March 18, 2021). More recently, San Francisco voters were asked to vote on a proposal to raise taxes on large companies whose top executives earn more than 100 times the median worker's pay (See "San Francisco Rejects a Tax Hike on Companies with Highly Paid Executives", The Wall Street Journal, June 9, 2026).

[4] The City of Portland imposed a pay-ratio surtax on publicly traded companies' city business-license tax: +10% if the SEC-reported CEO-to-median-worker ratio is ≥100:1 and <250:1, and +25% if ≥250:1; adopted in 2016 and administered since 2017 (see Holcomb, G. 2016, Portland, Oregon authorizes surtax on executive pay for publicly traded companies, EY Tax News, 9 December 2016). In California, state lawmakers have considered bills to raise state corporate taxes based on CEO pay ratios. The proposals would increase the base rate and add surtaxes that scale with the ratio, with recent coverage highlighting a plan to lift the 8.8% rate up to as high as 14.8% depending on the pay gap (see Doyle, C. 2020, The Impact of Pay Ratio Tax Plans, Equilar report, January 16, 2020).

financial institutions to increase fixed salaries or deferred compensation, limiting the effect on total pay (Murphy, 2013). Say-on-pay rules and compensation disclosure requirements can alter the design and visibility of pay, but they often leave boards with substantial discretion to preserve incentives through alternative channels (Ferri and Maber, 2013). While these settings are informative about regulatory avoidance and compensation redesign, they make it difficult to study what happens inside firms when performance-based monetary incentives are actually compressed.

China's 2015 executive pay-cap reform for state-owned enterprises (SOEs) provides a useful setting for studying this question. Implemented as part of a broader austerity and anti-corruption campaign, the reform tightened administrative control over SOE executive compensation by linking managerial pay more closely to employee wage benchmarks and imposing stricter limits on executive compensation and related benefits.[5] Consistent with the reform's binding nature, Zhang et al. (2026) document that top-executive pay multiples in central SOEs declined from about 12 to roughly 7–8 times average employee wages, with the largest reductions concentrated in major financial SOEs. The reform differs from many developed-market pay regulations because it combined meaningful pay compression with limited scope for substitution. Equity-based compensation was negligible in Chinese SOEs, perks and informal benefits were constrained by concurrent anti-corruption enforcement, and the state's dual role as regulator and controlling shareholder strengthened compliance. The reform therefore created a setting in which executives faced a substantial reduction in formal monetary incentives, with fewer observable channels through which to offset the reduction.

While the Chinese SOE setting is distinctive, the mechanism we study is not. The central question is how managers respond when performance-based monetary rewards are compressed while other sources of managerial discipline, such as career concerns, reputation, internal status, political advancement, and organizational monitoring, remain operative. This trade-off is relevant beyond China. Pay caps, bonus restrictions, pay-ratio taxes, and compensation guidelines in regulated or politically sensitive sectors all seek to limit executive pay without removing managerial responsibility for investment, risk-taking, and operating performance. The Chinese reform is useful because the incentive shock is unusually sharp and the usual compensation substitutes are unusually limited. We therefore view the setting as an informative benchmark for the direction and rough magnitude of the real effects that can arise when pay

[5] The CCP Politburo approved the *Reform Plan for the Remuneration System for Executives of Centrally Administered Enterprises* (《中央管理企业负责人薪酬制度改革方案》) on 29 August 2014; the reform was implemented in 2015.

regulation weakens high-powered monetary incentives. The estimates should not be interpreted as mechanically transferable to all firms or all countries, but they speak to a broader policy question: whether pay regulation can impose efficiency costs by reducing managerial initiative even when career, reputational, and political incentives remain in place.

The effect of pay compression is theoretically ambiguous. If high executive pay mainly reflects rents, then restricting pay may improve efficiency by reducing excess risk-taking, empire-building, or private-benefit extraction. If managers are strongly motivated by promotion prospects, reputation, or political incentives, then non-monetary rewards may sustain effort despite lower formal pay. But if monetary incentives are an important complement to these implicit incentives, then pay compression may induce a passive agency response. Managers may avoid difficult decisions, pursue fewer risky or effort-intensive projects, retain more idle resources, and respond less aggressively to growth opportunities. This "quiet life" response differs from active rent extraction. It does not require managers to consume more perks, tunnel resources, or build empires. The agency cost instead appears through foregone initiative, weaker responsiveness, and more conservative resource allocation.

We test these ideas using a difference-in-differences design that compares SOEs with non-SOEs around the 2015 reform. Our sample covers non-financial A-share firms listed in Shanghai and Shenzhen from 2011 to 2019. The treatment indicator is based on SOE status before the reform. The empirical specification includes firm fixed effects, industry-by-year fixed effects, and province-by-year fixed effects, so the estimates compare changes within treated firms relative to non-SOEs operating in the same broad time-varying industry and regional environments. We interpret the coefficient on the SOE-by-post interaction as the reduced-form effect of exposure to the pay-compression regime. This interpretation is appropriate because the reform changed the state-sector compensation environment as a whole, not only realized pay cuts for individual executives.

Our evidence points to a broad decline in managerial engagement and initiative. Treated SOEs exhibit lower work intensity after the reform. Media-based overtime mentions (Yang et al., 2025) fall by about 2.4%, and satellite-based nighttime activity falls by about 4.8% relative to the sample mean. CEO engagement with investors also declines sharply: the probability that the CEO participates in at least one investor interaction falls by 6.9 percentage points, or about 35% relative to the pre-reform mean. These results suggest that compensation compression affects not only accounting outcomes or formal policies, but also discretionary managerial involvement.

Treated firms also become more conservative in risk-related and investment decisions.

The probability of undertaking an unrelated acquisition falls by 12.0 percentage points, an economically large decline relative to the 14.7% sample mean. Risk-related language in MD&A disclosures (Ru et al., 2025; Gao et al., 2023) also declines, indicating lower risk salience in managerial disclosure. Investment falls by 1.0 percentage point of assets, or about 22% relative to the sample mean. The decline is accompanied by lower M&A activity and lower repurchase activity, while dividend propensity increases and R&D participation is largely unchanged. These patterns are consistent with a shift away from discretionary expansion and toward more conservative capital allocation.

The decline in investment is not accompanied by better investment efficiency. Instead, treated firms' investment becomes less sensitive to Tobin's Q, and residual-based investment inefficiency increases, mainly among firms that appear to underinvest. Cash policy shows a similar pattern. Treated SOEs hold more cash after the reform, adjust more slowly toward target cash levels, and experience a lower marginal value of cash. The increase in cash holdings is not matched by stronger sales growth. It is accompanied by higher working capital and more external financing, suggesting that firms retain and raise funds without deploying them more productively. Consistent with these operating and financial-policy changes, treated firms experience lower Tobin's Q and lower total factor productivity.

We then examine mechanisms and alternative explanations. One possibility is that executives respond to lower formal pay by extracting more private benefits. We find little support for this channel. Observable perks and tunneling decline rather than increase, consistent with the view that concurrent anti-corruption and austerity enforcement constrained these margins. Another possibility is that the reform changes the composition of managers. Consistent with this channel, CEO turnover rises after the reform, especially among high-performing executives, and pay-performance sensitivity declines. These results suggest that selection effects and weaker formal incentives reinforce the behavioral response. Finally, treated firms increase ESG and CSR scores and employment. We interpret these results cautiously. They are consistent with greater attention to politically salient firm outcomes[6], but they do not by themselves identify a specific mandate channel or imply that effort is simply reallocated from financial to social objectives.

[6] One concern is that firms may reduce the reported executive-to-employee pay multiple mechanically by outsourcing low-wage workers, rather than by lowering executive compensation. However, this explanation would typically predict a decline in employee headcount and an increase in average employee pay. The observed increase in employment is difficult to reconcile with broad-based outsourcing of low-wage workers as the primary mechanism, although it does not rule out limited labor reclassification.

Heterogeneity tests support the incentive interpretation. The effects are stronger for older CEOs, whose promotion prospects are likely weaker, and in more competitive industries, where performance incentives are likely more salient. They are also stronger along several margins in firms with weaker internal governance. These patterns suggest that career concerns and internal monitoring can partly substitute for monetary incentives, but do not fully offset the loss of high-powered pay. The results are not concentrated in a narrow subset of SOEs. They appear for both central and local SOEs and are not driven solely by strategic industries, firms with high initial pay gaps, or politically connected managers.

We also provide supporting exposure-based evidence using pre-policy executive-worker pay gaps. Because the reform linked executive compensation to employee wages, SOEs with larger pre-policy pay multiples were more exposed to binding compensation constraints. In a 2SLS design, larger predicted compensation cuts are associated with lower CEO-investor interactions, fewer unrelated acquisitions, less risk-related disclosure language, lower investment, higher cash holdings, and lower firm valuation. We note that this evidence is not the paper's main identification strategy, but it complements the DID estimates by showing that the intensity of pay compression matters.

A series of robustness tests supports the interpretation of the baseline results. Event-study estimates show no evidence of differential pre-trends and a clear post-reform decline in the main outcomes. Placebo tests based on randomly assigned treatment status produce treatment effects far from the actual estimates. The results remain similar after entropy balancing, alternative matching procedures, nonlinear models for binary and count outcomes, double machine-learning controls, and specification-curve analysis. They are also robust to controlling for contemporaneous credit-market conditions, bond-market exposure, anti-corruption intensity, deleveraging exposure, and alternative outcome definitions. These tests do not eliminate all concerns inherent in a broad policy setting, but they make it less likely that the results are driven by pre-existing trends, observable selection, or a single modeling choice.

Our paper makes three contributions. First, it contributes to the literature on executive pay regulation. Prior work shows that firms often respond to pay restrictions through compensation redesign, including option grants, fixed salaries, deferred pay, and other substitutes (Rose and Wolfram, 2000, 2002; Colonnello et al., 2023; Kleymenova and Tuna, 2021). We study a setting in which these substitution channels were more constrained and examine real organizational outcomes rather than only compensation design. The evidence shows that binding pay compression can reduce managerial engagement, risk-related activity, investment, investment efficiency, and firm performance, while increasing cash retention. These findings extend the

pay-regulation literature from avoidance and compensation structure to the real effects of weakened monetary incentives.

Second, the paper contributes to the literature on executive incentives by documenting limits to the substitutability between explicit and implicit incentives. Classic theories emphasize that career concerns, reputation, and labor-market discipline can motivate managers even when current pay is low powered (Fama, 1980; Holmström, 1999; Gibbons and Murphy, 1992). Our evidence suggests that these forces do not fully offset a sharp reduction in performance-based monetary rewards. The effects are strongest where alternative incentives are weaker or where the loss of monetary incentives is more salient. This evidence is consistent with monetary incentives and implicit incentives acting as imperfect substitutes, and in some settings as complements.

Third, the paper contributes to the agency-cost and quiet-life literature. The usual concern with executive pay restrictions is that managers may offset lower pay through perks, tunneling, or empire-building (Jensen and Meckling, 1976; Bertrand and Mullainathan, 2003). We find a different pattern. Treated executives do not appear to increase observable private-benefit extraction. Instead, firms show lower engagement, fewer discretionary expansion decisions, slower cash adjustment, more cash retention, and weaker productivity. This perspective complements recent studies of pay regulation (e.g., Abudy et al., 2020; Bae et al., 2024; Balogh et al., 2026; Nanda et al., 2024; Cao et al., 2026; Lan and Pan, 2026) in pointing to a passive agency cost of pay compression: managers may respond to weaker incentives not by taking more, but by doing less.

Our findings have implications for current debates over executive pay regulation. The evidence does not imply that pay caps are always inefficient, nor does it measure the distributional or political benefits of limiting executive compensation. It does show, however, that binding pay compression can create real efficiency costs when it weakens incentives for managerial initiative and reduces the likelihood that high-performing executives remain in affected firms. Policies designed to limit executive pay may therefore involve a trade-off between fairness, rent containment, and productive effort. Understanding that trade-off requires looking beyond compensation levels to the real decisions managers make when high-powered monetary incentives are compressed.

2. Institutional Background and Conceptual Framework

2.1 Executive Pay Regulation and China's 2015 Reform

Governments regulate executive compensation for several reasons: to limit rent extraction, reduce perceived inequities, constrain excessive risk-taking, and align managerial pay with broader social or political objectives. In developed markets, these interventions have typically taken three forms. The first is tax-based regulation, such as U.S. Section 162(m), Canada's limits on stock-option deductions, and Austria's deductibility cap. These policies change the tax cost of specific forms of compensation, but prior evidence generally finds substantial redesign of pay packages and limited reductions in total compensation (Rose and Wolfram, 2000, 2002; Bird, 2018; Bornemann et al., 2023). The second is disclosure-based regulation, including the SEC's 2006 disclosure reforms and Dodd-Frank pay-ratio disclosure. These rules increase the visibility of executive compensation and can affect investor and stakeholder responses, but they do not mechanically restrict pay levels (Ferri et al., 2018; Pan et al., 2022; Chang et al., 2023). The third is governance-based regulation, such as mandatory say-on-pay votes, which gives shareholders a formal channel to express views on compensation policy. These votes can lead firms to adjust pay design or pay-performance sensitivity, although the evidence on pay levels is mixed (Cai and Walkling, 2011; Ferri and Maber, 2013; Iliev and Vitanova, 2019).

A common feature of many developed-market reforms is that they leave firms with substantial scope for adjustment. Boards can shift compensation across salary, bonus, equity grants, deferred pay, pensions, and other benefits. As a result, these reforms are informative about regulatory avoidance and compensation design, but they do not always create a clean setting for studying how managers respond when performance-based monetary incentives are substantially compressed. China's 2015 executive pay-cap reform differed in this respect. It imposed a more direct constraint on executive pay in SOEs and was implemented in an environment where conventional substitution channels were limited.

To understand the 2015 reform, it is useful to place it in the longer evolution of SOE compensation policy. Before the 1990s, SOE executive pay was largely administrative, with compensation tied more closely to bureaucratic rank than to firm performance. This changed in the early 2000s after the establishment of the State-owned Assets Supervision and Administration Commission (SASAC). SASAC introduced a more market-oriented compensation framework under which SOEs had greater flexibility to reward executives based on firm size, profitability, and other performance metrics. This shift increased the role of formal

monetary incentives in the state sector, but it also led to growing dispersion between executive pay and rank-and-file employee wages. The divergence was particularly visible after the 2008 financial crisis and generated public criticism of high SOE executive compensation.

Earlier attempts to control SOE pay had limited effect. The 2009 guidelines sought to broaden the definition of compensation and restrict bonuses, but enforcement was incomplete. Executives and firms could still shift compensation toward perks, expense-based benefits, and other less transparent forms of pay (Bae et al., 2024). These workarounds highlighted the limits of pay regulation when firms retain access to informal or off-balance-sheet compensation channels. The institutional environment changed after the 2013 anti-corruption campaign, which increased scrutiny of illicit benefits, official consumption, and informal compensation practices among SOE executives (Griffin et al., 2022). By the time the 2015 pay-cap reform was implemented, the state had both a stronger political mandate and stronger enforcement tools to constrain substitution into perks and informal benefits.

The 2015 reform marked a substantial reassertion of centralized control over SOE executive compensation. The reform was approved in 2014 and formally implemented on January 1, 2015. It tightened administrative oversight of executive pay and sharply curtailed performance-linked bonuses in affected SOEs. Zhang et al. (2026) document that, among central SOEs, the average top executive pay multiple fell from about 12 times to roughly 7–8 times average employee wages. The largest nominal cuts occurred in major financial SOEs, where some executive pay reductions reached 40% to 70%. Although the formal policy was directed primarily at centrally administered SOEs, the reform diffused across the broader state sector. Central SOEs began reducing and publicly disclosing executive pay, while many provincial governments subsequently released their own pay-cap plans for local SOEs. [7] Some local SOE pay-reform rules explicitly adopted, or appear to imply, a ceiling of 10.4 times average employee pay (Cao et al., 2026). For example, public reports on Sichuan's provincial SOE pay-reform rules state that the total annual compensation of provincial SOE executives should not exceed 10.4 times the average wage of employees in provincial SOEs in the previous year.[8]

[7] See for example, Er, L. 2015, China imposes pay cap on SOE executives as part of reform push, BBC, January 3, 2015. The policy was also extended to provincial SOEs, with 25 provinces publicly releasing reform plans that restrict the compensation of executives in these firms (see It's official! 25 provinces have announced state-owned enterprise salary cap orders—how will executive pay change in your hometown? CCTV Finance, April 26, 2016).

[8] See for example, Sichuan's SOE Executive Compensation Reform: Annual Pay Capped at 10.4 Times the Average Employee Wage, Sohu, October 14, 2014. The same multiple follows from the standard compensation formula used in some local rules: basic annual salary is capped at twice the compensation benchmark; performance-based annual salary can reach up to basic annual salary multiplied by an annual assessment coefficient of 2 and an adjustment coefficient of 1.5, equivalent to six times the benchmark; and tenure incentive income is capped at 30% of the sum of the first two components, equivalent to 2.4 times

Several features make this setting useful for our analysis. First, the reform represented a meaningful compression of formal executive compensation rather than a small change in disclosure, tax treatment, or shareholder voting rights. Second, equity-based compensation was negligible in Chinese SOEs, limiting the scope for firms to offset lower cash compensation through stock options or restricted shares. Third, concurrent anti-corruption and austerity enforcement reduced the ability of executives to substitute toward perks, expense accounts, or informal private benefits. Fourth, the state's dual role as regulator and controlling shareholder limited the ability of affected firms to resist implementation. These features distinguish the 2015 reform from many pay regulations in developed markets, where firms often preserve incentives by redesigning compensation packages.

Our descriptive evidence is consistent with the reform producing meaningful pay compression in the state sector. Figure 1 shows a relative decline in SOE executive compensation after the reform compared with non-SOEs. [9] Internet Appendix IA.2 provides additional evidence on the distribution of compensation changes around the policy. A larger share of SOEs experience executive pay cuts from 2014 to 2015 than non-SOEs, and the distribution of compensation changes for SOEs shifts visibly to the left. Both central and local SOEs exhibit sizable pay reductions, while non-SOEs show little comparable change. The evidence also indicates that the reform compressed executive-worker pay multiples among SOEs.

Figure 2 provides further evidence that the intensity of the compensation shock varied with pre-policy exposure to the reform. SOEs with larger pre-policy executive-worker pay multiples experienced larger subsequent reductions in log executive compensation. This pattern is important because the reform linked executive pay to average employee wages. Firms with larger pre-reform internal pay gaps therefore faced more binding constraints when the pay-cap regime was imposed. We use this variation later as supporting exposure-based evidence, but our main empirical design compares SOEs with non-SOEs around the reform.

Our institutional setting therefore provides two sources of empirical leverage. The first is the broad state-sector treatment created by the 2015 reform, which motivates our difference-in-differences design comparing SOEs with non-SOEs before and after the policy. The second is variation in the intensity of pay compression within the treated sector, based on pre-policy

---

the benchmark.

[9] Internet Appendix IA.1 illustrates a similar decline for financial SOEs in our sample. The relative CEO compensation gap between financial and non-financial SOEs declines after the 2015 pay-cap reform, consistent with the large nominal cuts observed at major state banks. Our main analysis, however, is not confined to the financial sector and instead exploits SOE versus non-SOE variation across the broader listed-firm sample.

executive-worker pay gaps. We interpret the baseline estimates as the reduced-form effect of exposure to the post-2015 pay-compression regime. This interpretation recognizes that the reform changed the compensation environment faced by SOE executives as a group, rather than only mechanically reducing pay for a subset of individuals. It also fits the central economic question of the paper: how managers and firms respond when performance-based monetary incentives are compressed and the usual substitution channels are constrained.

2.2 Incentives under State Ownership and Weak Contracting

Managerial incentives in SOEs differ from those in privately controlled firms because ownership, control, and objectives are less tightly aligned. In private firms, shareholders are the residual claimants, boards are expected to monitor managers on their behalf, and external governance mechanisms such as takeovers, activist pressure, and managerial labor markets can discipline poor performance. These mechanisms are weaker or less direct in SOEs. Residual claims are more diffuse, boards may balance commercial and political objectives, and the threat of external control is limited by state ownership (Jiang and Kim, 2020; Firth et al., 2015). As a result, managerial contracting in SOEs is inherently incomplete.

However, this does not mean that SOE executives lack incentives. Instead, their incentives come from a broader set of sources. Though formal pay-for-performance incentives matter, they operate alongside career concerns, political promotion, bureaucratic advancement, internal prestige, reputation, and continued access to positions within the state sector. In China, prior research suggests that political advancement can be linked to firm performance, making career concerns a potentially important substitute for high-powered financial incentives (Cao et al., 2019). These implicit incentives can motivate effort when executives believe that good performance improves their future prospects. However, they are less precise than formal compensation contracts. Their strength depends on whether promotion opportunities are credible, whether performance is observable, and whether executives believe that additional effort will be rewarded.

The pay-cap reform can weaken this incentive system in two ways. First, it directly lowers the monetary return to performance by compressing executive pay and reducing performance-linked compensation. Second, it may change executives' beliefs about how the state rewards managerial performance. If executives interpret the reform as a shift away from rewarding commercial outcomes and toward limiting pay dispersion, then the motivational effect may extend beyond the mechanical reduction in compensation. In this sense, the reform affects not only the level of pay, but also the perceived power of the incentive contract.

The predicted effect of pay compression is ambiguous. If high executive pay mainly reflects rents, then restricting compensation could improve efficiency. Lower pay could reduce incentives for empire-building, excessive risk-taking, or private-benefit extraction. Similarly, if career concerns, political promotion, and reputational incentives are strong enough, managers may continue to exert effort even when formal monetary rewards are compressed. In this case, the real effects of the pay caps could be small.

A different prediction follows if monetary incentives and implicit incentives are imperfect substitutes. When performance-based pay is compressed, managers may have weaker incentives to undertake costly or discretionary actions whose benefits are uncertain and whose private rewards are limited. They may exert less effort, avoid difficult decisions, pursue fewer risky or complex projects, and become less responsive to investment opportunities. This response is consistent with the "quiet life" view of managerial behavior, under which managers facing weaker discipline prefer lower effort, lower risk, and greater organizational slack over value-maximizing but costly actions (Hicks, 1935; Rhoades and Rutz, 1982; Bertrand and Mullainathan, 2003).

This quiet-life channel is distinct from active rent extraction. Managers need not consume more perks, tunnel resources, or deliberately divert assets from shareholders. The agency cost may instead appear as passivity: fewer discretionary initiatives, slower adjustment, greater cash retention, weaker investment responsiveness, and lower operating efficiency. These outcomes can be less visible than direct expropriation, but they are economically important because they affect how firms allocate capital and respond to growth opportunities.

Several alternative channels could produce overlapping empirical patterns. One possibility is managerial power. Executives whose formal pay is restricted may attempt to restore private benefits through perks, tunneling, or empire-building, especially when monitoring is weak (Adams et al., 2005; Bebchuk et al., 2011). This channel predicts an increase in observable private-benefit extraction or self-serving expansion. A second possibility is that governance and reputational mechanisms offset the decline in monetary incentives. Stronger boards, internal controls, political oversight, and reputational concerns may sustain effort even when compensation is constrained (Ferri and Maber, 2013). This channel predicts weaker responses in firms with stronger internal governance or stronger external discipline. A third possibility is selection. More able or more marketable executives may leave affected firms when compensation becomes less attractive, while those who remain may have weaker outside options or lower responsiveness to performance incentives (Kleymenova and Tuna, 2021; Nanda et al., 2024). This channel predicts higher turnover, especially among better-performing

managers.

A fourth possibility is that managers shift attention toward outcomes that remain politically visible or institutionally rewarded. SOEs operate under mixed objectives, and compensation compression may increase the relative importance of non-financial or state-aligned goals. Managers may therefore place greater weight on employment, ESG, CSR, or other visible stakeholder-oriented outcomes. This need not imply a formal mandate channel. It may simply reflect a change in the relative payoff to different actions when commercial performance is less strongly rewarded through pay. The empirical implication is that treated firms may increase activity on politically salient margins even as investment efficiency, productivity, or shareholder valuation deteriorate.

The strength of these channels should vary with the availability of alternative incentives. Career concerns should matter less for executives close to retirement, because promotion prospects become less valuable when the remaining career horizon is short. This logic is consistent with tournament-style models in which promotion incentives weaken as the probability of advancement declines (Lazear and Rosen, 1981). Internal governance should also matter. Stronger monitoring can partially substitute for monetary incentives by constraining managerial passivity or private-benefit extraction. Market competition may matter as well. In competitive industries, managerial effort and strategic responsiveness are likely to be more valuable, so the loss of performance-based rewards may have larger real effects.

These arguments guide the empirical analysis. We examine whether treated firms exhibit lower effort and engagement, less risk-related activity, weaker investment, lower investment efficiency, more cash retention, and worse performance. We then assess whether these patterns are accompanied by increases in perks, tunneling, or empire-building; whether CEO turnover rises, especially among high-performing executives; and whether treated firms shift toward politically salient outcomes such as ESG, CSR, and employment. Finally, we test whether the effects are stronger where implicit incentives or monitoring are weaker, such as among older CEOs and firms with weaker internal governance, and where performance incentives are more salient, such as in more competitive industries. This structure allows us to distinguish among managerial disengagement, private-benefit substitution, executive selection, and a shift toward politically visible firm outcomes.

3. Data, Sample, and Methodology

3.1 Data Sources

Our empirical analysis uses Chinese A-share firms listed on the Shanghai and Shenzhen

Stock Exchanges from 2011 to 2019, covering four pre-reform years and the implementation year plus four subsequent years. This window allows us to study post-reform changes in managerial behavior and firm outcomes while avoiding a much longer post-period in which later regulatory or macroeconomic shocks may become more difficult to separate from the pay-compression regime.

We obtain firm-level financial, governance, ownership, executive, and compensation data from the China Securities Markets and Accounting Research (CSMAR) database and the Chinese Research Data Services (CNRDS) Platform. These data provide the information needed to identify SOE status, construct firm and CEO characteristics, measure investment, cash holdings, payout policy, firm performance, and several governance and mechanism variables.

We supplement these databases with two additional sources that capture managerial behavior more directly. First, we use NASA's VNP46A2 nighttime light data to construct a satellite-based measure of nighttime corporate activity. This measure provides an independent proxy for operating intensity that does not rely on firms' own disclosures. Second, we use text from corporate annual reports to construct Risk Tone, which measures the salience of risk-related language in MD&A disclosure. Together with media-based overtime measures and CEO-investor interaction data, these sources allow us to study dimensions of managerial effort, engagement, and risk-related behavior that are not fully captured by standard accounting variables.

The baseline sample consists of non-financial A-share firms. We exclude financial firms because their balance sheets, investment policies, regulatory environment, and executive compensation practices differ from those of industrial firms. We also exclude firms with special treatment status and particular transfer listings. To maintain a stable treatment classification, we remove firms that change ultimate controlling ownership during the sample period, including firms that switch from private control to state ownership or from state ownership to private control. We further exclude firms listed after 2014 and firms without observations after 2015, because these firms do not provide the pre-policy information needed to define treatment status and baseline characteristics. Continuous variables are winsorized at the 1st and 99th percentiles to reduce the influence of extreme observations.

The final baseline sample contains 18,825 firm-year observations from 2011 to 2019. Sample sizes vary across some analyses because several outcome variables require additional data. In particular, the satellite-based overtime measure is available only for a smaller set of firms and years because nighttime light data begin later in the sample period and because we

exclude firms that cannot be uniquely mapped to a geographic grid.

3.2 Key Variables and Descriptive Statistics

This section defines the main treatment variable, outcome variables, mechanism variables, and controls. Detailed variable definitions are reported in the Appendix, and Table 1 presents summary statistics.

The treatment indicator, *Treat*, equals one for firms classified as SOEs in 2014, the year before the pay-cap reform, and zero otherwise. We define SOEs based on ultimate control by central or local government entities. Using pre-reform ownership fixes treatment status before the policy and avoids reclassifying firms based on post-reform ownership changes. SOEs account for 37.4% of the baseline firm-year observations. In robustness tests, we separately examine central and local SOEs.

Our first group of outcomes captures managerial effort and engagement. We use three complementary measures. The first two measure firm-level overtime intensity. These variables do not observe executives' own working hours directly, but they capture operating intensity within the organization, which senior managers can influence through production schedules, project deadlines, monitoring, and performance pressure. The first measure, *Ln*(1 + *Overtime Keywords*), is the natural logarithm of one plus the number of media articles that mention overtime-related keywords in connection with the firm, following Yang et al. (2025). In our sample, 2.6% of firms are identified as having overtime-related media exposure, close to the 3.5% rate reported in Yang et al. (2025).

The second overtime measure uses nighttime light intensity from NASA's VNP46A2 dataset. We map firms to geographic grids and identify unusually high nighttime activity by comparing the firm's nighttime brightness with surrounding areas and with the firm's own brightness on statutory public holidays, when normal operations should be limited. *Overtime Ratio* is the share of non-holiday days in a year on which the firm's nighttime brightness exceeds its statutory-holiday baseline.[10] The average *Overtime Ratio* is 0.331, indicating that elevated nighttime activity occurs on about one-third of non-holiday days. The satellite-based measure is available for a smaller sample because the data begin after 2012 and because firms must be uniquely mapped to geographic grids.

The third engagement measure is *Investor Interactions* (0/1), an indicator equal to one if the CEO participates in at least one recorded investor interaction during the year, including site

[10] We also generate two alternative measures: weekday overtime and weekend overtime, respectively. Our findings remain consistent using these alternative proxies. Results are not tabulated for brevity.

visits, investor inquiries, or online investor communications. Unlike the overtime measures, this variable captures the CEO's direct involvement in communicating with external stakeholders. Such interactions require managerial time and preparation and provide a complementary measure of executive engagement. The sample mean is 0.197, indicating that CEOs participate in at least one recorded investor interaction in about 19.7% of firm-year observations. [11]

Our second group of outcomes captures risk-related managerial behavior. We use two measures. *Unrelated Acquisition* (0/1) equals one if the firm undertakes an acquisition outside its primary industry. Unrelated acquisitions are likely to involve greater operational uncertainty because they require managers to evaluate and integrate businesses outside the firm's core area of expertise. This variable therefore captures realized risk-related corporate activity (Rau et al., 2025; Kravet and Muslu, 2013; John et al., 2008). In our sample, 14.7% of firm-year observations involve unrelated acquisitions.

The second risk-related measure is *Risk Tone*, defined as the natural logarithm of one plus the number of risk-related keywords in the MD&A section of the annual report, following Ru et al. (2025) and Gao et al. (2023). Because MD&A disclosure is prepared under managerial oversight, *Risk Tone* captures the salience of risk in managerial disclosure. It may reflect actual risk exposure, managerial perceptions of risk, or disclosure incentives. We therefore interpret *Risk Tone* as a measure of risk-related language, not as a direct measure of risk-taking. The average value of *Risk Tone* is 5.430.

Our third group of outcomes captures investment and capital allocation. *Investment* is measured as net cash outflow for long-term assets, scaled by total assets. The average firm invests 4.6% of total assets in long-term assets, with substantial cross-sectional variation. We also examine four binary capital-allocation outcomes: *Repurchase* (0/1), *R&D* (0/1), *M&A* (0/1), and *Dividend* (0/1), which indicate whether the firm repurchases shares, reports R&D expenditure, undertakes M&A activity, or pays a cash dividend during the year.

We measure investment efficiency in two ways. First, we estimate investment-Q sensitivity, which captures how strongly investment responds to market valuation signals (Jayaraman and Wu, 2019; Hou et al., 2024). Higher investment-Q sensitivity is consistent with stronger responsiveness to growth opportunities. Second, we use a Richardson (2006)-style

[11] In robustness tests, we also use two alternative engagement proxies: *Ln*(1+*Board Meetings*), the natural logarithm of one plus the number of board meetings, and *Ln*(1+*Investor Interactions*), the natural logarithm of one plus the number of CEO–investor interactions. The results remain consistent using these alternative measures and are reported in Internet Appendix IA.20.

measure of investment inefficiency[12], defined as the absolute residual from a model of expected investment based on firm fundamentals. Larger residuals indicate greater deviations from predicted investment. We also distinguish positive and negative residuals to examine overinvestment and underinvestment separately. The average investment-inefficiency measure is 0.022, lower than the 0.046 reported in Wu et al. (2022).

Our fourth group of outcomes covers cash policy and firm performance. *Cash* is measured as cash and cash equivalents scaled by total assets, following Dessaint and Matray (2017). The average firm holds 16.2% of assets in cash, with a median of 12.3%. We later use this measure to study cash holdings, the speed of adjustment toward target cash levels, and the marginal value of cash. Firm performance is measured using Tobin's Q and total factor productivity. Tobin's Q captures market valuation and growth opportunities. Total factor productivity (TFP) is estimated using the Levinsohn and Petrin (2003) method and captures operating efficiency.

To examine mechanisms, we use measures of CEO turnover, managerial rent extraction, and attention to politically salient firm outcomes. For CEO turnover, we use two measures. The first follows Nanda et al. (2024) and classifies turnover as voluntary when the CEO departs because of resignation, term expiration, personal reasons, or related categories, after excluding cases in which the CEO moves to an affiliated corporate group. Voluntary CEO turnover occurs in 5.3% of firm-year observations. [13] The second measure follows Lin et al. (2022) and equals one when the CEO is replaced despite strong prior performance, defined as prior-year ROA above the industry-adjusted median. This measure has a mean of 3.6%.

We use two measures to test whether executives substitute toward private benefits when formal pay is compressed. Perk is total perks scaled by sales, following Gul et al. (2011). Tunneling is measured using net other receivables, following Jiang et al. (2010). The mean perk ratio is 0.008, implying approximately 8 RMB of perks per 1,000 RMB of sales. The average value of the tunneling measure is 18.148 in logs.

Because SOEs may face objectives beyond shareholder value maximization, we also examine outcomes that are politically or socially salient. We measure social performance using Bloomberg ESG scores and Hexun CSR scores. We measure labor-related outcomes using employment, labor productivity, and wage-bill intensity. *Employment* is the natural logarithm

[12] Richardson (2006) estimates the optimal investment level using firm fundamentals:

$$Investment_{it} = \alpha_0 + \alpha_1 Growth_{it-1} + \alpha_2 Return_{it-1} + \alpha_3 Leverage_{it-1} + \alpha_4 Size_{it-1} + \alpha_5 Cash_{it-1} + \varepsilon_{it}$$

The absolute value of the residual ε_{it} from the regression is the investment inefficiency measure. Positive (negative) ε_{it} denotes overinvestment (underinvestment).

[13] This is a lower rate than the 15.7% value documented by Nanda et al. (2024). This discrepancy is likely attributable to their measure encompassing both CEO and chairperson turnover, as well as their potential inclusion of CEOs moving to affiliated corporate groups as voluntary turnovers. Despite this, our findings are robust to precisely adopting Nanda et al.'s (2024) CEO turnover methodology.

of the number of employees, following Zhang et al. (2026). *Labor productivity* is sales divided by the number of employees. *Wage-bill intensity* is total salary expense scaled by total assets. In Table 1, the average ESG score is 20.419, the average CSR score is 13.556, average employment is 7.705 in logs, average labor productivity is 1.610, and average wage-bill intensity is 0.064.

The baseline includes firm-level controls, with CEO/local controls used in specific tests or robustness checks. Firm-level controls include size, leverage, largest-shareholder ownership, cash flow, ROA, board size, independent-director ratio, and firm age. CEO-level controls include the natural logarithm of CEO age, CEO gender, and an indicator for whether the CEO also serves as chair. Macro controls include local GDP, employment, population, and fiscal pressure. The sample characteristics are broadly consistent with prior studies of Chinese listed firms (Xu et al., 2016; Chang et al., 2021): average ROA is 3.3%, average leverage is 43.3%, and the largest shareholder owns 35.1% of the firm on average. The mean value of the natural logarithm of CEO age is 3.888, corresponding to a typical CEO age of approximately 49 years; 6.5% of CEOs are female, and 25.8% of CEOs also serve as chair.

3.3 Methodology

We estimate the effect of the 2015 executive pay-cap reform using a difference-in-differences design that compares SOEs with non-SOEs before and after the reform. The treatment group consists of firms classified as SOEs in 2014, the year before the policy was implemented. The control group consists of non-SOEs. The post-reform period begins in 2015. Our baseline specification is:

$$Y_{it} = \alpha_i + \delta_{j,t} + \lambda_{p,t} + \beta\,(Treat_i \times Post_t) + \gamma' Z_{it} + \varepsilon_{it} \qquad (1)$$

where Y_{it} denotes the outcome of interest for firm i in year t. The outcomes include measures of managerial effort and engagement, risk-related behavior, investment, investment efficiency, cash policy, firm performance, CEO turnover, pay-performance sensitivity, rent extraction, and social or labor-related outcomes. $Treat_i$ is an indicator equal to one for firms classified as SOEs in 2014, and $Post_t$ is an indicator equal to one for fiscal years 2015 and later. The coefficient of interest is β, which captures the differential change in treated SOEs after the reform relative to non-SOEs.

The specification includes firm fixed effects, α_i, which absorb time-invariant differences across firms, including persistent differences in ownership, industry position, governance structure, and business model. It also includes industry-by-year fixed effects, $\delta_{j,t}$, and province-by-year fixed effects, $\lambda_{p,t}$. These fixed effects absorb shocks that vary over time across

industries and regions, including industry-specific demand conditions, regulatory changes, and local macroeconomic conditions. The vector Z_{it} includes time-varying firm-level controls. Standard errors are clustered at the firm level.

We interpret β as the reduced-form effect of exposure to the 2015 pay-compression regime. This interpretation is important because the reform affected the compensation environment of SOE executives as a group. The policy may influence outcomes through realized pay cuts, weaker pay-performance sensitivity, changes in perceived rewards to effort, executive selection, and constraints on substitution toward informal compensation. The baseline design therefore estimates the overall effect of being subject to the post-reform state-sector compensation regime, rather than only the mechanical effect of an observed change in compensation for a particular executive.

The identifying assumption is that, absent the reform, treated SOEs and non-SOEs would have followed parallel trends in the outcomes after accounting for firm fixed effects, industry-by-year fixed effects, province-by-year fixed effects, and time-varying controls. We assess this assumption in several ways. We report event-study estimates to examine pre-reform trends and the timing of post-reform effects. We also conduct placebo tests, entropy balancing, alternative specifications, nonlinear models for binary and count outcomes, controls for contemporaneous credit-market and policy shocks, and specification-curve analysis. These tests do not make the reform equivalent to random assignment, but they help assess whether the baseline estimates are likely to reflect pre-existing trends, observable selection, or a particular modeling choice.

4. The Impact of Executive Pay Caps on Corporate Behavior and Outcomes

4.1 Managerial Effort and Engagement

We begin by examining whether the pay-compression regime is associated with lower managerial effort and engagement. The key idea is that when the marginal monetary return to performance is reduced, managers may have weaker incentives to push the organization toward costly or discretionary activities. We test this prediction using three complementary proxies: two measures of firm-level overtime intensity and one measure of the CEO's direct engagement with investors.

Table 2 reports the results. Columns (1) and (2) examine overtime intensity. In Column (1), the dependent variable is *Ln*(1 + *Overtime Keywords*), based on media reports that link the firm to overtime-related activity. The coefficient on *Treat* × *Post* is −0.024 and statistically significant, indicating that treated SOEs experience an approximately 2.4% decline after the reform relative to non-SOEs. In Column (2), the dependent variable is *Overtime Ratio*, our

satellite-based measure of nighttime corporate activity. The coefficient on *Treat* × *Post* is −0.016 and statistically significant, implying a 1.6 percentage-point decline. Relative to the sample mean of 0.331, this corresponds to a decline of about 4.8%.[14]

These estimates suggest that treated firms exhibit lower organizational work intensity after the pay-cap reform. The two overtime measures have different sources of measurement error: the media-based measure depends on public reporting of overtime, while the satellite-based measure captures nighttime activity at firm locations. The fact that both measures decline is therefore useful. It suggests that the result is not specific to a single data source or disclosure channel. At the same time, these proxies do not directly measure executives' own working hours. We interpret them as measures of operating intensity that senior managers can influence through production schedules, project deadlines, monitoring, and internal performance pressure.

Column (3) turns to a more direct measure of CEO engagement. The dependent variable is *Investor Interactions* (0/1), an indicator equal to one if the CEO participates in at least one recorded investor interaction during the year. The coefficient on *Treat* × *Post* is −0.069 and statistically significant at the 1% level. This estimate implies that treated SOEs are 6.9 percentage points less likely to have CEO participation in investor interactions after the reform. Relative to the sample mean of 19.7%, the effect represents a decline of about 35.0%.

The investor-interaction result is important because it captures a discretionary activity that directly requires CEO time and attention. Investor meetings, site visits, and online communications are not mechanical accounting outcomes; they involve preparation, explanation of firm strategy, and engagement with outside stakeholders. The decline therefore complements the overtime evidence by showing that the reform is associated not only with lower firm-level operating intensity, but also with lower direct executive engagement.

Overall, Table 2 provides initial evidence consistent with a quiet-life response to pay compression. Treated SOEs reduce both organizational work intensity and CEO engagement after the reform. These results are consistent with the view that weaker monetary incentives reduce managerial initiative, although we cannot observe effort directly and therefore interpret the evidence as changes in observable proxies for effort and engagement.

[14] We further partition the full sample into industries with relatively limited shift work and overtime intensity (e.g., wholesale and real estate) and industries characterized by more continuous production processes and shift-based operations (e.g., manufacturing, mining, utilities and logistics). This distinction is particularly relevant because our effort and engagement measures partly rely on overtime-related activity, which is likely to be more salient and measured with greater precision in settings where extended or nighttime work schedules are more prevalent. Consistent with the interpretation that the satellite measure captures operating intensity, Internet Appendix IA.19 shows that the estimated decline in Overtime Ratio is stronger in shift-intensive industries.

4.2 Risk-taking and Risk-related Disclosure

We next examine whether pay compression is associated with changes in risk-related managerial behavior. When performance-based compensation is compressed, the upside from pursuing risky or complex projects may decline, while managers may continue to bear reputational, career, or political costs if these projects fail. This asymmetry can make managers less willing to pursue actions with uncertain payoffs, especially when the private rewards to successful outcomes are weakened (Kahneman and Tversky, 1979; Jensen and Meckling, 1976; Smith and Stulz, 1985).

We use two complementary measures to test this hypothesis. The first is *Unrelated Acquisition* (0/1), which captures whether a firm acquires a target outside its primary industry. Unrelated acquisitions are likely to involve greater uncertainty because they require managers to evaluate and integrate businesses outside the firm's core area of expertise. This measure therefore captures a realized corporate decision with risk-taking content. The second measure is Risk Tone, which captures the use of risk-related language in the MD&A section of the annual report. We interpret this variable as risk salience in managerial disclosure. It may reflect actual risk exposure, managerial risk perceptions, or disclosure incentives, and therefore should not be interpreted as a direct measure of risk-taking.

Table 3 reports the results. Column (1) shows that treated SOEs become significantly less likely to undertake unrelated acquisitions after the reform. The coefficient on *Treat* × *Post* is −0.120 and statistically significant at the 1% level. Relative to the sample mean of 14.7%, this estimate implies an economically large decline of about 81.6%. This result is consistent with a reduction in realized risk-related corporate activity after pay compression.

Column (2) shows a similar pattern for risk-related disclosure. The coefficient on *Treat* × *Post* is negative and statistically significant when *Risk Tone* is the dependent variable. This result indicates that treated firms use less risk-related language in MD&A disclosures after the reform. Because *Risk Tone* can reflect several underlying forces, we interpret this result cautiously. It suggests a decline in the salience of risk in managerial disclosure, rather than providing direct evidence of lower risk-taking by itself.

Overall, the results in Table 3 indicate that treated SOEs become more conservative along risk-related margins after the pay-cap reform. The decline in unrelated acquisitions points to lower realized risk-related corporate activity, while the decline in *Risk Tone* suggests lower risk salience in disclosure. These findings are consistent with the broader interpretation that pay compression weakens incentives to pursue uncertain or effort-intensive actions.

4.3 Investment Decisions and Capital Allocation

We next examine whether pay compression affects investment and capital allocation. Investment decisions are a natural margin on which managerial incentives may matter. Large investment projects, acquisitions, and repurchases require managerial attention, information gathering, negotiation, and a willingness to bear uncertainty. When performance-based compensation is compressed, managers may have weaker incentives to pursue discretionary projects with uncertain payoffs (Smith and Stulz, 1985), especially when failed projects can still impose reputational, career, or political costs.

Table 4 reports the results. Column (1) examines overall investment, measured as net cash outflow for long-term assets scaled by total assets. The coefficient on *Treat* × *Post* is −0.010 and statistically significant at the 1% level. Relative to the sample mean of 0.046, this estimate implies a decline of about 21.7%. Thus, treated SOEs reduce investment after the pay-cap reform relative to non-SOEs.

Columns (2) through (5) examine the composition of capital allocation. Treated SOEs become less likely to undertake M&A activity and less likely to repurchase shares. The coefficient on *Treat* × *Post* is −0.024 for M&A activity and −0.053 for share repurchases, both statistically significant. These estimates imply that treated firms reduce two forms of discretionary capital allocation that typically require active managerial decision-making. By contrast, the probability of paying a cash dividend increases by 6.6 percentage points. R&D participation does not change significantly, consistent with Lan and Pan (2026), who also find no R&D response to China's pay-restriction reform.

Overall, Table 4 indicates that treated SOEs shift away from discretionary expansion and repurchases after the reform. The decline in investment and M&A activity is consistent with lower managerial initiative in pursuing uncertain or effort-intensive projects. The increase in dividend propensity suggests a more conservative allocation of cash, while the lack of a significant R&D response indicates that the investment decline is not uniform across all forms of spending. These results fit the broader pattern of a quieter, more cautious managerial response to pay compression.

4.4 Investment Efficiency

The decline in investment documented in Table 4 need not imply lower efficiency. If treated firms were previously overinvesting, reduced investment could improve capital allocation. We therefore examine whether investment becomes more or less efficient after the pay-cap reform.

Table 5 provides evidence that investment efficiency deteriorates. Column (1) examines investment-Q sensitivity by interacting Tobin's Q with *Treat* × *Post*. The coefficient on *Treat* × *Post* × *Tobin's Q* is negative and statistically significant, indicating that investment by treated SOEs becomes less responsive to market valuation signals after the reform. [15] This result is consistent with weaker alignment between investment and growth opportunities.

Column (2) examines residual-based investment inefficiency, measured as the absolute deviation of actual investment from predicted investment based on firm fundamentals. The coefficient on *Treat* × *Post* is positive and statistically significant, indicating that treated firms' investment deviates more from predicted investment after the reform. Columns (3) and (4) split the inefficiency measure into overinvestment and underinvestment components. The increase in inefficiency is concentrated among underinvesting firms, with no comparable significant effect among overinvesting firms. [16]

These results are useful for interpreting the investment decline. The evidence does not suggest that the reform mainly reduced excessive investment. Instead, treated firms appear less likely to invest in line with their fundamentals and less responsive to growth opportunities. This pattern is consistent with a quieter and more conservative managerial response, in which pay compression weakens incentives to identify, evaluate, and pursue value-enhancing investment opportunities.

4.5 Cash Management

We next examine whether pay compression affects corporate cash management. Cash policy is a useful setting for studying managerial responsiveness because cash can either support valuable investment opportunities or reflect precaution, inertia, and agency-related slack. If managers become less willing to deploy resources after pay compression, treated firms may accumulate more cash, adjust more slowly toward target cash levels, and hold cash that investors value less.

Table 6 reports the results. Panel A examines cash holdings, measured as cash and cash equivalents scaled by total assets. [17] The coefficient on *Treat* × *Post* is positive and statistically

[15] This investment–Q sensitivity result is robust to using alternative proxies for investment opportunities. In Internet Appendix IA.3, we replace contemporaneous *Tobin's Q* with lagged *Tobin's Q* and industry median *Tobin's Q*. The coefficients on *Treat* × *Post* × *L.Tobin's Q* and *Treat* × *Post* × *Industry Median Q* remain negative, suggesting that the decline in investment responsiveness is not specific to the contemporaneous Tobin's Q measure.

[16] To rule out the possibility that our findings are driven by pre-existing financing constraints, we split the sample into two groups based on firms' ex-ante levels of financial constraints. We find no significant difference in the estimated *Treat* × *Post* effects across the two subsamples, suggesting that the documented decline in investment efficiency and evidence of underinvestment is unlikely to be attributable to differential financing constraints. The results are presented in Internet Appendix IA.4.

[17] Our results are robust to using an alternative cash proxy, *Cash1*, defined as cash and cash equivalents scaled by net assets. The results, reported in Internet Appendix IA.6, remain broadly consistent across cash holdings, cash-adjustment speed, and the marginal value of cash.

significant across specifications. In Column (3), which includes firm fixed effects as well as industry-by-year and province-by-year fixed effects, treated SOEs increase cash holdings by 4.6 percentage points relative to non-SOEs after the reform. Relative to the sample mean of 16.2%, this represents an increase of about 28.4%. This result indicates that treated firms accumulate larger liquidity buffers after the pay-cap reform.

Panel B examines whether treated firms adjust cash holdings differently relative to estimated target cash levels. Following Jiang and Lie (2016), we estimate a partial-adjustment model in which the dependent variable is the one-year change in cash holdings, $\Delta Cash$.

$$\begin{aligned}\Delta \mathrm{Cash}_{it} = {} & \beta_0 + \beta_1 \left(\mathrm{Treat}_i \times \mathrm{Post}_t \times \left(\mathrm{Cash}^*_{it} - \mathrm{Cash}_{i,t-1}\right)\right) + \beta_2\left(\mathrm{Cash}^*_{it} - \mathrm{Cash}_{i,t-1}\right) \\ & + \beta_3(\mathrm{Treat}_i \times \mathrm{Post}_t) + \beta_4 \left(\mathrm{Treat}_i \times \left(\mathrm{Cash}^*_{it} - \mathrm{Cash}_{i,t-1}\right)\right) + \\ & \beta_5 \left(\mathrm{Post}_t \times \left(\mathrm{Cash}^*_{it} - \mathrm{Cash}_{i,t-1}\right)\right) + \mu_i + \delta_{st} + \omega_{pt} + \varepsilon_{ispt} \qquad (2)\end{aligned}$$

where the key variable of interest is *Treat* × *Post* × ($Cash^*_{it} - Cash_{i,t-1}$), where Cash^*_{it} denotes the estimated optimal cash level for firm *i* at year *t*. [18] A positive gap between the optimal cash level and beginning-of-year cash holdings, $Cash^*_{it} - Cash_{i,t-1}$, indicates a cash shortfall, while a negative gap indicates excess cash.

The coefficient on *Treat* × *Post* × ($Cash^*_{it} - Cash_{i,t-1}$), β_1, is negative and statistically significant in the full sample, indicating that treated SOEs adjust more slowly toward target cash holdings after the reform. The slowdown in adjustment is concentrated among firms with excess cash. In the excess-cash subsample, the coefficient on this interaction term is negative and statistically significant. In the cash-shortfall subsample, the coefficient is small and statistically insignificant. This pattern suggests that treated firms do not mainly fail to build cash when they are below target. Instead, they appear less willing to reduce cash when they hold more than their estimated target level. This result is consistent with a more passive approach to liquidity management under compressed incentives.

Panel C examines whether investors value the additional cash held by treated firms. We estimate the marginal value of cash using the Faulkender and Wang (2006) framework:

$$\begin{aligned}\mathrm{r_{it}} - \mathrm{R_{mt}} = {} & \beta_0 + \beta_1(\Delta\mathrm{Cash_{it}} \times \mathrm{Treat_i} \times \mathrm{Post_t}) + \beta_2(\Delta\mathrm{Cash_{it}} \times \mathrm{Treat_i}) + \\ & \beta_3(\Delta\mathrm{Cash_{it}} \times \mathrm{Post_t}) + \beta_4(\mathrm{Treat_i} \times \mathrm{Post_t}) + \beta_5\Delta\mathrm{Cash_{it}} + \beta_6\left(\frac{\Delta \mathrm{E_{it}}}{\mathrm{M_{i,t-1}}}\right) + \\ & \beta_7\left(\frac{\Delta \mathrm{NA_{it}}}{\mathrm{M_{i,t-1}}}\right) + \beta_8\left(\frac{\Delta \mathrm{I_{it}}}{\mathrm{M_{i,t-1}}}\right) + \beta_9\mathrm{Cash_{i,t-1}} + \beta_{10}\left(\frac{\mathrm{NF_{it}}}{\mathrm{M_{i,t-1}}}\right) + \beta_{11}\left(\frac{\mathrm{L_{it}}}{\mathrm{M_{i,t-1}}}\right) + \\ & \beta_{12}(\mathrm{L_{it}} \times \Delta\mathrm{Cash_{it}}) + \beta_{13}\left(\mathrm{Cash_{i,t-1}} \times \Delta\mathrm{Cash_{it}}\right) + \beta_{14}\left(\frac{\mathrm{DIV_{it}}}{\mathrm{M_{i,t-1}}}\right) + \mu_i + \\ & \delta_{st} + \omega_{pt} + \varepsilon_{ispt} \qquad (3)\end{aligned}$$

[18] We discuss the calculation of optimal cash levels in Internet Appendix IA.5.

in which the dependent variable is the firm's excess stock return relative to a benchmark return. The key coefficient is on *Treat* × *Post* × Δ*Cash*, which captures whether the market value of incremental cash changes for treated SOEs after the reform. Control variables include changes in earnings(ΔE_{it}), net assets (ΔNA_{it}), interest expenses (ΔI_{it}), dividends (DIV_{it}), leverage (L_{it}), net financing (NF_{it}), and lagged cash, with firm-specific variables scaled by lagged market equity, following Chang et al. (2021).

Across specifications, this coefficient is negative and statistically significant. In Column (3), the estimate implies that the marginal value of an additional RMB of cash declines by 0.424 RMB for treated firms relative to the control group. [19] This result suggests that investors discount incremental cash held by treated firms after the pay-cap reform, consistent with the view that the additional liquidity is expected to be used less productively or retained as idle balances.

Panel D studies whether cash accumulation is accompanied by stronger operating activity or by other balance-sheet changes. The coefficient on *Treat* × *Post* is positive but statistically insignificant for *Sales Growth*, indicating that higher cash holdings are not associated with stronger revenue growth. By contrast, the coefficient is positive and significant for *Net Working Capital*, suggesting that more resources are tied up in inventories and receivables net of payables. The coefficient is also positive and significant for *New Financing*, indicating that treated firms raise more external funds after the reform.

Overall, Table 6 shows that treated SOEs hold more cash, adjust more slowly toward target cash levels, and experience a lower marginal value of cash after the pay-cap reform. The increase in cash is not accompanied by stronger sales growth and is associated with higher working capital and external financing. These findings are difficult to reconcile with the view that the additional cash mainly supports productive expansion. They are more consistent with a quieter managerial response in which firms retain liquidity, adjust less actively, and deploy resources less efficiently after monetary incentives are compressed.

4.6 Firm Performance

The preceding results show that treated SOEs exhibit lower work intensity, weaker CEO engagement, less risk-related activity, lower investment, weaker investment efficiency, and less active cash management after the pay-cap reform. We next examine whether these changes are associated with broader declines in firm performance.

[19] Our results on value of cash are also robust to using Fama-MacBeth regressions (not tabulated for brevity).

Table 7 reports estimates for two performance measures. The first is Tobin's Q, which captures market valuation and growth opportunities. The second is total factor productivity (TFP), estimated using the Levinsohn and Petrin (2003) method, which captures operating efficiency.[20] The coefficient on *Treat* × *Post* is negative and statistically significant at the 1% level in both specifications.

In Column (1), the coefficient on *Treat* × *Post* is −0.243 when Tobin's Q is the dependent variable. Relative to the sample mean of 2.013, this estimate implies a decline of about 12.1%. In Column (2), the coefficient is −0.169 when TFP is the dependent variable. Because the Levinsohn-Petrin TFP measure is reported in logs, the coefficient implies a decline of approximately 15.6% ($e^{-0.169}$–1).

These results indicate that treated firms experience lower market valuation and lower operating efficiency after the reform. Taken together with the earlier evidence, the performance results suggest that the post-reform changes are not merely accounting reallocations or harmless shifts in disclosure. They are associated with economically meaningful deterioration in firm value and productivity, consistent with the interpretation that pay compression weakens managerial initiative and has real effects on firm outcomes.

5. Heterogeneity and Robustness Tests

5.1 Heterogeneous Effects: Career Concerns, Market Discipline, and Governance

The baseline results show that treated SOEs exhibit lower work intensity, weaker CEO engagement, less risk-related activity, and lower investment after the pay-cap reform. We next examine whether these effects vary with alternative sources of managerial motivation and discipline. This analysis helps distinguish a general state-sector shock from an incentive-based response. If the results reflect weakened incentives, the effects should be stronger when implicit incentives or monitoring are weaker, or when the loss of performance-based rewards is more salient.

We focus on three dimensions: CEO career horizons, industry competition, and internal governance. For each dimension, we examine six outcomes that summarize the main behavioral margins in the baseline analysis: *Ln*(1 + *Overtime Keywords*), *Overtime Ratio*, *Investor Interactions* (0/1), *Unrelated Acquisition* (0/1), *Risk Tone*, and *Investment*. The heterogeneity variables are measured using pre-reform values in 2014. As a result, sample sizes

[20] We exclude *Size* from the regressions for *Tobin's Q* and *TFP* to prevent mechanical bias, as firm scale is inherently embedded in the construction of these dependent variables (specifically, the TFP framework utilizes the Levinsohn-Petrin method, which already conditions on firm scale). However, the results remain qualitatively robust to the inclusion of the size variable.

vary across panels depending on the availability of CEO age, industry concentration, and internal-control data.

We first examine CEO career concerns. In China's SOE setting, executives may be motivated not only by current compensation, but also by prospects for political promotion, administrative advancement, and future positions within the state sector (Cao et al., 2019, Guo et al., 2025). These incentives are likely to be weaker for older CEOs, whose remaining career horizons and promotion prospects are more limited (Belenzon et al., 2019). We therefore define Old CEO as an indicator equal to one for firms whose CEO age is above the sample median in 2014.

Panel A of Table 8 shows that the post-reform decline in managerial engagement and conservative decision-making is stronger among firms led by older CEOs. The coefficient on *Treat* × *Post* × *Old CEO* is negative across all six outcomes and statistically significant in five. The only exception is *Overtime Ratio*, where the coefficient is negative but statistically insignificant. The effects are especially clear for CEO-investor interactions, unrelated acquisitions, risk-related disclosure, and investment. This pattern is consistent with the view that career concerns partly substitute for monetary incentives. When promotion-related incentives are weaker, pay compression has larger behavioral effects.

We next examine industry competition. Competitive markets increase the value of managerial effort and strategic responsiveness because firms face stronger product-market pressure and have less room to absorb managerial slack (Schmidt, 1997; Cuñat and Guadalupe, 2009; Gartenberg and Wulf, 2020). At the same time, performance-based rewards may be especially salient in competitive industries, where managerial decisions are more closely tied to firm outcomes. We measure competition using the Herfindahl-Hirschman Index and define competitive industries as those with below-median HHI values in 2014.

Panel B shows that the effects of the reform are stronger in more competitive industries. The coefficient on *Treat* × *Post* × *Competitive Industries* is negative and statistically significant across most outcomes. Treated SOEs in competitive industries experience larger declines in overtime intensity, CEO engagement, unrelated acquisitions, risk-related disclosure, and investment. This evidence suggests that compensation compression is more consequential when managerial initiative and performance responsiveness are more valuable.

Finally, we examine internal governance. Strong internal controls can constrain managerial passivity and may partly substitute for monetary incentives by maintaining accountability through monitoring and organizational discipline (Bertrand and Mullainathan, 2003). We measure governance quality using the *Internal Control Index* and define *Weak*

Governance as an indicator equal to one for firms with below-median internal-control scores in 2014 (Chen et al., 2020).

Panel C shows that weak governance amplifies the post-reform decline along several behavioral margins. The coefficient on *Treat* × *Post* × *Weak Governance* is negative in all six specifications and statistically significant for *Overtime Ratio*, *Investor Interactions*, *Unrelated Acquisition*, and *Risk Tone*. The effects are weaker for media-based overtime and statistically insignificant for *Investment*. This pattern suggests that internal governance matters most for discretionary engagement and risk-related behavior. The weaker investment result may reflect the fact that major investment decisions are also shaped by approval processes, financing conditions, and political oversight, which can limit managerial discretion regardless of internal-control quality.

Taken together, the heterogeneity results support the incentive interpretation. The effects of pay compression are stronger when alternative sources of motivation and discipline are weaker, as in firms led by older CEOs and firms with weaker internal governance. They are also stronger when performance incentives are likely more salient, as in competitive industries. These patterns suggest that career concerns and governance mechanisms can partly discipline managers, but they do not fully offset the loss of high-powered monetary incentives under the pay-compression regime.

5.2 Implementation Strictness and State-Sector Heterogeneity

We next examine whether the baseline effects are concentrated in specific implementation environments or particular segments of the state sector. These tests complement the main heterogeneity analysis in Section 5.1. Rather than testing the incentive mechanism directly, they assess whether the results are driven by stricter local enforcement, central SOEs, strategic industries, high initial pay gaps, or politically connected managers.

We first examine provincial enforcement strictness. If the reform had stronger effects where implementation was more restrictive, treated firms in stricter provinces should exhibit larger post-reform declines in engagement, risk-related behavior, and investment. Internet Appendix IA.7 tests this prediction by interacting *Treat* × *Post* with an indicator for strict provincial enforcement. The triple-interaction estimates are mixed and generally statistically insignificant, while the baseline *Treat* × *Post* coefficients remain negative and significant across the main behavioral outcomes. This pattern suggests that the estimated effects are not confined to provinces with stricter measured implementation. One interpretation is that formal provincial rules measure enforcement intensity with error. Another is that the pay-compression

regime operated broadly across the state sector, making cross-province differences less informative than the SOE versus non-SOE comparison.

We then examine heterogeneity within SOEs. Internet Appendix IA.8 considers strategic industries, defined following Lai et al. (2023) as sectors with greater state influence or policy importance. The evidence is mixed. The triple interaction is negative for overtime-related media coverage and investment, suggesting stronger declines on these margins, but it is not consistently negative across outcomes and is positive for unrelated acquisitions. Thus, strategic-industry status does not systematically explain the baseline effects either.

Internet Appendix IA.9 distinguishes central and local SOEs. This test is useful because the formal reform was directed primarily at centrally administered SOEs, while our main design treats the reform as affecting the broader state sector. The estimates show that both central and local SOEs move in the same direction after the reform. The coefficients on *Central SOE* × *Post* and *Local SOE* × *Post* are negative for the six main behavioral outcomes, and most are statistically significant. This evidence supports the interpretation that the reform was not limited to central SOEs and that the baseline results are not driven solely by one ownership tier.

Internet Appendix IA.10 examines whether the response is concentrated among SOEs with larger pre-policy executive-worker pay gaps. These firms were more likely to face binding compensation constraints under the reform. Both high-gap and low-gap SOEs exhibit negative post-reform changes across the main behavioral outcomes. Although some coefficients are larger for high-gap SOEs, the overall pattern indicates that the baseline response is not confined to firms with the largest initial internal pay disparities. We return to the intensity of compensation compression more directly in Section 5.3 using a continuous exposure design.

Finally, Internet Appendix IA.11 examines managers' political connections. Politically connected executives may be less sensitive to monetary pay because they value political advancement, but they may also be more responsive to central discipline. The results show that SOEs with and without politically connected managers both reduce discretionary engagement, risk-related activity, and investment after the reform. The evidence for effort-based proxies is weaker among politically connected SOEs, but the core discretionary outcomes remain negative and significant. Political connections therefore do not appear to insulate managers fully from the effects of pay compression.

Overall, the evidence in this section suggests that the baseline results broadly hold across the state sector. The estimated effects are not concentrated only in strict-enforcement provinces, central SOEs, strategic industries, high-pay-gap firms, or firms without politically connected managers. These tests support the interpretation of the reform as a broad change in the state-

sector compensation environment rather than a narrow shock affecting only one subset of SOEs.

5.3 Exposure-Based Evidence: Predicted Compensation Cuts

The baseline DID estimates capture the broad reduced-form effect of exposure to the post-2015 pay-compression regime. We next ask whether the intensity of compensation compression matters. This analysis uses cross-firm variation in pre-policy executive-worker pay multiples to predict which SOEs faced larger compensation cuts after the reform. The purpose is not to replace the baseline DID design, but to provide supporting evidence that firms more exposed to the binding pay constraint exhibit stronger behavioral and real-side responses.

The logic is straightforward. The 2015 reform linked executive compensation to average employee wages. SOEs with larger pre-policy executive-worker pay multiples should therefore have faced more binding compensation constraints when the reform was implemented. Consistent with this logic, Figure 2 shows that SOEs with larger pre-policy pay gaps experienced larger subsequent reductions in log executive compensation.

We implement a continuous-exposure Two-Stage Least Squares (2SLS) design. The endogenous variable is *PayCut*, defined as $-\Delta Ln(Compensation)$, so that larger values indicate larger reductions in executive compensation. We instrument *PayCut* with the interaction of the firm's 2014 executive-worker pay multiple, *Treat*, and *Post*. The first-stage specification is:

$$PayCut_{it} = \alpha_i + \delta_{j,t} + \lambda_{p,t} + \pi\left(PayGap_{i,2014} \times Treat_i \times Post_t\right) + \gamma' Z_{it} + u_{it} \quad (4)$$

where $PayGap_{i,2014}$ is the firm's pre-reform executive–worker pay multiple. A positive value of π indicates that treated firms with larger pre-policy pay gaps experienced larger compensation cuts after the reform. In the second stage, we replace realized compensation cuts with their fitted values from the first stage:

$$Y_{it} = \alpha_i + \delta_{j,t} + \lambda_{p,t} + \theta \widehat{PayCut}_{it} + \gamma' Z_{it} + \varepsilon_{it} \quad (5)$$

where Y_{it} denotes corporate behavior and firm outcomes.

Because *PayCut* is defined as the negative change in log compensation, a negative value of θ means that larger predicted compensation cuts are associated with lower values of the outcome. For example, when the dependent variable is investment, a negative coefficient indicates that firms predicted to experience larger pay cuts also reduce investment more.

Internet Appendix IA.12 reports the results. The first stage confirms that the instrument predicts compensation cuts. The coefficient on $PayGap_{i,2014} \times Treat_i \times Post_t$ is positive and statistically significant across specifications, and the Kleibergen-Paap F-statistics exceed conventional weak-instrument thresholds. The second-stage estimates show that larger predicted pay cuts are associated with lower CEO-investor interactions, fewer unrelated

acquisitions, less risk-related language in MD&A disclosures, and lower investment. They are also associated with higher cash holdings and lower Tobin's Q.

We interpret this evidence as complementary to the baseline results. The 2SLS design helps separate variation in compensation compression from average SOE exposure to the reform, but it does not rule out all shocks correlated with pre-policy pay gaps. The results therefore should not be read as a stand-alone causal design. Rather, they support the paper's main interpretation by showing that the behavioral and real-side responses are stronger among firms predicted to face larger pay reductions under the reform.

Hence, the evidence in this section points to both a broad state-sector response and an intensive-margin role for pay compression. Treated SOEs respond to the reform across multiple behavioral margins, and these responses are stronger among firms more exposed to the binding compensation constraint.

5.4 Robustness Tests and Identification

We next conduct a series of tests to assess whether the baseline results are robust to alternative identification concerns, sample definitions, measurement choices, estimation methods, and contemporaneous shocks. These tests focus on the six main behavioral outcomes: *Ln*(1 + *Overtime Keywords*), *Overtime Ratio*, *Investor Interactions* (0/1), *Unrelated Acquisition* (0/1), *Risk Tone*, and *Investment*.

We first examine the parallel-trends assumption. Figure 3 reports event-study estimates with 95% confidence intervals, using 2014 as the omitted year. The pre-reform coefficients are statistically indistinguishable from zero, while the post-reform coefficients become negative and statistically significant. This pattern supports the identifying assumption that treated SOEs and non-SOEs were not already following differential trends before the reform, and that the decline in behavioral outcomes emerges after the pay-cap regime is implemented.

We next conduct placebo tests based on randomly assigned treatment status. We randomly reassign treatment across firms 1,000 times and re-estimate the baseline model in each iteration. Figure 4 plots the distribution of placebo estimates. The actual treatment effects, denoted by a vertical dashed line, lie far in the tails of the placebo distributions, with p-values below 0.001 for the main outcomes. This evidence suggests that the estimated effects are unlikely to arise from random variation in treatment assignment. As an additional diagnostic, we also simulate a hypothetical 2013 policy shock using only pre-policy data from 2011 to 2014. The placebo interaction is statistically insignificant, which further reduces the concern that the results reflect anticipatory behavior or latent pre-reform divergence (results not tabulated for brevity).

Because SOE status is not randomly assigned, we also address selection on observable

characteristics. Following Tian et al. (2024), we apply entropy balancing to reweight non-SOE observations so that the control group matches treated SOEs on observable covariate moments. Table 9 shows that the reweighted estimates remain consistent with the baseline results. Internet Appendix IA.13 confirms covariate balance after reweighting, IA.14 shows no pre-trends in the reweighted sample, and IA.15 reports placebo tests that are consistent with the main analysis. We also implement one-to-one nearest-neighbor matching based on propensity scores, and the matched-sample estimates remain qualitatively similar.

A related concern is that the pay-cap reform coincided with changes in SOE financing conditions. If treated SOEs received cheaper credit after 2015, the observed changes in investment, risk-related activity, and managerial behavior could partly reflect credit-market conditions rather than incentive effects. We address this concern in three ways. First, Internet Appendix IA.16 adds time-varying credit controls, including *Cost of Debt* and *Credit Spread*, and includes issuer-by-year fixed effects to absorb bond-market-specific shocks. The *Treat* × *Post* coefficients remain negative and statistically significant across the six main behavioral outcomes. Second, Internet Appendix IA.17 confirms that treated SOEs experienced lower debt financing costs after the reform, but this change does not absorb the baseline effects once financing conditions are controlled for. Third, Internet Appendix IA.18 tests whether the effects are concentrated among firms with pre-policy bond-market access. The triple interactions with bond-issuer status are generally small and statistically insignificant, while the baseline *SOE* × *Post* effects remain negative and significant. These tests suggest that the behavioral results are not driven primarily by improved SOE credit access or bond-market exposure.

We then examine robustness to alternative outcome definitions, estimators, and sample restrictions. Internet Appendix IA.19 addresses measurement concerns about the satellite-based overtime measure by separating shift-intensive industries from industries where nighttime work is less common. The decline in *Overtime Ratio* remains negative in both subsamples and is larger in shift-intensive industries, where nighttime production is more plausible. Internet Appendix IA.20 replaces the baseline CEO engagement indicator with *Ln*(1 + *Board Meetings*) and *Ln*(1 + *Investor Interactions*). The coefficients on *Treat* × *Post* remain negative and statistically significant, indicating that the engagement result is not specific to the binary CEO-investor interaction measure.

The results are also robust to alternative estimation methods. Internet Appendix IA.21 reports Poisson and logit models for count and binary outcomes and finds similar conclusions. Internet Appendix IA.22 applies a double machine-learning framework with Lasso selection to address concerns about omitted variables and data-driven covariate choice. The estimated *Treat*

× *Post* coefficients remain negative and statistically significant across the six main behavioral outcomes. Internet Appendix IA.23 re-estimates the baseline specifications after excluding industries in the top quartile of revenue-weighted SOE presence in 2014. The results remain negative and statistically significant, suggesting that the findings are not driven by industries dominated by SOEs before the reform.

We also address contemporaneous policy shocks. The 2015 pay-cap reform occurred during a broader period of anti-corruption enforcement and SOE deleveraging pressure. Internet Appendix IA.24 allows the treatment effect to vary with provincial anti-corruption intensity and firm-level deleveraging exposure. Anti-corruption intensity is measured using the number of provincial anti-corruption cases in 2014, and deleveraging exposure is measured using firms' 2014 leverage ratios. Both variables are standardized before being interacted with *Treat* × *Post*, so the baseline *Treat* × *Post* coefficient captures the estimated pay-cap effect at average levels of these concurrent policy exposures.[21] The estimates remain negative and statistically significant across all six behavioral outcomes. The interaction terms are generally small and insignificant, although anti-corruption intensity is associated with stronger declines in unrelated acquisitions and risk-related disclosure, and deleveraging exposure modestly attenuates the investment decline. These patterns suggest that contemporaneous policies may affect specific margins, but they do not account for the main behavioral response.

Finally, Figure 5 reports a specification-curve analysis. We re-estimate the DID models across 144 specifications that vary fixed effects, clustering, controls, sample restrictions, matching methods, and adjustments for concurrent reforms. The *Treat* × *Post* estimates remain negative and statistically significant at the 1% level across all specifications, indicating that the results are not driven by a particular modeling choice.

As an aggregate validation exercise, we construct a composite behavioral index using the first principal component of the six main behavioral outcomes. The principal-component weights are estimated using complete-case observations from the 2011–2014 pre-treatment period, and the sign of the index is normalized so that higher values indicate greater managerial engagement. Internet Appendix IA.25 reports the construction of the index. The first principal component explains 20.1% of the total variance, with the largest loadings on *Investor Interactions* (0/1), *Unrelated Acquisition* (0/1), and *Risk Tone*. Because the index requires non-missing values for all six outcomes, the sample declines from 18,825 to 14,396 firm-year observations.

[21] As an additional diagnostic, we examine the marginal effects of *Treat* × *Post* across the distributions of anti-corruption intensity and deleveraging exposure. The results are consistent with the centered triple-difference estimates.

Internet Appendix IA.26 shows that the baseline DID estimate for the composite index is negative and statistically significant. The dynamic, placebo, and 2SLS tests yield similar conclusions, and Internet Appendix IA.27 shows that the index follows the same heterogeneity patterns as the outcome-by-outcome analysis. We interpret this index as a summary validation measure rather than a core outcome, since it explains only part of the variation and loads most heavily on a subset of the proxies. Nevertheless, it provides useful corroborating evidence that the baseline findings are not driven by a single behavioral measure.

Overall, these tests support the robustness of the baseline evidence. The results are not explained by differential pre-trends, random assignment artifacts, observable selection, credit-market changes, alternative outcome definitions, nonlinear estimators, machine-learning-based covariate selection, sample restrictions, contemporaneous SOE policies, or a narrow specification choice. These tests do not eliminate every concern inherent in a broad policy setting, but they strengthen the interpretation that treated SOEs experienced a post-reform decline in managerial engagement, risk-related activity, and investment following exposure to the pay-compression regime.

6. Mechanisms: Selection, Incentives, and Reallocation

The baseline results show that treated SOEs exhibit lower engagement, less risk-related activity, weaker investment, more cash retention, and lower firm performance after the pay-cap reform. These patterns are consistent with a quiet-life response, but they do not by themselves identify the mechanism. Pay compression may affect firm behavior by changing the composition of managers who remain in treated firms, by weakening the formal link between pay and performance, by inducing substitution toward private benefits, or by shifting managerial attention toward outcomes that remain politically visible or institutionally rewarded. This section examines these channels. We first study executive turnover and selection. We then examine pay-performance sensitivity, observable perks and tunneling, ESG and CSR outcomes, and labor-related outcomes.

6.1 Executive Selection: Do High-Performing Managers Exit?

The first mechanism we examine is executive selection. If pay compression reduces the returns to managerial talent, executives with stronger outside options may be more likely to leave treated SOEs for positions where compensation is more market-based or performance-sensitive. In that case, the post-reform decline in effort, risk-related activity, and investment may reflect not only weaker incentives among incumbent managers, but also a change in the

composition of managers who remain. This channel is consistent with evidence that compensation regulation can affect executive retention (Kleymenova and Tuna, 2021) and with evidence on executive mobility following earlier pay regulation in China (Nanda et al., 2024).[22]

We examine this channel using CEO turnover. Table 10 reports two sets of tests. The first follows Nanda et al. (2024) and classifies CEO turnover as voluntary when the CEO departs because of resignation, term expiration, personal reasons, or related CSMAR categories. We exclude moves to affiliated firms because these moves are more likely to reflect internal reassignment or promotion within the same corporate group rather than exit from the treated firm's opportunity set. Columns (1) and (2) show that voluntary CEO turnover increases after the pay-cap reform. In the linear probability specification, the coefficient on *Treat* × *Post* is 0.025 and statistically significant. Relative to the pre-reform mean turnover rate of 5.3%, this estimate implies an increase of about 47.2%. This effect is larger than the 20% increase documented by Nanda et al. (2024) following China's 2009 pay regulation, suggesting that the 2015 reform had stronger consequences for executive retention.[23]

The second test focuses more directly on whether better-performing executives are more likely to leave. Following Lin et al. (2022), we classify turnover as occurring after good prior performance when the CEO is replaced despite prior-year ROA above the industry-adjusted median. Columns (3) and (4) show that this form of turnover also increases significantly among treated SOEs after the reform. In the linear probability specification, the coefficient on *Treat* × *Post* is 0.015 and statistically significant. This pattern is consistent with adverse selection: after monetary rewards are compressed, CEOs with stronger recent performance appear more likely to exit treated firms.

We next ask whether executive selection fully explains the baseline results. To address this concern, we match firms on their ex-ante turnover probability using pre-reform data from 2011 to 2014. Internet Appendix IA.28 describes the matching procedure and shows that the matched samples are balanced on baseline turnover risk (Panel B). Table 11 then re-estimates the main specifications using these matched samples. The negative effects on effort, risk-related activity, and investment remain statistically and economically significant.

These results suggest that executive selection is part of the mechanism, but not the whole

[22] An example of this trend is the 2015 departure of 50-year-old Gao Yonggang from a central SOE to join Semiconductor Manufacturing International Corporation (SMIC, stock code: 00981.HK; 688981), a company listed in both Hong Kong and Shanghai, as CFO and Vice President. Gao Yonggang cited SMIC's more market-driven operating mechanisms and full decision-making autonomy as key factors in his decision. For more information, please see http://politics.people.com.cn/n/2015/0504/c1001-26945703.html (in Chinese).

[23] This estimate may be conservative, as it compares post-policy turnover to the 2011–2014 baseline period. If SOE turnover rates were already elevated prior to the policy, our analysis would understate the true effect. In practice, the data indicate higher turnover among SOE CEOs in the post-policy period (2016–2019), consistent with selection out of the state sector.

story. The reform increases CEO turnover, including turnover among high-performing executives, which is consistent with the idea that pay compression makes treated SOEs less attractive to more marketable managers. At the same time, the baseline behavioral effects remain after matching on ex-ante turnover risk. Additional evidence in Internet Appendix IA.29, which restricts the sample to firms with the same CEO in 2014 and 2015, also shows negative post-reform effects across the main behavioral outcomes. The evidence therefore points to both selection and incentive effects: pay compression changes who remains in treated firms, but it also appears to weaken engagement among managers who stay.

6.2 Incentive Structure: Does Pay Become Less Tied to Performance?

A second mechanism is the weakening of formal incentive alignment. The pay-cap reform may have reduced not only the level of executive compensation, but also the extent to which compensation responds to firm performance. This distinction matters because pay-performance sensitivity is central to the incentive role of executive compensation. If the reform weakened the link between pay and performance, managers would have faced lower rewards for effort, difficult decisions, and value-enhancing investment even if they remained in their positions.

We test this channel by estimating whether pay-performance sensitivity declines for treated SOEs after the reform. Following Bae et al. (2024), we estimate the following triple-difference specification:

$$Ln(Compensation)_{it} = \alpha_i + \delta_{j,t} + \lambda_{p,t} + \lambda_1(Treat_i \times Post_t \times Performance_{it}) + \lambda_2(Post_t \times Performance_{it}) + \lambda_3(Treat_i \times Performance_{it}) + \lambda_4(Treat_i \times Post_t) + \gamma' Z_{it} + \varepsilon_{it} \quad (6)$$

where the dependent variable is the natural logarithm of CEO compensation. Performance is measured using either ROA or return on sales. The coefficient of interest is λ_1, which captures whether the sensitivity of CEO compensation to firm performance changes for treated SOEs after the reform relative to non-SOEs.

Table 12 reports the results. The coefficient on *Treat* × *Post* × *Performance* is negative and statistically significant using both performance measures. In Column (1), where performance is measured using ROA, the coefficient is −0.895. In Column (2), where performance is measured using ROS, the coefficient is −0.370. These estimates indicate that CEO compensation becomes less responsive to firm performance in treated SOEs after the reform.

This result supports the incentive interpretation of the baseline findings. In addition to reducing executive pay levels, the pay reform also weakened the formal link between

performance and compensation. Lower pay-performance sensitivity reduces the private return to managerial effort and may therefore contribute to the observed declines in engagement, risk-related activity, investment, and investment efficiency. The evidence is consistent with a pay-compression regime that lowered incentive intensity, creating conditions under which managers had weaker incentives to pursue costly or uncertain value-enhancing actions.

6.3 Alternative Channels: Rent-Seeking

A natural alternative explanation is that executives respond to lower formal compensation by substituting toward informal private benefits. This channel is especially relevant in China's SOE setting because prior evidence shows that earlier pay restrictions led executives to rely more on perks and other less transparent forms of compensation (Bae et al., 2024). If the 2015 reform operated in the same way, the decline in formal incentives could be accompanied by higher perk consumption, tunneling, or other forms of private-benefit extraction.

We test this possibility using two observable measures of rent-seeking. The first is *Perk*, measured as discretionary expenses scaled by sales, following Gul et al. (2011). The second is *Tunneling*, proxied by the natural logarithm of net other receivables, following Jiang et al. (2010). These measures do not capture all possible informal benefits, but they cover two standard margins through which managers or controlling parties may extract private benefits.

Table 13 provides little evidence that treated executives substitute toward these observable rent-extraction channels. The coefficient on *Treat* × *Post* is negative and statistically significant for both outcomes. In Column (1), perk consumption declines by 0.001 of sales, which is about 12.5% relative to the sample mean of 0.008. In Column (2), tunneling declines by 0.378 log points, corresponding to an approximate 31.5% reduction.

These results help distinguish the 2015 reform from earlier pay-regulation episodes. In the earlier setting studied by Bae et al. (2024), executives had greater scope to offset formal pay restrictions through informal benefits. By contrast, the 2015 reform was implemented alongside a broader austerity and anti-corruption campaign that likely constrained perks, expense accounts, and other informal compensation channels. The negative coefficients in Table 13 are consistent with that institutional environment.

The evidence therefore does not support the view that the baseline results are driven by substitution from formal compensation into observable private benefits. Instead, the decline in perks and tunneling strengthens the quiet-life interpretation. When both monetary incentives and private-benefit channels are constrained, affected managers appear to respond less through active rent extraction and more through lower initiative, more conservative policies, and

weaker responsiveness to value-enhancing opportunities.

6.4 Politically Salient Outcomes

We next examine whether treated firms place greater weight on politically salient non-financial outcomes after the pay-cap reform. The baseline results suggest weaker engagement with performance-oriented activities, but they do not imply that managers become inactive. When monetary incentives tied to commercial performance are compressed, managers may redirect attention toward outcomes that remain visible to political principals, regulators, employees, or other stakeholders. This logic is consistent with multitask agency models, in which weakening incentives along one dimension can shift effort toward other dimensions that remain observable or rewarded (Holmström and Milgrom, 1991). In the SOE setting, such outcomes plausibly include ESG performance and CSR activity.

Table 14 examines this possibility. The dependent variables are Bloomberg ESG scores and Hexun CSR scores. The coefficient on *Treat* × *Post* is positive and statistically significant for both outcomes. In Column (1), treated SOEs' ESG scores increase by 0.900 after the reform relative to non-SOEs. In Column (2), CSR scores increase by 0.523. These estimates indicate that treated firms improve measured ESG and CSR performance after the pay-cap reform.

We interpret these results cautiously. The evidence is consistent with greater attention to politically or socially salient outcomes, but it does not show that ESG or CSR activity substitutes for financial performance in a mechanical sense. Nor does it establish that formal ESG mandates drive the effect. ESG and CSR scores capture broad firm-level outcomes and disclosures, and improvements in these measures may reflect several forces, including stakeholder engagement, reputational concerns, compliance efforts, or changes in disclosure.

Internet Appendix IA.30 examines whether the ESG and CSR effects are stronger in regions with higher measured ESG-mandate intensity, using the share of green-related keywords in prefecture-level government work reports. The evidence does not support a clean mandate-channel interpretation. The triple interaction is negative for ESG and statistically insignificant for CSR. This pattern may reflect measurement error in the mandate proxy, broad-based ESG pressure across regions, or the possibility that the post-reform ESG and CSR increases are not driven by formal local mandates.

The main takeaway is therefore narrower. Treated SOEs increase measured ESG and CSR performance after the pay-cap reform, even as they reduce engagement, investment, risk-related activity, and productivity. This pattern is consistent with a shift in managerial attention toward politically salient outcomes under compressed monetary incentives. It should not be

interpreted as proof that managers reallocate effort from financial performance to ESG or CSR, or that a specific mandate channel explains the result.

6.5 Labor-Related Outcomes

Labor outcomes provide another setting in which to examine whether treated firms place greater weight on politically salient margins after the pay-cap reform. Employment stability is especially salient in China's political economy, and this salience is likely stronger for SOEs because job creation and labor stability are closely connected to social stability and local policy priorities. If pay compression weakens incentives to pursue productivity-enhancing actions, managers may become more willing to preserve or expand employment even when doing so is not accompanied by proportional gains in output.

This analysis is related to Cao et al. (2026), who interpret reductions in abnormal labor investment after the reform as evidence of improved internal governance. Our focus is different. Instead of estimating abnormal labor investment, we examine three direct labor outcomes: *Employment*, *Labor productivity*, and *Wage-bill intensity*. *Employment* is the natural logarithm of the number of employees. *Labor productivity* is sales divided by the number of employees. *Wage-bill intensity* is total salary expense scaled by total assets. These variables capture the level, productivity, and compensation intensity of firms' labor input.

Table 15 reports the results. The coefficient on *Treat* × *Post* is positive and statistically significant for *Employment*. The estimate of 0.075 implies that treated SOEs increase headcount after the pay-cap reform relative to non-SOEs. By contrast, the coefficients on *Treat* × *Post* are negative and statistically significant for both *Labor Productivity* and *Wage-bill intensity*. The labor-productivity coefficient is −0.194, and the wage-bill-intensity coefficient is −0.003.

This combination of results suggests that treated SOEs expand employment, but this expansion is not matched by proportional gains in output per worker. The decline in *Labor productivity* suggests weaker labor efficiency, while the decline in *Wage-bill intensity* indicates that the increase in headcount does not translate into a larger wage bill relative to firm assets. One interpretation is that treated firms expand lower-productivity or lower-compensation employment. Another is that they preserve or add labor for reasons other than short-run productivity maximization. In either case, the pattern is more consistent with visible employment expansion than with productivity-enhancing labor investment.

Internet Appendix IA.31 examines whether the labor effects are stronger in regions with higher employment-mandate intensity, measured using the share of employment-related

keywords in prefecture-level government work reports. The triple-interaction terms are statistically insignificant for *Employment*, *Labor Productivity*, and *Wage-bill intensity*. This evidence does not support a clean formal mandate-channel interpretation. It suggests instead that the labor effects are broad among treated SOEs rather than concentrated in regions with stronger measured employment mandates. Because the specifications include province-by-year fixed effects, the results are also unlikely to be driven solely by local labor-market shocks.

Overall, the labor results reinforce the broader mechanism evidence. Treated firms increase employment, a politically salient outcome, while experiencing lower labor productivity and lower wage-bill intensity. We interpret this pattern cautiously. It does not prove that managers deliberately reallocate effort from financial performance to employment objectives, nor does it identify a specific employment mandate channel. It is nevertheless consistent with a post-reform shift toward visible labor outcomes under compressed monetary incentives.

Taken together, the mechanisms in Section 6 point to a multifaceted response to pay compression. The reform is followed by higher turnover among high-performing executives, weaker pay-performance sensitivity, and no evidence of substitution into observable perks or tunneling. At the same time, treated firms increase measured ESG and CSR performance and expand employment. These patterns suggest that the baseline effects are not driven solely by talent selection or private-benefit substitution. Rather, they are consistent with a broader incentive-erosion mechanism in which pay compression reduces engagement with performance-oriented activities while increasing attention to outcomes that remain politically visible or institutionally rewarded.

7. Conclusions

This paper studies the real effects of executive pay regulation using China's 2015 executive pay-cap reform for SOEs. The reform provides a useful setting because it substantially compressed formal monetary incentives while limiting common compensation-substitution channels. Using a difference-in-differences design that compares SOEs with non-SOEs around the reform, we document broad changes in managerial behavior and firm outcomes consistent with a quiet-life response.

Treated firms reduce work intensity, CEO engagement, unrelated acquisitions, risk-related disclosure language, investment, and investment efficiency. They also accumulate more cash, adjust more slowly toward target cash levels, and experience a lower marginal value of cash. These changes are accompanied by declines in Tobin's Q and total factor productivity. The

pattern is economically meaningful and is stronger among older CEOs, in more competitive industries, and in firms with weaker internal governance. These heterogeneity results suggest that career concerns, market discipline, and internal monitoring can partly discipline managers, but do not fully substitute for high-powered monetary incentives when compensation is compressed.

The mechanism evidence clarifies the nature of the response. CEO turnover rises after the reform, especially among high-performing executives, indicating that executive selection contributes to the real effects of pay compression. CEO pay also becomes less sensitive to firm performance, consistent with weaker formal incentive alignment. At the same time, treated firms do not increase observable perks or tunneling; both measures decline. This evidence is difficult to reconcile with a simple rent-substitution story. Instead, the dominant response appears to be more passive: lower initiative, more conservative policies, slower adjustment, and weaker responsiveness to investment opportunities.

The evidence also suggests that treated firms increase attention to politically salient outcomes. ESG and CSR scores rise, employment increases, and labor productivity falls. We interpret these results cautiously. They are consistent with greater attention to visible stakeholder-oriented outcomes under compressed monetary incentives, but they do not establish a specific mandate channel or imply that managers simply reallocate effort from financial to social objectives.

The broader implication is that the costs of pay regulation may arise through a less visible agency margin. Traditional concerns about executive pay restrictions often emphasize substitution into perks, tunneling, or other private benefits. We find little evidence of such behavior in this setting. Instead, pay compression appears to generate passive agency costs: foregone initiatives, delayed adjustments, weaker investment responsiveness, higher cash retention, and lower productivity. These costs are harder to observe than active rent extraction, but they can matter for firm value and resource allocation.

The policy implication is not that executive pay regulation is necessarily undesirable. Pay caps, bonus restrictions, and pay-ratio taxes may reduce rents, address distributional concerns, or improve perceptions of fairness. In addition, we do not suggest that magnitudes from China's state-controlled sector be mechanically extrapolated to market economies with different governance institutions and managerial labor markets. The main lesson is narrower. When pay constraints are binding and substitution channels are limited, policymakers should account for the effects on managerial effort, executive retention, investment, cash policy, and productivity. Effective pay regulation may require complementary governance mechanisms that preserve

productive managerial engagement. Without such substitutes, pay compression can shift firms toward a quieter form of agency cost: less initiative, more caution, and greater emphasis on objectives that are visible or institutionally rewarded but not necessarily value-enhancing.

References

Abudy, M. M., Amiram, D., Rozenbaum, O., Shust, E., 2020. Do executive compensation contracts maximize firm value? Evidence from a quasi-natural experiment. *Journal of Banking & Finance* 114, 105787.

Adams, R.B., Almeida, H., Ferreira, D., 2005. Powerful CEOs and their impact on corporate performance. *Review of Financial Studies* 18(4), 1403–1432.

Bae, K. H., Gong, Z., Tong, W. H., 2024. Restricting CEO pay backfires: Evidence from China. *Journal of Business Finance & Accounting* 51, 1015-1045.

Balogh, A., Wright, D., Zein, J., 2026. Does stakeholder outrage determine executive pay? *Review of Corporate Finance Studies* 15(1), 227-268.

Bebchuk, L.A., Cremers, K.J.M., Peyer, U.C., 2011. The CEO pay slice. *Journal of Financial Economics* 102, 199-221.

Belenzon, S., Shamshur, A., Zarutskie, R., 2019. CEO's age and the performance of closely held firms. *Strategic Management Journal* 40(6), 917-944.

Bertrand, M., Mullainathan, S., 2003. Enjoying the quiet life? Corporate governance and managerial preferences. *Journal of Political Economy* 111(5), 1043-1075.

Bird, A., 2018. Taxation and executive compensation: Evidence from stock options. *Journal of Financial Economics* 127(2), 285-302.

Bornemann, T., Jacob, M., Sailer, M., 2023. Do corporate taxes affect executive compensation? *The Accounting Review* 98(2), 31-58.

Cai, J., Walkling, R. A., 2011. Shareholders' say on pay: Does it create value? *Journal of Financial and Quantitative Analysis* 46(2), 299–339.

Cao, J., Hasan, I., Huang, Z., Zhao, J., 2026. Pay restrictions and labor investment. *Journal of Corporate Finance* 99, 102990.

Cao, X., Lemmon, M., Pan, X., Qian, M., Tian, G., 2019. Political promotion, CEO incentives, and the relationship between pay and performance. *Management Science* 65(7), 2947–2965.

Cebon, P., Hermalin, B.E., 2015. When less is more: The benefits of limits on executive pay. *Review of Financial Studies* 28(6), 1667-1700.

Chaigneau, P., Edmans, A., Gottlieb, D., 2025. A theory of fair CEO pay. *American Economic Review: Insights* 7(3), 306-324.

Chang, Y., Pan, X., Wang, J., Zhou, Q., 2021. Depoliticization and corporate cash holdings: Evidence from the mandated resignation of directors in China. *Journal of Corporate Finance* 69, 102004.

Chang, W., Dambra, M., Schonberger, B., Suk, I., 2023. Does sensationalism affect executive compensation? Evidence from pay ratio disclosure reform. *Journal of Accounting Research* 61(1), 187–242.

Chen, H., Yang, D., Zhang, J. H., Zhou, H., 2020. Internal controls, risk management, and cash holdings. *Journal of Corporate Finance* 64, 101695.

Colonnello, S., Koetter, M., Wagner, K., 2023. Compensation regulation in banking: Executive director behavior and bank performance after the EU bonus cap. *Journal of Accounting and Economics* 76(1), 101576.

Cuñat, V., Guadalupe, M., 2009. Globalization and the provision of incentives inside the firm: The effect of foreign competition. *Journal of Labor Economics* 27(2), 179-212.

Dessaint, O., Matray, A., 2017. Do managers overreact to salient risks? Evidence from hurricane strikes. *Journal of Financial Economics* 126, 97-121.

Edmans, A., Gosling, T., Jenter, D., 2023. CEO compensation: Evidence from the field. *Journal of Financial Economics* 150, 103718.

Fama, E., 1980. Agency problems and the theory of the firm. *Journal of Political Economy* 88(2), 288-307.

Faulkender, M., Wang, R., 2006. Corporate financial policy and the value of cash. *Journal of Finance* 61, 1957-1990.
Ferri, F., Maber, D. A., 2013. Say on pay votes and CEO compensation: Evidence from the UK. *Review of Finance* 17, 527-563.
Ferri, F., Zheng, R., Zou, Y., 2018. Uncertainty about managers' reporting objectives and investors' response to earnings reports: Evidence from the 2006 executive compensation disclosures. *Journal of Accounting and Economics* 66(2–3), 339–365.
Firth, M., Leung, T.Y., Rui, O.M., Na, C., 2015. Relative pay and its effects on firm efficiency in a transitional economy. *Journal of Economic Behavior & Organization* 110, 59-77.
Gao, Q., Lin, M., Sias, R., 2023. Words matter: The role of readability, tone, and deception cues in online credit markets. *Journal of Financial and Quantitative Analysis* 58(1), 1-28.
Gartenberg, C., Wulf, J., 2020. Competition and pay inequality within and between firms. *Management Science* 66(12), 5485-6064.
Gibbons, R., Murphy, K.J., 1992. Optimal incentive contracts in the presence of career concerns: Theory and evidence. *Journal of Political Economy* 100(3), 468–505.
Griffin, J.M., Liu, C., Shu, T., 2022. Is the Chinese anticorruption campaign authentic? Evidence from corporate investigations. *Management Science* 68, 7248–7273.
Gul, F. A., Cheng, L. T. W., Leung, T. Y., 2011. Perks and the informativeness of stock prices in the Chinese market. *Journal of Corporate Finance* 17, 1410–1429.
Guo, P., Shi, G., Tian, G.G., 2025. Nexus effect: unraveling the impact of political patronage connections on corporate investment. *Journal of Financial and Quantitative Analysis* 60(8), 4035-4064.
Hicks, J.R., 1935. Annual survey of economic theory: The theory of monopoly. *Econometrica* 3(1), 1-20.
Holmström, B., 1999. Managerial incentive problems: A dynamic perspective. *Review of Economic Studies* 66(1), 169-182.
Holmström, B., Milgrom, P., 1991. Multitask principal-agent analyses: Incentive contracts, asset ownership, and job design. *Journal of Law, Economics, & Organization* 7, 24–52.
Hou, F., Mengistu, M.M., Ng, J., Zhang, J.J., 2024. Credit information sharing and investment efficiency: Cross country evidence. *Contemporary Accounting Research* 41(1), 2099-2133.
Iliev, P., Vitanova, S., 2019. The effect of the say-on-pay vote in the United States. *Management Science* 65(10), 4505–4521.
Jayaraman, S., Wu, J. S., 2019. Is silence golden? Real effects of mandatory disclosure. *Review of Financial Studies* 32(6), 2225–2259.
Jensen, M. C., Meckling, W. H., 1976. Theory of the firm: Managerial behavior, agency costs and ownership structure. *Journal of Financial Economics* 3(4), 305-360.
Jiang, F., Kim, K. A., 2020. Corporate governance in China: A survey. *Review of Finance* 24(4), 733-772.
Jiang, G., Lee, C.M.C., Yue, H., 2010. Tunneling through intercorporate loans: The China experience. *Journal of Financial Economics* 98, 1-20.
Jiang, Z., Lie, E., 2016. Cash holding adjustments and managerial entrenchment. *Journal of Corporate Finance* 36, 190-205.
John, K., Litov, L., Yeung, B., 2008. Corporate governance and risk-taking. *Journal of Finance* 63(4), 1679-1728.
Kahneman, D., Tversky, A., 1979. Prospect theory: An analysis of decision under risk. *Econometrica* 47(2), 263-291.
Kleymenova, A., Tuna, I., 2021. Regulation of Compensation and Systemic Risk: Evidence from the UK. *Journal of Accounting Research* 59(3), 1123-1175.
Kravet, T., Muslu, V., 2013. Textual risk disclosures and investors' risk perceptions. *Review of*

Accounting Studies 18, 1088-1122.
Lai, S., Chen, L., Wang, S.Q., Anderson, H.D., 2023. Bank competition and corporate employment: Evidence from the geographic distribution of bank branches in China, *Journal of Banking & Finance* 154, 106964.
Lan, T., Pan, L., 2026. Restricting executive pay, growth incentives, and corporate M&As. *Journal of Corporate Finance* 99, 102977.
Lazear, E.P., Rosen, S., 1981. Rank-order tournaments as optimum labor contracts. *Journal of Political Economy* 89(5), 841-864.
Levinsohn, J., Petrin, A., 2003. Estimating production functions using inputs to control for unobservables. *The Review of Economic Studies* 70(2), 317–341.
Lin, Y., Peters, F., Seo, H., 2022. Enforceability of noncompetition agreements and forced turnovers of chief executive officers. *Journal of Law and Economics* 65(1),177–209.
Murphy, K. J., 2013. Executive compensation: Where we are, and how we got there. *Handbook of the Economics of Finance* 2, 211-356.
Murphy, K. J., Jensen, M. C., 2018. The politics of pay: The unintended consequences of regulating executive compensation. *Journal of Law, Finance, and Accounting* 3, 189–242.
Nanda, V., Silveri, S., Wang, K., Zhao, L., 2024. Executive compensation limits and executive turnover. *Management Science* 70(4), 2382-2405.
Pan, Y., Pikulina, E. S., Siegel, S., Wang, T. Y., 2022. Do equity markets care about income inequality? Evidence from pay ratio disclosure. *Journal of Finance* 77(2), 1371–1411.
Rau, P.R., Wu, Y., Ieong, R.L.S., 2025. Toxic origins, toxic decisions: Biases in CEO selection, Working Paper.
Richardson, S., 2006. Over-investment of free cash flow. *Review of Accounting Studies* 11(2-3), 159–189.
Rhoades, S.A., Rutz, R.D., 1982. Market power and firm risk: A test of the 'quiet life' hypothesis. *Journal of Monetary Economics* 9, 73-85.
Rose, N. L., Wolfram, C., 2000. Has the "million-dollar cap" affected CEO pay? *American Economic Review* 90(2), 197-202.
Rose, N. L., Wolfram, C., 2002. Regulating executive pay: Using the tax code to influence chief executive officer compensation. *Journal of Labor Economics* 20(2), 138-175.
Ru, H., Yang, E., Zou, K., 2025. Early-life experience and CEOs' reactions to COVID-19. *Journal of Accounting and Economics* 79, 101734.
Schmidt, K. M., 1997. Managerial incentives and product market competition. *Review of Economic Studies* 64(2), 191–213.
Smith, C. W., Stulz, R. M., 1985. The determinants of firms' hedging policies. *Journal of Financial and Quantitative Analysis* 20(4), 391-405.
Tian, X., Tu, G., Wang, Y., 2024. The Real Effects of Shadow Banking: Evidence from China. *Management Science* 70(12), 8556-8582.
Wu, Y., Lee, C., Lee, C., Peng, D., 2022. Geographic proximity and corporate investment efficiency: Evidence from high-speed rail construction in China. *Journal of Banking & Finance* 140, 106510.
Xu, N., Chen, Q., Xu, Y., Chan, K. C., 2016. Political uncertainty and cash holdings: Evidence from China. *Journal of Corporate Finance* 40, 276-295.
Yang, J., Fan, D., Li, C., 2025. Employee overtime and innovation dilemma. *Journal of Business Ethics* 200, 689-713.
Zhang, B., Zhou, K.Z., Xie, E., 2026. Effort redirection across multiple career tracks: salary-cut policy and state-owned enterprises in China. *Journal of Management* 52(5), 2000-2032.

Appendix. Variable definitions

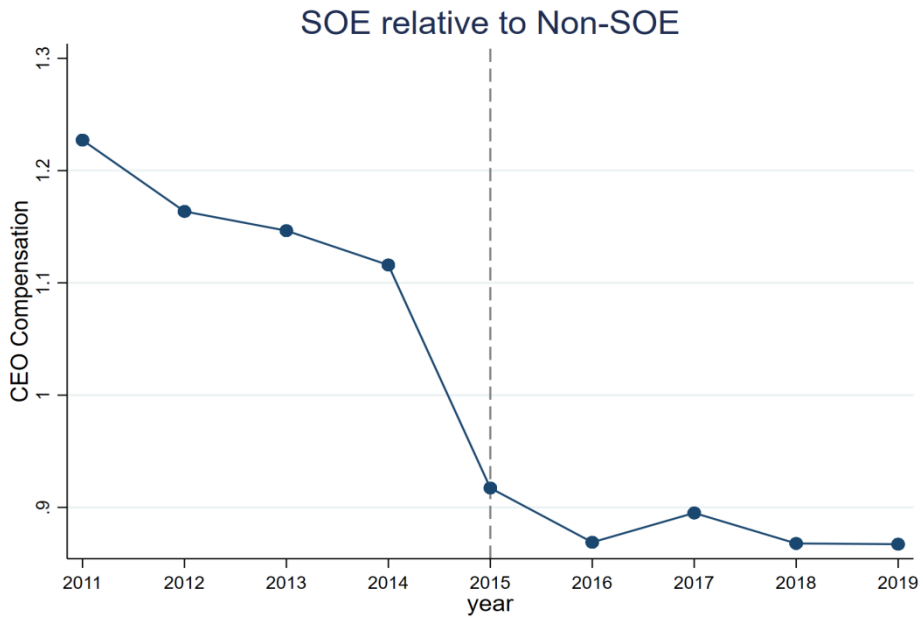
Variable	Description
A. Treatment, ownership, and compensation variables	
SOE	Indicator for government-controlled firms.
Central SOE	Indicator for central government-controlled firms.
Local SOE	Indicator for local government-controlled firms.
Treat	Indicator for firms classified as SOEs in 2014
Post	Indicator for 2015 and subsequent years.
Ln(Compensation)	Natural logarithm of CEO compensation.
PayCut	The negative change in *Ln(Compensation)*. Larger values indicate larger compensation cuts.
PayGap	A firm's pre-reform executive–worker pay multiple, measured before the 2015 reform.
B. Managerial effort and engagement	
Ln(1 + Overtime Keywords)	Natural logarithm of one plus the number of overtime-related keywords mentioned in media reports.
Overtime Ratio	The percentage of overtime days in a given year.
Investor Interactions (0/1)	Indicator for CEOs who participate in at least one CEO–investor interaction.
C. Risk-related behavior and disclosure	
Unrelated Acquisition (0/1)	Indicator for acquisitions outside the firm's primary industry.
Risk Tone	Natural logarithm of one plus the number of risk-related keywords extracted from MD&A.
D. Investment, capital allocation, and investment efficiency	
Investment	Cash paid for fixed, intangible, and other long-term assets, net of disposal proceeds, scaled by total assets.
Repurchase (0/1)	Indicator for firms that repurchase their own shares in a given year.
R&D (0/1)	Indicator for firms reporting R&D expenditure in a given year.
M&A (0/1)	Indicator for firms engaging in M&A activity during the year.
Dividend (0/1)	Indicator for firms distributing cash dividends in a given year.
Investment inefficiency	Absolute value of the residual, ε, from the Richardson (2006) model.
Over-/Underinvestment	Positive and negative residuals from the Richardson (2006) model indicate overinvestment and underinvestment, respectively.
E. Cash policy and firm performance	
Cash	The ratio of cash and cash equivalents to total assets.
Cash*	Estimated target cash holdings, obtained from the optimal cash-holdings model (Jiang and Lie, 2016)
ΔCash	The one-year change in cash and cash equivalents scaled by total assets.
Tobin's Q	Market value of equity plus book value of debt, scaled by book value of total assets.
TFP	Total factor productivity using the Levinsohn and Petrin (2003) method.
F. Executive selection and incentive structure	
CEO turnover, Nanda et al. (2024)	Indicator for CEOs who leave due to resignation, term expiration, or personal reasons.
CEO turnover, Lin et al. (2022)	Indicator for CEO replacement following good prior-year performance, defined as ROA above the industry-adjusted median.
Performance	Firm performance measure used in the pay-performance-sensitivity tests, measured as ROA or ROS depending on the specification.
ROA	Net income scaled by total assets.
ROS	Net income scaled by sales.
G. Rent-seeking and politically salient outcomes	

Perk	Discretionary expenses scaled by sales, as reported in the financial statement footnotes under cash payments for operating expenses.
Tunneling	Natural logarithm of net other receivables.
ESG	Bloomberg ESG scores.
CSR	Corporate social responsibility scores sourced from Hexun.com.
Employment	Natural logarithm of a firm's total number of employees.
Labor productivity	Total sales (in millions) scaled by the number of employees.
Wage-bill intensity	The ratio of total salary expenses to total assets.
H. Firm, CEO, and local controls	
Size	Natural logarithm of total assets.
Leverage	Total liabilities scaled by total assets.
Largest shareholder stake	The percentage of shares held by the largest shareholder.
Cash flow	Operating cash flow scaled by total assets.
Board size	Natural logarithm of the number of board directors.
Independent director ratio	The number of independent directors scaled by board size
Firm age	Natural logarithm of the number of years since the firm's establishment.
CEO age	Natural logarithm of the CEO's age.
CEO female (0/1)	Indicator for firms with a female CEO.
CEO dual (0/1)	Indicator for CEO–chair duality.
Ln(GDP)	Natural logarithm of city-level local GDP.
Ln(Population)	Natural logarithm of city-level local population.
Ln(City employment)	Natural logarithm of city-level working population.
Fiscal pressure	The difference between local fiscal expenditure and local fiscal revenue, scaled by local GDP.

Figure 1. Changes in relative compensation

This figure illustrates relative CEO compensation trends from 2011 to 2019, covering the four years before and after the introduction of the executive pay-cap reform in early 2015. Panel A depicts the evolution of the average compensation gap between all SOEs and non-SOEs over time, while Panel B compares CEO compensation across central SOEs, local SOEs, and non-SOEs.

Panel A. Relative CEO Compensation in SOEs Versus Non-SOEs



Panel B. Relative CEO Compensation in Central and Local SOEs Versus Non-SOEs

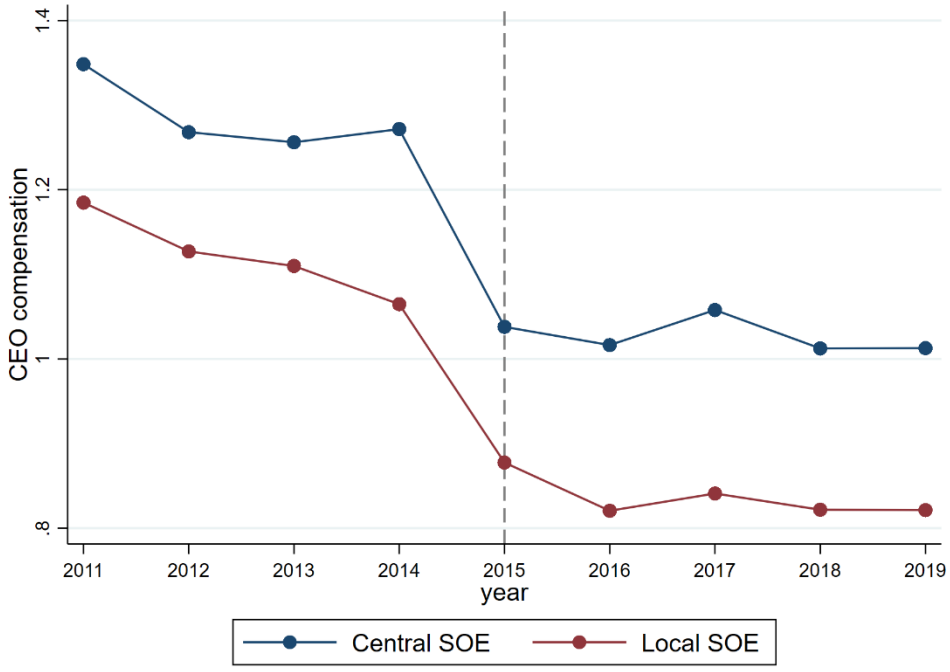


Figure 2. Compensation cuts and pre-policy pay gaps among SOEs

This figure presents a binned-scatter plot for SOEs. The x-axis shows firms' pre-2014 pay multiple (the executive–worker pay gap before the reform), and the y-axis shows the post-reform change in log compensation, Δ*Ln(Compensation)*. More negative values of Δ*Ln(Compensation)* indicate larger compensation cuts. The downward-sloping fitted line indicates that, among SOEs, firms with larger pre-policy pay gaps experienced larger compensation reductions after the 2015 pay-cap policy.

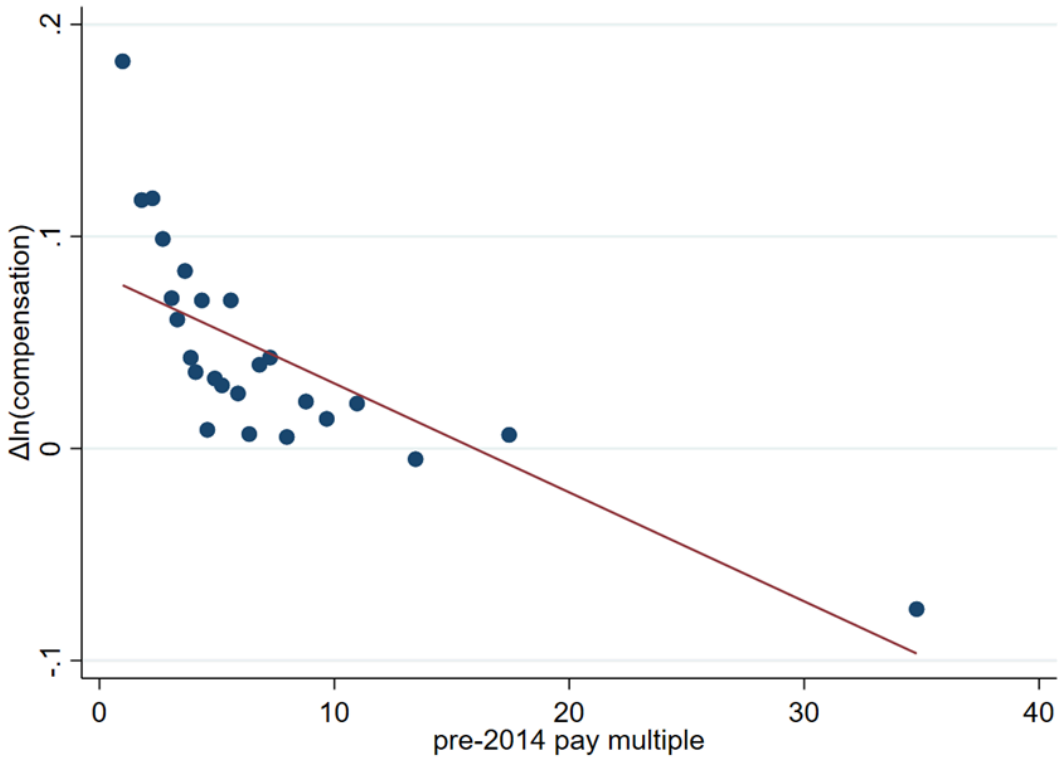


Figure 3. Dynamic effects of executive pay-cap regulation on firm behavior

This figure illustrates the time-varying effects of the executive pay-cap regulation on managerial disengagement and conservative behavior, as reflected in measures of work effort, risk-taking, and investment activity. Definitions of all dependent variables are provided in the Appendix. The year 2014 is used as the reference point, with each coefficient representing the change in the corresponding outcome relative to that baseline year. The lack of statistically significant coefficients prior to 2015 supports the parallel trends assumption between the treatment and control groups. In contrast, the negative and statistically significant coefficients observed after 2015 indicate a notable decline in these behavioral outcomes following the policy's implementation. Vertical lines represent 95% confidence intervals.

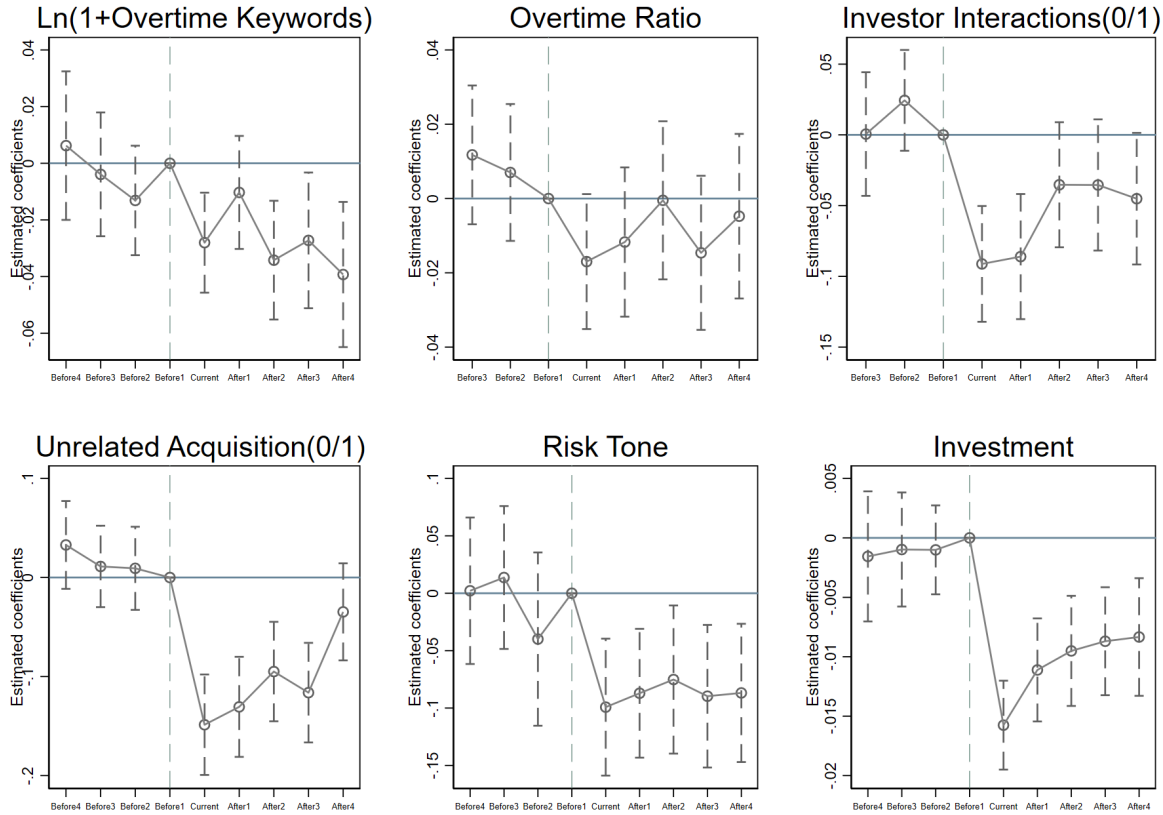


Figure 4. Placebo tests for six direct behavioral outcomes

This figure plots the kernel density of 1,000 placebo estimates for the coefficient on *Treat_F×Post*, generated by randomly assigning the treatment status across firms. Placebo tests are conducted on measures of work effort, risk-taking, and investment activity. In each specification, the distribution of false estimates centers around zero, while the true estimate lies outside the placebo distribution, suggesting that the observed effects are unlikely to be driven by chance.

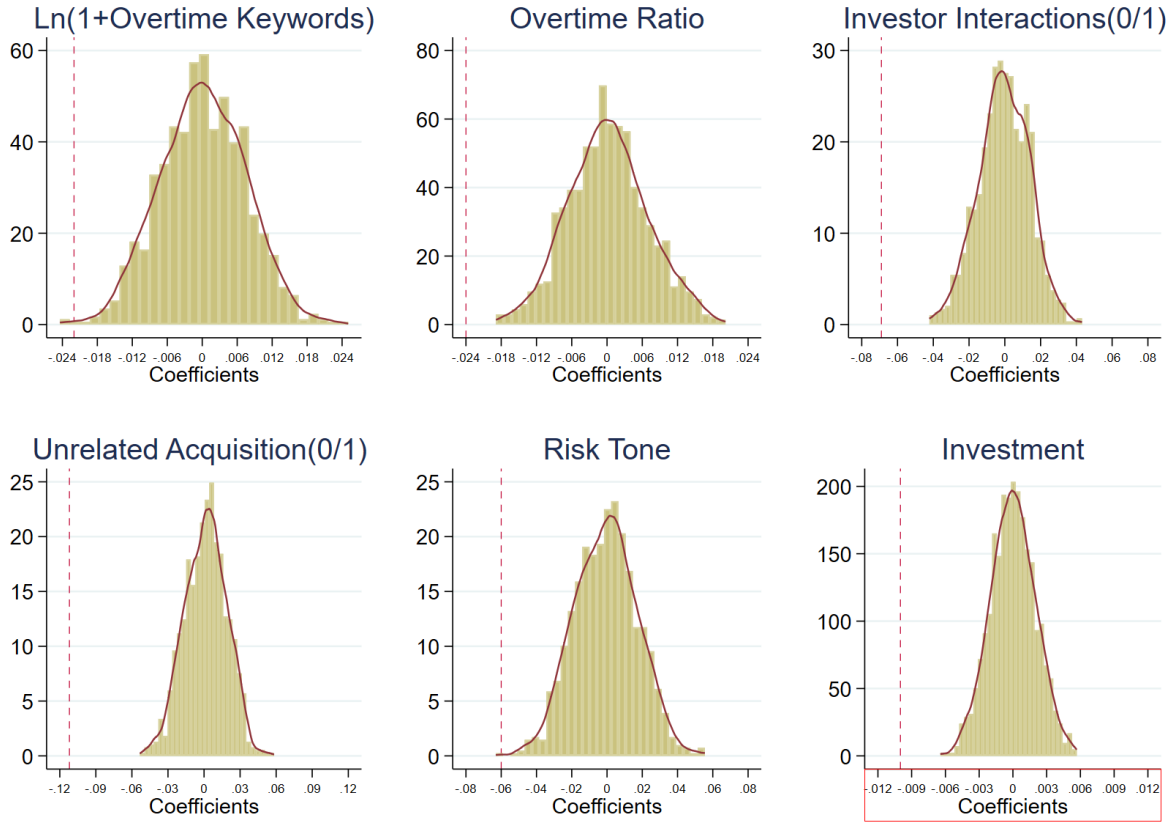


Figure 5. Specification curve analysis

Note: This figure displays the results of a specification curve analysis, which showcases the impact of the executive pay-cap regulation on measures of work effort, risk-taking, and investment activity across 144 model specifications. The dependent variables include *Ln*(1+*Overtime Keywords*), *Overtime Ratio*, *Investor Interactions (0/1), Unrelated Acquisition*, *Risk Tone*, and *Investment*. Horizontal lines indicate the zero threshold, along with the 90%, 95%, and 99% confidence intervals. Notably, the coefficient estimates for the focal variable are statistically significant at the 1% level in all specifications, underscoring the robustness of the results.

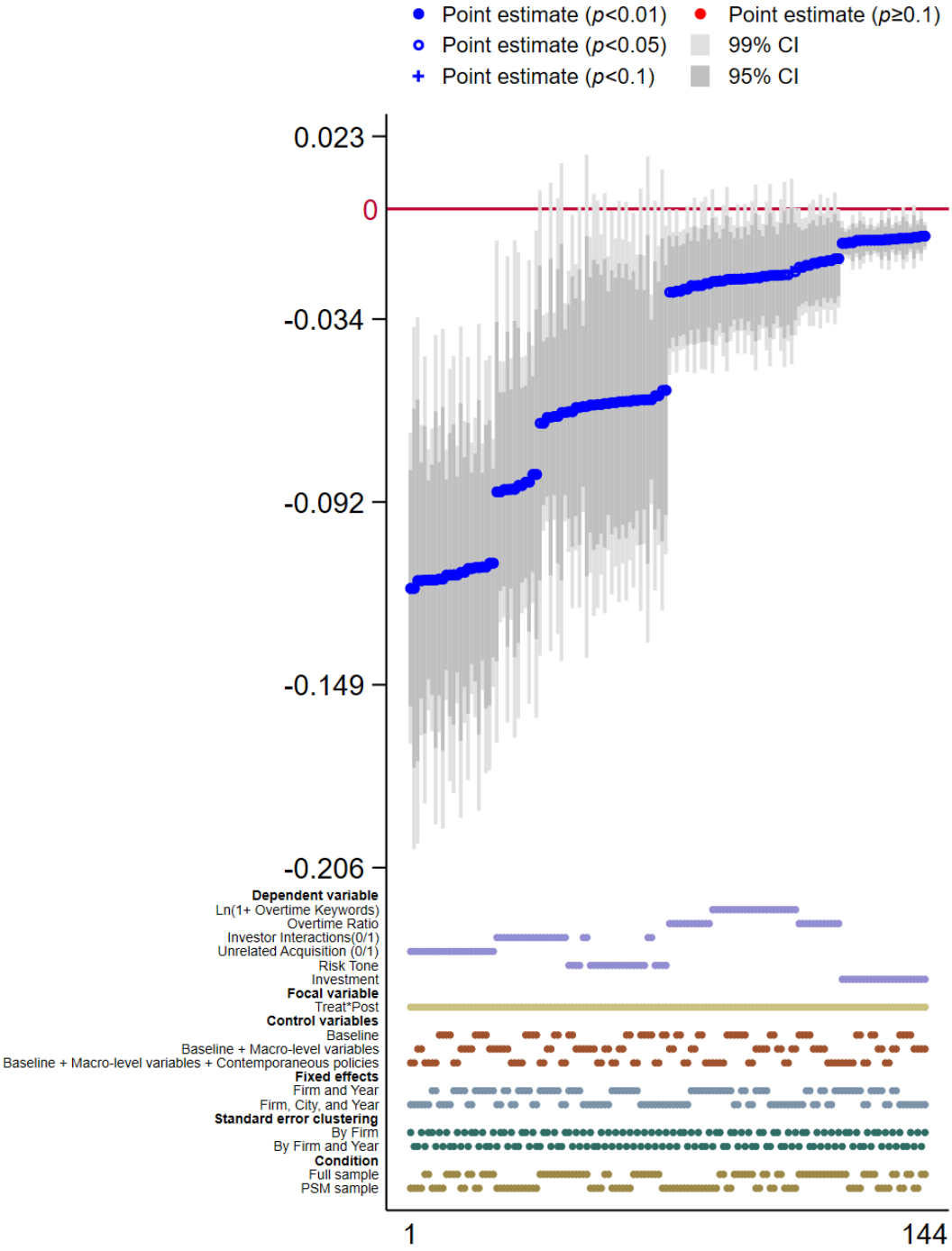


Table 1. Descriptive statistics

This table reports descriptive statistics for the main variables used in the analysis. Panel A reports treatment and compensation variables. Panels B–E report the main outcome variables. Panel F reports mechanism variables. Panel G reports firm, CEO, and local controls. Variable definitions are provided in the Appendix. Continuous variables are winsorized at the 1st and 99th percentiles.

Variables	N	Mean	SD	Median
A. Treatment and compensation variables				
Treat	18,825	0.374	0.484	0
Post	18,825	0.563	0.496	1
Ln(Compensation)	17,996	13.222	0.771	13.221
B. Managerial effort and engagement				
Ln(1 + Overtime Keywords)	18,825	0.026	0.168	0
Overtime Ratio	14,406	0.331	0.156	0.328
Investor Interactions (0/1)	16,933	0.197	0.398	0
C. Risk-related behavior and disclosure				
Unrelated Acquisition (0/1)	18,825	0.147	0.354	0
Risk Tone	18,825	5.430	0.499	5.489
D. Investment, capital allocation, and investment efficiency				
Investment	18,814	0.046	0.047	0.034
M&A (0/1)	18,825	0.091	0.288	0
Repurchase (0/1)	18,825	0.039	0.193	0
R&D (0/1)	18,825	0.190	0.392	0
Dividend (0/1)	18,825	0.712	0.453	1
Investment inefficiency	15,395	0.022	0.023	0.015
E. Cash policy and firm performance				
Cash	18,825	0.162	0.131	0.123
Tobin's Q	18,325	2.013	1.33	1.565
TFP	18,814	16.87	1.102	16.777
F. Mechanism variables				
CEO turnover, Nanda et al. (2024)	18,457	0.053	0.225	0
CEO turnover, Lin et al. (2022)	18,457	0.036	0.185	0
Perk	17,779	0.008	0.011	0.005
Tunneling	18,815	18.148	2.07	18.211
ESG	7,639	20.419	6.829	19.835
CSR	18,801	13.556	6.500	14.200
Employment	18,817	7.705	1.326	7.644
Labor productivity	18,817	1.610	2.281	0.887
Wage-bill intensity	18,401	0.064	0.046	0.053
G. Firm, CEO, and local controls				
Size	18,825	22.236	1.305	22.063
Leverage	18,825	0.433	0.211	0.427
Largest shareholder stake	18,825	0.351	0.149	0.33
Cash flow	18,825	0.041	0.071	0.042
ROA	18,825	0.033	0.063	0.033
ROS	18,824	0.063	0.189	0.064
Board size	18,825	2.288	0.249	2.303
Independent director ratio	18,825	0.38	0.072	0.364
Firm age	18,825	2.813	0.353	2.833

CEO age	18,457	3.888	0.138	3.912
CEO female (0/1)	18,459	0.065	0.247	0
CEO dual (0/1)	18,342	0.258	0.438	0
Ln(GDP)	18,450	18.114	1.037	17.386
Ln(Population)	18,590	6.462	0.664	6.068
Ln(City employment)	18,593	5.16	1.07	4.44
Fiscal pressure	18,450	0.042	0.075	0.016

Table 2. Executive pay caps and managerial effort

This table presents the estimated effects of the executive pay-cap regulation on managerial effort. Columns (1) and (2) measure work effort using *Ln*(1+*Overtime Keywords*), the natural logarithm of one plus the number of overtime-related keywords mentioned in media reports, and the *Overtime Ratio*, defined as the percentage of overtime days in a given year. Column (3) measures managerial engagement using *Investor Interactions (0/1)*, a binary indicator equal to one if the CEO participates in at least one investor interaction activity, and zero otherwise. All variable definitions are provided in the Appendix. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5% and 10% level, respectively.

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Investor Interactions (0/1)
	(1)	(2)	(3)
Treat × Post	-0.024***	-0.016**	-0.069***
	(-3.528)	(-2.551)	(-4.715)
Size	0.012***	0.006	0.068***
	(2.738)	(1.306)	(7.227)
Leverage	-0.014	-0.034**	-0.082**
	(-0.928)	(-2.069)	(-2.377)
Largest shareholder stake	-0.049	-0.029	-0.019
	(-1.603)	(-1.090)	(-0.320)
Cash flow	-0.001	0.003	-0.070
	(-0.039)	(0.129)	(-1.499)
ROA	-0.029	-0.048	0.365***
	(-0.734)	(-1.584)	(5.507)
Board size	-0.005	-0.013*	-0.003
	(-0.583)	(-1.900)	(-0.175)
Independent director ratio	-0.013	0.049**	-0.016
	(-0.568)	(2.273)	(-0.333)
Firm age	0.024	-0.016	0.042
	(0.668)	(-0.480)	(0.582)
Constant	-0.262*	0.287**	-1.383***
	(-1.945)	(2.128)	(-4.743)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes
Adj R^2	0.134	0.275	0.364
N	18,825	14,406	16,933

Table 3. Executive pay caps, risk-taking, and risk-related disclosure

This table examines the impact of the executive pay-cap regulation on corporate risk-taking and on risk-related language in MD&A disclosures. Column (1) employs *Unrelated Acquisition* (0/1), a binary indicator equal to one if the firm undertakes an acquisition outside its primary industry, and zero otherwise. Column (2) uses *Risk Tone*, the natural logarithm of one plus the number of risk-related keywords extracted from MD&A. Variable definitions are provided in the Appendix. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5% and 10% level, respectively.

Dependent variable =	Unrelated Acquisition (0/1)	Risk Tone
	(1)	(2)
Treat × Post	-0.120***	-0.082***
	(-6.937)	(-4.524)
Size	0.020**	0.049***
	(2.163)	(4.434)
Leverage	0.021	0.058
	(0.597)	(1.483)
Largest shareholder stake	-0.021	-0.114*
	(-0.373)	(-1.694)
Cash flow	0.039	-0.072
	(0.830)	(-1.285)
ROA	0.123**	-0.051
	(2.019)	(-0.621)
Board size	-0.004	0.024
	(-0.232)	(1.324)
Independent director ratio	0.013	0.058
	(0.288)	(1.028)
Firm age	0.095*	0.003
	(1.672)	(0.030)
Constant	-0.538**	4.301***
	(-2.130)	(13.030)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes
Adj R^2	0.119	0.489
N	18,825	18,825

Table 4. Executive pay caps and investment allocation

This table examines the impact of the executive pay-cap regulation on corporate investment and capital allocation. Column (1) captures Investment, defined as the cash paid for fixed assets, intangible assets, and other long-term assets minus the cash received from their disposal, scaled by total assets. Columns (2) through (5) examine allocation decisions: R&D (0/1), a dummy variable equal to one if a firm reported R&D expenditure; M&A (0/1), a dummy variable equal to one if a firm engaged in any M&A activity; Repurchase (0/1), a dummy variable equal to one if a firm repurchased its own shares; and Dividend (0/1), a dummy variable equal to one if a firm distributed cash dividends. Definitions of all variables are provided in the Appendix. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5% and 10% level, respectively.

Dependent variable =	Investment	R&D (0/1)	M&A (0/1)	Repurchase (0/1)	Dividend (0/1)
	(1)	(2)	(3)	(4)	(5)
Treat × Post	-0.010***	0.001	-0.024**	-0.053***	0.066***
	(-5.551)	(0.088)	(-2.573)	(-8.190)	(4.567)
Size	0.004***	0.019***	0.039***	0.030***	0.103***
	(2.792)	(4.258)	(5.358)	(6.141)	(10.428)
Leverage	-0.003	-0.012	0.131***	-0.067***	-0.240***
	(-0.688)	(-0.756)	(4.764)	(-3.573)	(-6.900)
Largest shareholder stake	0.030***	-0.032	0.084**	-0.081***	0.194***
	(3.796)	(-1.110)	(1.968)	(-2.605)	(3.226)
Cash flow	0.009*	0.018	0.007	-0.010	0.080
	(1.682)	(0.760)	(0.186)	(-0.406)	(1.560)
ROA	0.038***	-0.009	0.251***	0.057	1.972***
	(5.352)	(-0.279)	(5.180)	(1.468)	(28.186)
Board size	-0.005***	0.005	-0.009	-0.021**	-0.035**
	(-3.255)	(0.702)	(-0.756)	(-2.558)	(-2.171)
Independent director ratio	0.005	-0.010	-0.052	0.015	0.045
	(1.103)	(-0.468)	(-1.324)	(0.570)	(0.963)
Firm age	-0.033***	0.116***	-0.051	0.120***	-0.230***
	(-3.944)	(3.849)	(-1.114)	(2.844)	(-3.793)
Constant	0.059	-0.551***	-0.684***	-0.848***	-0.924***
	(1.550)	(-4.160)	(-3.312)	(-5.377)	(-3.368)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.461	0.898	0.105	0.217	0.484
N	18,814	18,825	18,825	18,825	18,825

Table 5. Executive pay caps and corporate investment efficiency

This table reports the effect of the executive pay-cap regulation on corporate investment efficiency. Column (1) presents results from an investment-Q sensitivity analysis. Column (2) follows the approach of Richardson (2006) to estimate expected investment based on firm fundamentals, with investment inefficiency defined as the deviation from this predicted benchmark. Columns (3) and (4) split the sample based on the sign of the residuals from Column (2), with Column (3) focusing on overinvesting firms and Column (4) on underinvesting firms. Variable definitions are provided in the Appendix. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5% and 10% level, respectively.

Dependent variable =	Investment	Investment inefficiency		
	Full sample	Full sample	Overinvesting firms	Underinvesting firms
	(1)	(2)	(3)	(4)
Treat × Post	0.003	0.004***	-0.001	0.006***
	(0.813)	(4.234)	(-0.444)	(6.451)
Treat × Post × Tobin's Q	-0.007***			
	(-4.211)			
Tobin's Q	-0.002**			
	(-2.466)			
Treat × Tobin's Q	0.004***			
	(2.676)			
Post × Tobin's Q	0.004***			
	(4.115)			
Size	0.004***	-0.000	0.001	-0.001
	(2.891)	(-0.175)	(0.832)	(-1.582)
Leverage	-0.002	0.000	-0.003	0.001
	(-0.435)	(0.092)	(-0.533)	(0.571)
Largest shareholder stake	0.028***	0.009**	0.013	0.006
	(3.492)	(2.185)	(1.262)	(1.489)
Cash flow	0.010*	0.003	0.019**	-0.010***
	(1.855)	(0.790)	(2.229)	(-3.194)
ROA	0.039***	0.013***	0.032***	0.002
	(5.319)	(3.125)	(2.937)	(0.578)
Board size	-0.006***	0.000	-0.002	0.001
	(-3.469)	(0.200)	(-0.652)	(0.907)
Independent director ratio	0.005	-0.006*	-0.013*	-0.004
	(0.983)	(-1.757)	(-1.776)	(-1.255)
Firm age	-0.032***	-0.005	-0.008	-0.008*
	(-3.818)	(-1.051)	(-0.764)	(-1.736)
Constant	0.046	0.036*	0.025	0.062***
	(1.169)	(1.701)	(0.558)	(2.988)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes
Adj R^2	0.465	0.214	0.202	0.212
N	18,302	15,373	5,708	9,177

Table 6. Executive pay caps and corporate cash management

This table presents the impact of the executive pay-cap regulation on corporate cash management. Panel A examines the effect of executive pay caps on corporate cash holdings, measured as the ratio of cash and cash equivalents to total assets (*Cash*). Columns (1) to (3) report the regressions with industry fixed effects, industry and year fixed effects, and firm fixed effects together with province-by-year and industry-by-year fixed effects, respectively. Panel B reports the effect of executive pay caps on the adjustment speed of corporate cash holdings. The dependent variable is *ΔCash*, the one-year change in cash holdings. The key variable of interest is $Treat \times Post \times (Cash_t^* - Cash_{t-1})$, where $Cash_t^* - Cash_{t-1}$ measures the gap between the target cash ratio and the beginning-of-year cash ratio. A positive value indicates a cash shortfall (actual cash below target), whereas a negative value indicates excess cash (actual cash above target). Column (1) reports results for the full sample. Columns (2) and (3) report subsample results for firms with excess cash and cash shortfall, respectively, where excess cash is defined as $Cash_{t-1} > Cash_t^*$ and cash shortfall is defined as $Cash_{t-1} < Cash_t^*$. Panel C reports the effect of executive pay caps on the marginal value of cash using Faulkender and Wang's (2006) model. The dependent variable is $r - R$, the difference between a firm's yearly stock return (r) and the benchmark return (R). Columns (1)–(3) use different fixed-effects specifications. Panel D examines the financing and balance-sheet channels associated with cash accumulation following the executive pay-cap regulation. The dependent variables are *Sales Growth*, *Net Working Capital*, and *New Financing*. *Sales Growth* is the growth rate of operating revenue. *Net Working Capital* is measured as inventory plus accounts receivable minus accounts payable, scaled by total assets. *New Financing* captures external financing inflows and is measured as the sum of long-term debt issuance and new stock issuance, scaled by equity market value. The regressions in columns (1)–(3) include firm fixed effects as well as province-by-year and industry-by-year fixed effects. Definitions for all other variables are provided in the Appendix. t-statistics are based on heteroskedasticity-robust standard errors clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Impact of executive pay caps on corporate cash holdings

Dependent variable =	Cash	Cash	Cash
	(1)	(2)	(3)
Treat × Post	0.053***	0.055***	0.046***
	(13.105)	(13.630)	(9.729)
Treat	-0.013***	-0.017***	
	(-2.792)	(-3.639)	
Post	-0.051***		
	(-15.212)		
Size	-0.008***	-0.007***	0.004
	(-4.712)	(-4.054)	(1.225)
Leverage	-0.220***	-0.223***	-0.252***
	(-20.671)	(-20.924)	(-20.628)
Largest shareholder stake	0.037***	0.037***	0.029
	(3.206)	(3.229)	(1.478)
Cash flow	0.160***	0.175***	0.196***
	(8.949)	(9.749)	(13.946)
ROA	0.181***	0.149***	0.026
	(7.865)	(6.365)	(1.366)
Board size	0.001	0.006	0.008*
	(0.219)	(1.092)	(1.879)
Independent director ratio	-0.037**	-0.015	-0.030**
	(-2.330)	(-0.919)	(-2.502)

Firm age	-0.023***	-0.012**	-0.135***
	(-4.202)	(-2.060)	(-6.180)
Dividend(0/1)	0.010***	0.011***	0.010***
	(3.583)	(3.910)	(5.006)
Repurchase(0/1)	-0.008*	-0.004	-0.002
	(-1.822)	(-0.808)	(-0.456)
Constant	0.510***	0.409***	0.519***
	(13.262)	(10.122)	(5.464)
Year FE	No	Yes	-
Industry FE	Yes	Yes	-
Firm, Province × Year FE, and Industry × Year FE	No	No	Yes
Adj R^2	0.300	0.315	0.634
N	18,825	18,825	18,825

Panel B. Impact of executive pay caps on cash adjustment speed

	Full sample	Excess cash	Cash shortfall
Dependent variable =	$\Delta Cash_t$	$\Delta Cash_t$	$\Delta Cash_t$
	(1)	(2)	(3)
Treat × Post × ($Cash_t^*$–$Cash_{t-1}$)	-0.163***	-0.106***	-0.006
	(-6.281)	(-4.002)	(-0.269)
$Cash_t^*$–$Cash_{t-1}$	0.500***	0.938***	0.894***
	(31.485)	(51.291)	(64.825)
Treat × Post	0.010***	0.006**	0.001
	(3.501)	(2.162)	(0.364)
Treat×($Cash_t^*$–$Cash_{t-1}$)	0.072***	0.027	0.011
	(2.691)	(0.955)	(0.531)
Post×($Cash_t^*$–$Cash_{t-1}$)	0.251***	0.098***	0.056***
	(13.288)	(5.287)	(3.856)
Constant	-0.011***	0.046***	-0.051***
	(-16.820)	(51.529)	(-119.531)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes
Adj R^2	0.401	0.746	0.819
N	16,387	7,707	8,680

Panel C. Marginal value of cash

Dependent variable =	r-R	r-R	r-R
	(1)	(2)	(3)
Treat × Post × ΔCash	-0.583***	-0.653***	-0.424*
	(-2.872)	(-3.201)	(-1.897)
Post × Cash	0.019	0.041	-0.117
	(0.175)	(0.382)	(-1.009)
Treat × ΔCash	0.078	0.101	-0.118
	(0.499)	(0.647)	(-0.689)
Treat × Post	-0.069***	-0.070***	-0.065***
	(-5.925)	(-5.983)	(-4.838)
Treat	0.015	0.014	
	(1.567)	(1.519)	
Post	0.009		
	(1.135)		
ΔCash	0.564***	0.526***	0.725***
	(4.056)	(3.816)	(4.784)
ΔEarnings	0.557***	0.577***	0.512***
	(12.964)	(13.445)	(11.932)
ΔNet Assets	0.286***	0.276***	0.281***
	(19.779)	(19.333)	(17.915)
ΔInterest Expenses	-0.474*	-0.335	-0.181
	(-1.750)	(-1.259)	(-0.656)
$Cash_{t-1}$	0.057**	0.066**	0.117***
	(2.138)	(2.459)	(2.591)
Net Financing	-0.131***	-0.126***	-0.112***
	(-5.808)	(-5.632)	(-4.551)
Leverage	-0.055***	-0.052***	-0.103***
	(-16.982)	(-16.131)	(-16.775)
Leverage×ΔCash	0.407***	0.384***	0.335***
	(5.827)	(5.543)	(4.605)
$Cash_{t-1}$×ΔCash	-0.959***	-0.848***	-0.853**
	(-2.964)	(-2.639)	(-2.451)
ΔDividends	1.432***	1.475***	1.471***
	(7.040)	(7.313)	(7.003)
Constant	0.046***	0.050***	0.086***
	(5.540)	(9.177)	(10.029)
Year FE	No	Yes	-
Industry FE	Yes	Yes	-
Firm, Province × Year FE, and Industry × Year FE	No	No	Yes
Adj R^2	0.123	0.132	0.148
N	16,323	16,323	16,323

Panel D. Financing and balance-sheet channels of cash accumulation

Dependent variable =	Sales growth	Net working capital	New financing
	(1)	(2)	(3)
Treat × Post	0.008	0.038**	0.024***
	(0.560)	(1.999)	(5.133)
Size	0.150***	0.066***	0.063***
	(10.900)	(3.896)	(16.316)
Leverage	0.357***	0.043	0.145***
	(7.033)	(0.702)	(11.271)
Largest shareholder stake	0.226***	0.112	0.006
	(3.031)	(1.231)	(0.286)
Cash flow	-0.030	-0.823***	-0.212***
	(-0.393)	(-10.987)	(-13.632)
ROA	1.984***	-0.650***	-0.095***
	(19.886)	(-6.585)	(-4.766)
Board size	0.126***	-0.026	-0.002
	(5.333)	(-1.371)	(-0.354)
Independent director ratio	-0.088	0.060	-0.036**
	(-1.460)	(1.194)	(-2.574)
Firm age	0.074	-0.084	-0.054**
	(1.161)	(-1.329)	(-2.351)
Dividend(0/1)	0.009	-0.021**	0.007***
	(0.834)	(-2.161)	(2.697)
Repurchase(0/1)	-0.058***	0.003	-0.000
	(-3.562)	(0.206)	(-0.079)
Constant	-3.931***	-0.720*	-1.135***
	(-11.477)	(-1.864)	(-10.986)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes
Adj R^2	0.159	0.804	0.622
N	18,426	18,638	18,325

Table 7. Executive pay caps and firm performance

This table reports the regression results on the impact of the executive pay caps on firm valuation and productivity. The dependent variables are *Tobin's Q* in Column (1) and total factor productivity (*TFP*) in Column (2), respectively. The controls are the same as in the baseline regression, except that Size is excluded because firm scale is mechanically embedded in Tobin's Q and in the TFP estimation framework. Definitions for all other variables are provided in the Appendix. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5% and 10% level, respectively.

Dependent variable =	Tobin's Q	TFP
	(1)	(2)
Treat × Post	-0.243***	-0.169***
	(-5.608)	(-7.149)
Leverage	-0.249**	0.622***
	(-2.129)	(8.764)
Largest shareholder stake	-0.393**	0.356***
	(-2.228)	(2.750)
Cash flow	1.064***	0.532***
	(7.590)	(7.015)
Board size	-0.047	0.072***
	(-1.068)	(3.349)
Independent director ratio	0.487***	-0.045
	(4.099)	(-0.733)
Firm age	0.689***	0.203**
	(4.060)	(1.998)
Constant	0.251	15.770***
	(0.514)	(53.829)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes
Adj R^2	0.663	0.894
N	18,325	18,814

Table 8. Heterogeneity in managerial disengagement and conservatism

This table explores how the effects of executive pay regulation on corporate behavior vary across firm-specific characteristics. Panel A investigates the moderating role of CEO promotion incentives, proxied by *CEO age*, under the assumption that older CEOs face fewer career concerns and thus weaker incentives for promotion. "Old CEO" firms are those with a CEO aged above the median in 2014, the year before the policy's implementation. Panel B explores the influence of industry competitiveness, measured using the Herfindahl-Hirschman Index (*HHI*). Firms in more competitive industries (i.e., those with below-median 2014 HHI values) are assumed to place greater reliance on performance-based incentives. Panel C investigates the role of internal governance quality, proxied by the Internal Control Index (INC). "Weak governance" firms are defined as those with INC values below the 2014 median. Across all panels, control variables align with those specified in Eq. (1) and are detailed in the Appendix. The models include firm fixed effects as well as province-by-year and industry-by-year fixed effects, and t-statistics are computed using heteroscedasticity-robust standard errors clustered at the firm level. Statistical significance is denoted by ***, **, and * at the 1%, 5%, and 10% levels, respectively.

Panel A. The role of CEO promotion incentives

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Investor Interactions (0/1)	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post × Old CEO	-0.034***	-0.006	-0.136***	-0.161***	-0.100***	-0.022***
	(-2.868)	(-0.531)	(-5.396)	(-5.525)	(-3.198)	(-6.677)
Post × Old CEO	0.003	-0.011	0.012	-0.028*	-0.004	0.010***
	(0.423)	(-1.523)	(0.703)	(-1.761)	(-0.272)	(4.467)
Treat × Post	-0.010	-0.013	-0.007	-0.045***	-0.036	-0.001
	(-1.430)	(-1.567)	(-0.418)	(-2.773)	(-1.635)	(-0.581)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.130	0.275	0.368	0.130	0.484	0.467
N	18,194	13,958	16,394	18,194	18,194	18,184

Panel B. The role of industry competition

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Investor Interactions (0/1)	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post × Competitive industries	-0.032**	-0.033***	-0.146***	-0.179***	-0.101***	-0.017***
	(-2.443)	(-2.672)	(-5.426)	(-5.634)	(-2.835)	(-4.945)
Post × Competitive industries	0.023	0.030	-0.043	-0.118**	-0.038	-0.006
	(1.340)	(0.992)	(-0.619)	(-2.369)	(-0.554)	(-0.824)
Treat × Post	-0.007	0.003	0.012	-0.022	-0.026	-0.002
	(-0.865)	(0.325)	(0.695)	(-1.276)	(-1.164)	(-0.707)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.129	0.276	0.366	0.125	0.487	0.467
N	18,524	14,203	16,688	18,524	18,524	18,513

Panel C. The role of governance quality

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Investor Interactions (0/1)	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post × Weak governance	-0.020	-0.020*	-0.120***	-0.193***	-0.093***	-0.000
	(-1.645)	(-1.682)	(-4.614)	(-6.516)	(-2.984)	(-0.008)
Post × Weak governance	-0.006	-0.014*	0.012	-0.037**	0.003	-0.002
	(-0.906)	(-1.911)	(0.681)	(-2.457)	(0.177)	(-1.056)
Treat × Post	-0.017*	-0.009	-0.013	-0.041**	-0.041*	-0.011***
	(-1.857)	(-0.973)	(-0.743)	(-2.456)	(-1.672)	(-4.759)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.131	0.274	0.363	0.132	0.482	0.464
N	17,827	13,595	15,992	17,827	17,827	17,816

Table 9. Executive pay caps and managerial disengagement and conservatism: entropy-balancing reweighted sample

This table explores the effects of executive pay regulation on corporate behavior in an entropy-balancing reweighted sample. The models include firm fixed effects as well as province-by-year and industry-by-year fixed effects, and t-statistics are computed using heteroscedasticity-robust standard errors clustered at the firm level. Statistical significance is denoted by ***, **, and * at the 1%, 5%, and 10% levels, respectively.

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Investor Interactions (0/1)	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	-0.028***	-0.028***	-0.063***	-0.113***	-0.109***	-0.007***
	(-3.585)	(-3.531)	(-3.654)	(-5.419)	(-4.652)	(-3.609)
Size	0.007	0.012*	0.054***	0.037***	0.035**	0.004**
	(0.992)	(1.736)	(4.478)	(2.891)	(2.268)	(2.456)
Leverage	-0.037	-0.044*	-0.151***	0.002	0.028	-0.006
	(-1.166)	(-1.697)	(-3.176)	(0.041)	(0.328)	(-0.919)
Largest shareholder stake	-0.069	-0.058	-0.051	-0.000	-0.239**	0.016
	(-1.442)	(-1.549)	(-0.747)	(-0.006)	(-2.124)	(1.641)
Cash flow	0.047	-0.061**	-0.173**	0.045	0.028	0.004
	(0.793)	(-2.084)	(-2.447)	(0.686)	(0.273)	(0.617)
ROA	-0.132*	-0.045	0.458***	0.151	-0.234	0.069***
	(-1.695)	(-0.983)	(4.352)	(1.554)	(-1.573)	(5.556)
Board size	-0.031**	-0.014	0.011	0.003	-0.045	-0.002
	(-2.085)	(-1.490)	(0.617)	(0.121)	(-0.935)	(-1.112)
Independent director ratio	0.170*	0.047*	0.051	-0.012	0.511*	0.007
	(1.760)	(1.699)	(0.827)	(-0.162)	(1.922)	(1.065)
Firm age	-0.106*	-0.079*	0.076	0.085	-0.115	-0.020*
	(-1.759)	(-1.728)	(0.879)	(0.948)	(-0.542)	(-1.716)
Constant	0.246	0.366*	-1.248***	-0.917**	5.017***	0.007
	(1.101)	(1.936)	(-3.207)	(-2.346)	(6.455)	(0.128)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.337	0.372	0.451	0.204	0.556	0.604
N	18,825	14,406	16,933	18,825	18,825	18,814

Table 10. Executive pay caps and CEO turnover

This table reports the effects of the executive pay-cap reform on CEO turnover. The dependent variable in Columns (1) and (2) follows Nanda et al. (2024) and equals one if the CEO voluntarily leaves the firm due to resignation, term expiration, or personal reasons according to the CSMAR classification. The dependent variable in Columns (3) and (4) follows Lin et al. (2022) and equals one if the CEO is replaced despite good prior performance, defined as a prior-year ROA above the industry-adjusted median. Columns (1) and (3) report logit estimates, and Columns (2) and (4) report linear probability OLS estimates. Because the logit model does not converge with the full high-dimensional fixed-effect structure, the logit specifications use firm, year, and industry fixed effects, while the OLS specifications use firm fixed effects together with province-by-year and industry-by-year fixed effects. Variable definitions are provided in the Appendix. t-statistics in parentheses are based on heteroskedasticity-robust standard errors clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable =	CEO turnover (Nanda et al., 2024)		CEO turnover (Lin et al., 2022)	
	Logit	OLS	Logit	OLS
	(1)	(2)	(3)	(4)
Treat × Post	0.598***	0.025***	0.637***	0.015**
	(3.369)	(3.018)	(3.201)	(2.292)
Size	-0.165*	-0.014**	-0.038	-0.001
	(-1.646)	(-2.286)	(-0.287)	(-0.274)
Leverage	0.098	0.015	-1.355***	-0.039**
	(0.247)	(0.644)	(-2.684)	(-2.337)
Largest shareholder stake	0.445	0.043	2.010**	0.099***
	(0.692)	(1.064)	(2.377)	(3.063)
Cash flow	1.242**	0.067**	0.556	0.005
	(2.061)	(2.006)	(0.726)	(0.194)
ROA	-1.020	-0.065	0.523	0.011
	(-1.366)	(-1.330)	(0.472)	(0.306)
Board size	3.926***	0.245***	3.527***	0.138***
	(16.464)	(17.266)	(12.493)	(12.550)
Independent director	-4.322***	-0.248***	-6.125***	-0.220***
ratio	(-6.117)	(-7.412)	(-7.239)	(-7.822)
Firm age	1.845**	0.109***	0.429	0.021
	(2.345)	(3.365)	(0.502)	(0.818)
CEO age	-0.875**	-0.074***	-0.820*	-0.032
	(-2.232)	(-2.827)	(-1.675)	(-1.520)
CEO female(0/1)	0.108	0.013	0.619**	0.025*
	(0.533)	(0.880)	(2.328)	(1.877)
CEO dual(0/1)	0.625***	0.042***	0.909***	0.034***
	(4.460)	(4.799)	(5.318)	(4.804)
Ln(GDP)	-0.149	-0.008	-0.129	-0.006
	(-0.473)	(-0.364)	(-0.334)	(-0.360)
Ln(Population)	-0.006	0.029	0.469	0.025
	(-0.017)	(0.945)	(1.003)	(1.095)

Ln(City employment)	0.296	-0.003	-0.039	-0.012
	(1.248)	(-0.255)	(-0.121)	(-1.144)
Fiscal pressure	1.434*	0.027	-0.699	-0.013
	(1.711)	(0.172)	(-0.907)	(-0.081)
Constant		-0.219		-0.275
		(-0.798)		(-1.330)
Firm, Year, and Industry FE	Yes	-	Yes	-
Firm, Province × Year FE, and Industry × Year FE	-	Yes	-	Yes
Adj R^2/ Pseudo R^2	0.189	0.169	0.144	0.047
N	5,922	17,978	4,291	17,978

Table 11. Mitigating the impact of ex-ante CEO turnover risk

This table re-estimates the main specifications using matched samples constructed on ex-ante CEO turnover risk. The matching procedure balances treated SOEs and control firms on their predicted likelihood of CEO turnover before the reform. Matching details are presented in Appendix IA.28. Panel A estimates turnover likelihood using the methodology from Nanda et al. (2024), while Panel B uses the approach from Lin et al. (2022). Definitions for all other variables are provided in the Appendix. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5% and 10% level, respectively.

Panel A. Matched sample based on CEO turnover risk from Nanda et al. (2024)

Dependent variable =	Ln(1+ Overtime Keywords)	Overtime Ratio	Investor Interactions (0/1)	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	-0.025***	-0.013*	-0.076***	-0.122***	-0.085***	-0.010***
	(-3.135)	(-1.929)	(-4.600)	(-6.234)	(-4.302)	(-5.039)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.100	0.283	0.364	0.121	0.477	0.467
N	14,516	11,130	13,072	14,516	14,516	14,510

Panel B. Matched sample based on CEO turnover risk from Lin et al. (2022)

Dependent variable =	Ln(1+ Overtime Keywords)	Overtime Ratio	Investor Interactions (0/1)	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	-0.023***	-0.015**	-0.071***	-0.112***	-0.085***	-0.011***
	(-2.949)	(-2.081)	(-4.304)	(-5.819)	(-4.104)	(-5.306)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.109	0.276	0.372	0.123	0.463	0.467
N	14,423	11,054	12,991	14,423	14,423	14,415

Table 12. Executive pay caps and CEO pay-performance sensitivity

This table presents the regression results for pay-performance sensitivity. The dependent variable, *Ln*(*Compensation*), is the natural logarithm of CEO compensation. Columns (1)-(2) use *ROA* and *ROS* as the *Performance* measure, respectively. Definitions for all other variables are provided in the Appendix. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5% and 10% level, respectively.

Dependent variable =	*Ln*(*Compensation*)	*Ln*(*Compensation*)
	(1)	(2)
Treat × Post × Performance	-0.895**	-0.370***
	(-2.131)	(-2.659)
Post × Performance	-0.364	-0.073
	(-1.620)	(-1.025)
Treat × Performance	1.801***	0.631***
	(4.880)	(4.938)
Treat × Post	-0.182***	-0.189***
	(-6.472)	(-7.250)
Size	0.178***	0.180***
	(9.978)	(9.987)
Leverage	-0.035	-0.100
	(-0.552)	(-1.583)
Largest shareholder stake	-0.016	0.007
	(-0.153)	(0.069)
Cash flow	0.023	0.115
	(0.295)	(1.474)
ROA	0.909***	
	(4.329)	
ROS		0.162**
		(2.468)
Board size	-0.191***	-0.195***
	(-7.058)	(-7.214)
Independent director ratio	0.395***	0.400***
	(5.147)	(5.202)
Firm age	-0.004	0.011
	(-0.036)	(0.095)
CEO age	0.474***	0.477***
	(7.317)	(7.356)
CEO female(0/1)	-0.068*	-0.064
	(-1.734)	(-1.625)
CEO dual(0/1)	0.034	0.035
	(1.519)	(1.545)
Constant	7.727***	7.674***
	(13.656)	(13.466)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes
Adj R^2	0.662	0.660
N	17,889	17,888

Table 13. Executive pay caps and rent-seeking behavior

This table reports the effect of executive pay caps on rent-seeking behavior. Perk consumption (*Perk*) is the ratio of discretionary expenses to sales. *Tunneling* is measured by the natural logarithm of net other receivables. Definitions for all other variables are provided in the Appendix. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5% and 10% level, respectively.

Dependent variable =	Perk	Tunneling
	(1)	(2)
Treat × Post	-0.001**	-0.378***
	(-2.494)	(-9.033)
Size	-0.002***	0.904***
	(-7.601)	(25.931)
Leverage	-0.002**	2.590***
	(-2.161)	(21.026)
Largest shareholder stake	-0.004**	-0.421**
	(-2.434)	(-2.035)
Cash flow	-0.003***	0.292**
	(-2.772)	(2.187)
ROA	-0.016***	-0.217
	(-8.655)	(-1.121)
Board size	0.000	0.091**
	(0.077)	(2.146)
Independent director ratio	-0.001	-0.176
	(-1.011)	(-1.387)
Firm age	-0.000	0.825***
	(-0.106)	(4.462)
CEO age	-0.002***	0.187*
	(-2.769)	(1.895)
CEO female(0/1)	0.001	-0.007
	(1.107)	(-0.134)
CEO dual(0/1)	0.001***	-0.020
	(2.677)	(-0.586)
Constant	0.071***	-6.048***
	(8.426)	(-6.077)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes
Adj R^2	0.736	0.856
N	17,320	18,330

Table 14. Executive pay caps and politically salient outcomes

This table reports regression results on the impact of executive pay caps on firms' political and social objectives. Columns (1) and (2) examine firms' ESG and CSR performance, measured by Bloomberg ESG scores and Hexun CSR scores, respectively. Definitions of all variables are provided in the Appendix. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects, and standard errors are heteroscedasticity-robust and clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable =	ESG	CSR
	(1)	(2)
Treat × Post	0.900***	0.523***
	(2.611)	(3.845)
Size	1.117***	1.236***
	(4.730)	(12.754)
Leverage	-1.920**	-4.139***
	(-2.323)	(-11.383)
Largest shareholder stake	2.167*	2.756***
	(1.750)	(5.146)
Cash flow	0.120	1.990***
	(0.134)	(4.078)
ROA	2.637**	62.569***
	(2.137)	(62.777)
Board size	-0.190	-0.476***
	(-0.741)	(-3.344)
Independent director ratio	0.023	0.849**
	(0.029)	(2.025)
Firm age	0.364	-2.566***
	(0.233)	(-4.416)
Constant	-6.258	-7.376***
	(-0.876)	(-2.768)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes
Adj R^2	0.776	0.811
N	7,639	18,801

Table 15. Executive pay caps and labor-related outcomes

This table reports regression results on the impact of executive pay caps on labor-related outcomes, including *Employment* (natural logarithm of the number of employees), *Labor productivity* (sales (in millions) scaled by the number of employees), and *Wage-bill intensity* (total salary expenses scaled by total assets). Definitions of all variables are provided in the Appendix. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects, and standard errors are heteroskedasticity-robust and clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable =	Employment	Labor productivity	Wage-bill intensity
	(1)	(2)	(3)
Treat × Post	0.075***	-0.194***	-0.003**
	(3.302)	(-2.823)	(-2.113)
Size	0.622***	0.351***	-0.016***
	(28.168)	(5.446)	(-12.690)
Leverage	0.222***	0.349*	0.014***
	(2.980)	(1.844)	(3.651)
Largest shareholder stake	0.085	0.332	0.016**
	(0.643)	(0.790)	(2.572)
Cash flow	0.233***	0.658**	0.023***
	(3.673)	(2.301)	(7.260)
ROA	-0.029	1.967***	-0.017***
	(-0.283)	(6.300)	(-2.792)
Board size	-0.008	0.103*	0.001
	(-0.395)	(1.697)	(0.587)
Independent director ratio	-0.021	0.128	0.001
	(-0.373)	(0.656)	(0.453)
Firm age	0.296***	0.056	0.011*
	(2.818)	(0.155)	(1.914)
Constant	-7.089***	-6.941***	0.383***
	(-12.612)	(-4.219)	(11.783)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes
Adj R^2	0.929	0.769	0.849
N	18,817	18,817	18,401

Internet Appendix

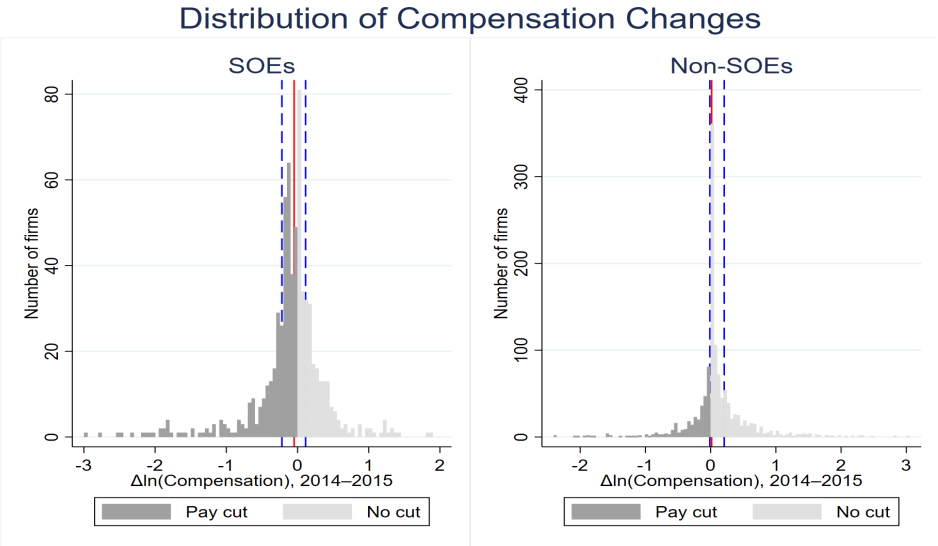
Appendix IA.1. Relative CEO compensation in financial SOEs versus non-financial SOEs

This figure depicts the evolution of the average compensation gap between Financial SOEs and non-Financial SOEs from 2011 to 2019, covering the four years before and after the introduction of the executive pay caps in early 2015.

Appendix IA.2. Distribution of compensation changes following the executive pay caps

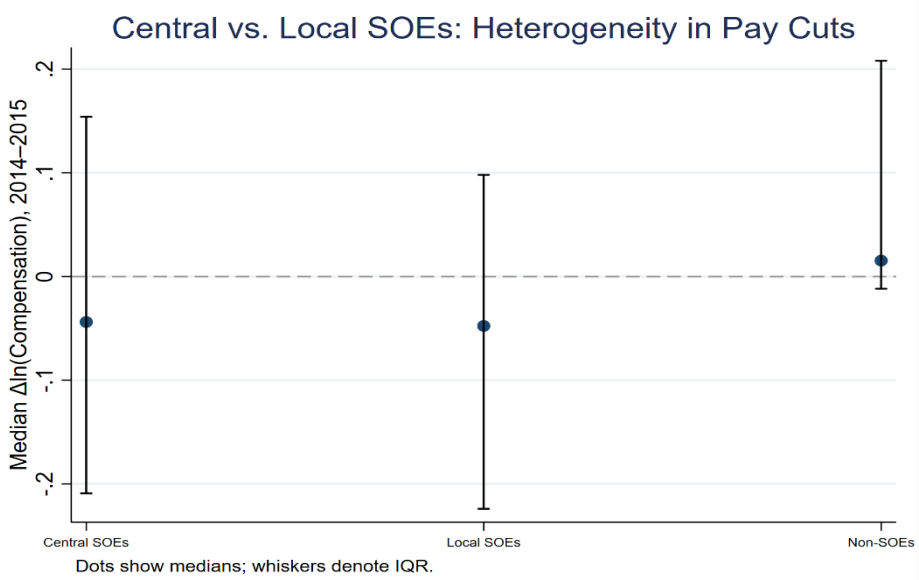
This figure illustrates firm-level changes in executive compensation from 2014 to 2015. Panel A plots histograms of changes in log compensation ($\Delta Ln(Compensation)$) separately for SOEs and non-SOEs. Dark shaded bars indicate firms experiencing compensation declines (pay cuts), while lighter bars indicate firms with no cut. The red vertical line denotes the median change, and the blue dashed lines mark the interquartile range (25^{th}–75^{th} percentiles). Panel B compares the median compensation changes across central SOEs, local SOEs, and non-SOEs. Dots represent group medians and whiskers denote interquartile ranges. Both central and local SOEs display sizable and similar median pay cuts with overlapping dispersion, while non-SOEs show little change. Panel C compares the distribution of the executive–worker pay multiple for SOEs in 2014 and 2015. The dashed vertical line indicates the 10.4 benchmark.

Panel A. Firm-level distributions of compensation changes: SOEs vs. non-SOEs



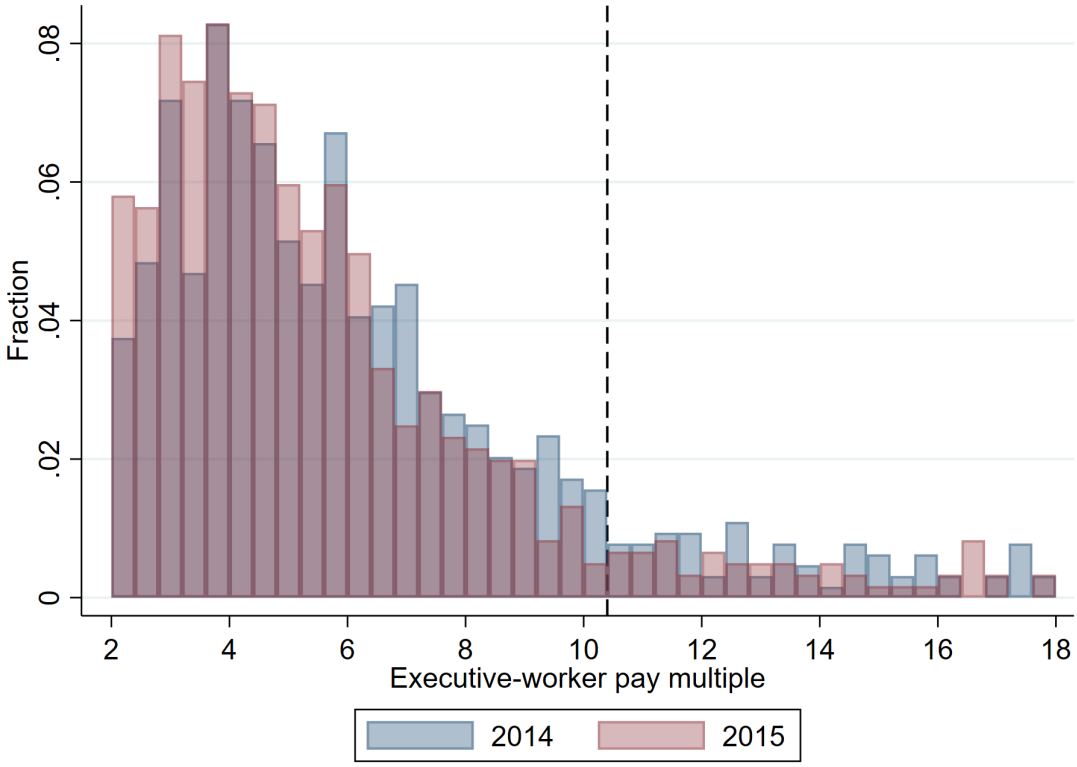
Red vertical line denotes the median; blue lines denote the interquartile range (25th–75th percentiles).

Panel B. Central vs. local SOEs: pay cuts "waterfall" comparison



Dots show medians; whiskers denote IQR.

Panel C. Distribution around a 10.4x benchmark, SOE only



Appendix IA.3. Investment–Q sensitivity using alternative opportunity proxies

This table examines investment–Q sensitivity using alternative proxies for firms' investment opportunities. In column (1), the opportunity proxy is the firm's lagged Tobin's Q. In column (2), the opportunity proxy is the industry median Tobin's Q. The dependent variable is *Investment*. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. Standard errors are clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable =	Investment	Investment
	(1)	(2)
Treat × Post	0.005	0.003
	(1.374)	(0.387)
Treat × Post × L.Tobin's Q	-0.008***	
	(-4.187)	
Treat × Post × Industry Median Q		-0.007*
		(-1.766)
L.Tobin's Q	-0.001	
	(-1.422)	
Treat × L.Tobin's Q	0.006***	
	(3.449)	
Treat × Industry Median Q		0.002
		(0.461)
Post × L.Tobin's Q	0.003***	
	(3.213)	
Size	0.004***	0.004***
	(2.826)	(2.885)
Leverage	-0.000	-0.003
	(-0.063)	(-0.660)
Largest shareholder stake	0.024***	0.029***
	(2.843)	(3.672)
Cash flow	0.005	0.009*
	(0.923)	(1.747)
ROA	0.034***	0.036***
	(4.597)	(5.102)
Board size	-0.006***	-0.005***
	(-3.427)	(-3.221)
Independent director ratio	0.005	0.005
	(1.024)	(1.101)
Firm age	-0.031***	-0.033***
	(-3.136)	(-3.883)
Constant	0.038	0.053
	(0.893)	(1.409)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes
Adj R^2	0.477	0.461
N	15,892	18,807

Appendix IA.4. Ruling out the role of financial constraints on investment efficiency

This table examines whether the observed decline in investment efficiency following the executive pay-cap reform is driven by pre-existing differences in firms' financing constraints. We split the sample into two groups based on firms' ex-ante levels of financial constraint, measured by the SA index. Columns (1)–(2) report results for investment, and columns (3)–(4) report results for investment inefficiency, separately for firms with high versus low financial constraints. All variable definitions are provided in the Appendix. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. T-statistics are reported in parentheses, with standard errors clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable =	Investment		Investment inefficiency	
	High SA	Low SA	High SA	Low SA
	(1)	(2)	(3)	(4)
Treat × Post	0.000	-0.001	0.006***	0.003***
	(0.069)	(-0.207)	(3.489)	(2.586)
Treat × Post × Tobin's Q	-0.004**	-0.006***		
	(-2.017)	(-2.617)		
Tobin's Q	0.000	0.001		
	(0.463)	(1.568)		
Treat × Tobin's Q	-0.001	0.003*		
	(-0.247)	(1.815)		
Post × Tobin's Q	0.002***	0.001		
	(3.166)	(1.029)		
Size	0.006***	0.004**	-0.000	-0.001
	(2.710)	(2.231)	(-0.017)	(-0.880)
Leverage	0.005	-0.009	0.000	-0.000
	(0.640)	(-1.511)	(0.013)	(-0.015)
Largest shareholder stake	0.050***	0.003	0.014**	0.002
	(4.231)	(0.342)	(2.116)	(0.443)
Cash flow	0.019**	-0.000	0.005	-0.002
	(2.280)	(-0.031)	(0.890)	(-0.433)
ROA	0.038***	0.036***	0.013**	0.014**
	(3.699)	(3.510)	(2.118)	(2.524)
Board size	-0.009***	-0.004*	-0.000	0.000
	(-3.617)	(-1.742)	(-0.228)	(0.180)
Independent director ratio	0.019**	-0.007	-0.005	-0.005
	(2.573)	(-0.968)	(-1.168)	(-1.084)
Firm age	-0.016	-0.047**	-0.000	-0.020
	(-1.243)	(-1.973)	(-0.005)	(-1.513)
Constant	-0.040	0.102	0.020	0.099**
	(-0.693)	(1.241)	(0.610)	(2.301)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes
Adj R^2	0.442	0.489	0.204	0.221
N	8,932	9,088	7,412	7,792

Appendix IA.5. Estimating the optimal cash holdings

This table presents the estimation of the optimal cash holdings (*Cash**). The dependent variable is corporate cash holdings (*Cash*). All other variables are defined in the Appendix. The regression includes firm fixed effects as well as province-by-year and industry-by-year fixed effects. The t-statistics are presented in parentheses, with standard errors clustered at the firm level. ***, **, * indicate significance at 1%, 5%, and 10%, respectively.

Dependent variable =	Cash
	(1)
Size	0.000
	(0.101)
Leverage	-0.258***
	(-21.054)
Largest shareholder stake	0.047**
	(2.368)
Cash flow	0.193***
	(13.686)
ROA	0.036*
	(1.873)
Board size	0.008**
	(2.022)
Independent director ratio	-0.034***
	(-2.812)
Firm age	-0.160***
	(-7.295)
Dividend(0/1)	0.011***
	(5.575)
Repurchase(0/1)	-0.006
	(-1.404)
Constant	0.678***
	(7.201)
Firm, Province × Year FE, and Industry × Year FE	Yes
Adj R^2	0.628
N	18,825

Appendix IA.6. Executive pay caps and corporate cash management using an alternative cash proxy

This table presents the impact of the executive pay-cap regulation on cash management using an alternative cash proxy. Panel A examines the effect of executive pay caps on corporate cash level, measured by the ratio of cash and cash equivalents to net assets (*Cash1*). Columns (1) to (3) present the regression results that include industry-fixed effects, industry and year-fixed effects, and firm fixed effects, as well as province-by-year and industry-by-year fixed effects, respectively. Panel B reports the effect of executive pay caps on the adjustment speed of corporate cash holdings. The dependent variable is *ΔCash1*, the one-year change in cash holdings. The key variable of interest is *Treat* × *Post* × $(Cash1_t^* - Cash1_{t-1})$, where $Cash1_t^* - Cash1_{t-1}$ measures the gap between the target cash ratio and the beginning-of-year cash ratio. A positive value indicates a cash shortfall (actual cash below target), whereas a negative value indicates excess cash (actual cash above target). Column (1) reports results for the full sample. Columns (2) and (3) report subsample results for firms with excess cash and cash shortfall, respectively, where excess cash is defined as $Cash1_{t-1} > Cash1_t^*$ and cash shortfall is defined as $Cash1_{t-1} < Cash1_t^*$. Panel C reports the results of the impact of executive pay caps on the marginal value of cash using Faulkender and Wang's (2006) model. The dependent variable is *r–R*, the difference between a firm's yearly stock returns (r) and the benchmark return (R). The regression results in columns (1)-(3) use different fixed effects. Definitions for all other variables are provided in the Appendix. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. Statistical significance is denoted by ***, **, and * at the 1%, 5%, and 10% levels, respectively.

Panel A. Impact of executive pay caps on corporate cash holdings

Dependent variable =	Cash1	Cash1	Cash1
	(1)	(2)	(3)
Treat × Post	0.120***	0.125***	0.098***
	(12.709)	(13.137)	(9.114)
Treat	-0.044***	-0.053***	
	(-4.441)	(-5.268)	
Post	-0.119***		
	(-14.520)		
Size	-0.015***	-0.013***	0.013*
	(-3.940)	(-3.351)	(1.664)
Leverage	-0.456***	-0.462***	-0.498***
	(-18.867)	(-19.055)	(-17.626)
Largest shareholder stake	0.090***	0.090***	0.056
	(3.592)	(3.624)	(1.181)
Cash flow	0.348***	0.382***	0.430***
	(8.546)	(9.363)	(12.478)
ROA	0.295***	0.227***	-0.032
	(5.604)	(4.221)	(-0.730)
Board size	-0.007	0.004	0.017*
	(-0.648)	(0.392)	(1.692)
Independent director ratio	-0.103***	-0.049	-0.087***
	(-2.832)	(-1.351)	(-3.044)
Firm age	-0.062***	-0.038***	-0.365***
	(-4.756)	(-2.801)	(-6.213)

Dividend(0/1)	0.012**	0.014**	0.016***
	(1.967)	(2.371)	(3.310)
Repurchase(0/1)	-0.025***	-0.021**	-0.012
	(-3.114)	(-2.446)	(-1.301)
Constant	0.992***	0.770***	1.126***
	(12.090)	(8.982)	(4.852)
Year FE	No	Yes	-
Industry FE	Yes	Yes	-
Firm, Province × Year FE, and Industry × Year FE	No	No	Yes
Adj R^2	0.265	0.281	0.589
N	18,825	18,825	18,825

Panel B. Impact of executive pay caps on cash adjustment speed

	Full sample	Excess cash	Cash shortfall
Dependent variable =	$\Delta Cash1_t$	$\Delta Cash1_t$	$\Delta Cash1_t$
	(1)	(2)	(3)
Treat × Post × ($Cash1_t^*$–$Cash1_{t-1}$)	-0.196***	-0.095***	0.038
	(-5.780)	(-2.906)	(1.465)
$Cash1_t^*$–$Cash1_{t-1}$	0.493***	0.972***	0.892***
	(26.934)	(38.884)	(61.943)
Treat × Post	0.009	0.007	-0.006
	(1.573)	(1.207)	(-1.274)
Treat×($Cash1_t^*$–$Cash1_{t-1}$)	0.063*	-0.004	-0.020
	(1.867)	(-0.130)	(-0.844)
Post×($Cash1_t^*$–$Cash1_{t-1}$)	0.279***	0.092***	0.033*
	(10.645)	(3.558)	(1.885)
Constant	-0.023***	0.076***	-0.091***
	(-16.507)	(36.694)	(-98.487)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes
Adj R^2	0.458	0.770	0.872
N	16,387	7,988	8,399

Panel C. Marginal value of cash

Dependent variable =	r-R	r-R	r-R
	(1)	(2)	(3)
Treat × Post × ΔCash1	-0.335***	-0.382***	-0.236*
	(-2.748)	(-3.107)	(-1.749)
Post × Cash1	0.060	0.077	-0.011
	(1.072)	(1.382)	(-0.187)
Treat × ΔCash1	0.076	0.093	-0.059
	(0.862)	(1.051)	(-0.607)
Treat × Post	-0.066***	-0.067***	-0.060***
	(-5.698)	(-5.769)	(-4.426)
Treat	0.017*	0.017*	
	(1.816)	(1.791)	
Post	0.012		
	(1.548)		
ΔCash1	0.196***	0.181***	0.308***
	(2.986)	(2.780)	(4.293)
ΔEarnings	0.558***	0.579***	0.514***
	(12.989)	(13.479)	(11.976)
ΔNet Assets	0.288***	0.277***	0.283***
	(19.843)	(19.365)	(17.995)
ΔInterest Expenses	-0.489*	-0.349	-0.217
	(-1.804)	(-1.308)	(-0.788)
$Cash1_{t-1}$	0.033**	0.036**	0.045**
	(2.227)	(2.412)	(2.087)
Net Financing	-0.129***	-0.124***	-0.111***
	(-5.720)	(-5.531)	(-4.496)
Leverage	-0.055***	-0.052***	-0.103***
	(-17.161)	(-16.289)	(-16.839)
Leverage×ΔCash1	0.338***	0.317***	0.279***
	(7.460)	(7.072)	(5.892)
$Cash1_{t-1}$×ΔCash1	-0.174***	-0.157***	-0.204***
	(-3.013)	(-2.742)	(-3.298)
ΔDividends	1.430***	1.475***	1.462***
	(7.033)	(7.315)	(6.967)
Constant	0.045***	0.052***	0.094***
	(5.891)	(11.069)	(13.637)
Year FE	No	Yes	-
Industry FE	Yes	Yes	-
Firm, Province × Year FE, and Industry × Year FE	No	No	Yes
Adj R^2	0.122	0.132	0.147
N	16,323	16,323	16,323

Appendix IA.7. Heterogeneity by provincial enforcement strictness

This table examines whether the estimated effects of the executive pay-cap regulation differ in provinces with stricter enforcement of compensation restrictions. The triple interaction term Strict province × Treat × Post captures whether SOEs in stricter provinces exhibit differential post-policy changes relative to other firms. All variables are defined in the Appendix. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. t-statistics are reported in parentheses, with standard errors clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Investor Interactions (0/1)	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)	(6)
Strict province × Treat × Post	0.015	0.029**	0.016	0.001	-0.019	-0.002
	(1.118)	(2.269)	(0.590)	(0.030)	(-0.515)	(-0.690)
Strict province × Post	0.106***	-0.009	0.083	-0.103	-0.061	0.026**
	(2.642)	(-0.255)	(1.250)	(-1.094)	(-1.029)	(2.124)
Treat × Post	-0.035***	-0.037***	-0.080***	-0.123***	-0.070**	-0.009***
	(-2.968)	(-3.339)	(-3.330)	(-4.428)	(-2.100)	(-3.396)
Size	0.012***	0.005	0.069***	0.022**	0.047***	0.003**
	(2.790)	(1.043)	(7.221)	(2.325)	(4.207)	(2.517)
Leverage	-0.016	-0.034**	-0.078**	0.022	0.058	-0.005
	(-1.027)	(-1.972)	(-2.206)	(0.610)	(1.448)	(-1.047)
Largest shareholder stake	-0.058*	-0.035	-0.027	-0.025	-0.122*	0.029***
	(-1.851)	(-1.323)	(-0.441)	(-0.442)	(-1.757)	(3.716)
Cash flow	-0.002	0.004	-0.061	0.032	-0.071	0.008
	(-0.071)	(0.180)	(-1.305)	(0.672)	(-1.253)	(1.548)
ROA	-0.027	-0.043	0.365***	0.118*	-0.046	0.040***
	(-0.680)	(-1.384)	(5.402)	(1.895)	(-0.545)	(5.594)
Board size	-0.004	-0.011	-0.002	-0.005	0.024	-0.005***
	(-0.441)	(-1.636)	(-0.110)	(-0.320)	(1.348)	(-3.434)
Independent director ratio	-0.015	0.045**	-0.018	0.023	0.058	0.006
	(-0.667)	(2.045)	(-0.382)	(0.485)	(1.017)	(1.144)

Firm age	0.028	-0.018	0.041	0.098*	-0.004	-0.031***
	(0.770)	(-0.534)	(0.562)	(1.701)	(-0.040)	(-3.739)
Constant	-0.323**	0.321**	-1.441***	-0.543**	4.374***	0.053
	(-2.365)	(2.332)	(-4.887)	(-2.092)	(13.026)	(1.360)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.129	0.276	0.365	0.120	0.487	0.466
N	18,524	14,203	16,688	18,524	18,524	18,513

Appendix IA.8. Heterogeneity by strategic industries

This table examines whether the estimated effects of the executive pay-cap regulation differ between firms in strategic industries and those in other industries. The triple-interaction term Strategic industries × Treat × Post captures whether SOEs in strategic industries exhibit differential post-policy changes relative to other firms. Strategic Industries is an indicator equal to one if a firm belongs to a strategic industry, and zero otherwise. All variable definitions are provided in the Appendix. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. t-statistics are reported in parentheses, with standard errors clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Investor Interactions (0/1)	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)	(6)
Strategic industries × Treat × Post	-0.049*	-0.016	0.037	0.090*	0.014	-0.019***
	(-1.905)	(-0.859)	(0.843)	(1.822)	(0.218)	(-3.710)
Strategic industries × Post	0.000	0.014	-0.019	-0.067**	0.046	0.013***
	(0.011)	(1.038)	(-0.553)	(-2.248)	(1.616)	(3.361)
Treat × Post	-0.019**	-0.014**	-0.073***	-0.130***	-0.084***	-0.008***
	(-2.575)	(-2.157)	(-4.798)	(-7.139)	(-4.612)	(-4.172)
Size	0.012***	0.006	0.069***	0.021**	0.048***	0.003***
	(2.704)	(1.235)	(7.236)	(2.310)	(4.365)	(2.609)
Leverage	-0.015	-0.034**	-0.082**	0.020	0.060	-0.003
	(-0.994)	(-2.058)	(-2.367)	(0.581)	(1.543)	(-0.668)
Largest shareholder stake	-0.050	-0.031	-0.017	-0.013	-0.118*	0.028***
	(-1.627)	(-1.156)	(-0.278)	(-0.231)	(-1.755)	(3.619)
Cash flow	-0.002	0.003	-0.070	0.041	-0.071	0.008
	(-0.075)	(0.116)	(-1.487)	(0.863)	(-1.276)	(1.625)
ROA	-0.030	-0.047	0.364***	0.120**	-0.048	0.038***
	(-0.754)	(-1.563)	(5.495)	(1.979)	(-0.583)	(5.440)
Board size	-0.005	-0.013*	-0.003	-0.004	0.024	-0.005***
	(-0.579)	(-1.868)	(-0.190)	(-0.275)	(1.344)	(-3.176)
Independent director ratio	-0.013	0.049**	-0.016	0.015	0.058	0.005
	(-0.593)	(2.261)	(-0.325)	(0.310)	(1.030)	(1.061)

Firm age	0.025	-0.016	0.042	0.093	0.003	-0.033***
	(0.690)	(-0.471)	(0.570)	(1.624)	(0.038)	(-3.889)
Constant	-0.262*	0.292**	-1.389***	-0.558**	4.314***	0.062*
	(-1.940)	(2.169)	(-4.751)	(-2.208)	(13.210)	(1.661)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.134	0.275	0.364	0.119	0.489	0.462
N	18,825	14,406	16,933	18,825	18,825	18,814

Appendix IA.9. Heterogeneous effects across central and local SOEs

This table examines whether the estimated effects of the executive pay-cap regulation differ by ownership type among state-owned enterprises (SOEs). Specifically, we replace the unified SOE treatment indicator with two separate variables: *Central SOE*, which equals one for central SOEs, and *Local SOE*, which equals one for local SOEs. All variables are defined in the Appendix. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. t-statistics are reported in parentheses, with standard errors clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Investor Interactions (0/1)	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)	(6)
Central SOE × Post	-0.039***	-0.004	-0.087***	-0.140***	-0.092**	-0.011***
	(-3.102)	(-0.344)	(-3.999)	(-4.697)	(-2.328)	(-3.935)
Local SOE × Post	-0.020***	-0.020***	-0.063***	-0.113***	-0.078***	-0.010***
	(-2.621)	(-2.924)	(-3.986)	(-6.098)	(-4.685)	(-4.954)
Size	0.012***	0.006	0.068***	0.019**	0.049***	0.004***
	(2.701)	(1.336)	(7.204)	(2.128)	(4.426)	(2.770)
Leverage	-0.015	-0.034**	-0.082**	0.021	0.058	-0.003
	(-0.930)	(-2.064)	(-2.381)	(0.597)	(1.482)	(-0.687)
Largest shareholder stake	-0.048	-0.030	-0.018	-0.020	-0.113*	0.030***
	(-1.572)	(-1.114)	(-0.300)	(-0.349)	(-1.691)	(3.808)
Cash flow	-0.000	0.003	-0.069	0.040	-0.071	0.009*
	(-0.017)	(0.128)	(-1.486)	(0.846)	(-1.278)	(1.692)
ROA	-0.028	-0.048	0.366***	0.124**	-0.050	0.038***
	(-0.719)	(-1.602)	(5.518)	(2.031)	(-0.615)	(5.361)
Board size	-0.005	-0.013*	-0.002	-0.003	0.024	-0.005***
	(-0.551)	(-1.931)	(-0.151)	(-0.206)	(1.341)	(-3.240)
Independent director ratio	-0.014	0.049**	-0.017	0.013	0.057	0.005
	(-0.599)	(2.284)	(-0.348)	(0.268)	(1.021)	(1.091)

Firm age	0.026	-0.018	0.045	0.098*	0.004	-0.033***
	(0.725)	(-0.523)	(0.619)	(1.714)	(0.047)	(-3.912)
Constant	-0.264*	0.289**	-1.387***	-0.540**	4.299***	0.058
	(-1.959)	(2.142)	(-4.752)	(-2.139)	(13.048)	(1.545)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.134	0.275	0.364	0.119	0.489	0.461
N	18,825	14,406	16,933	18,825	18,825	18,814

Appendix IA.10. Heterogeneity by pay gap

This table presents a robustness check examining whether the estimated effects of the executive pay-cap regulation differ by the size of a firm's executive–worker pay gap in the baseline year. Specifically, we replace the unified SOE treatment indicator with two mutually exclusive variables: *High pay gap SOE* equals 1 for SOE firms whose 2014 executive-to-average-employee pay gap is above the sample median (and 0 otherwise). *Low pay gap SOE* equals 1 for SOE firms whose 2014 pay gap is at or below the median. All variables are defined in the Appendix. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. t-statistics are shown in parentheses, with standard errors clustered at the firm level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Investor Interactions (0/1)	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)	(6)
High pay gap SOE × Post	-0.030***	-0.010	-0.084***	-0.137***	-0.086***	-0.013***
	(-2.955)	(-1.189)	(-4.534)	(-5.945)	(-3.363)	(-5.636)
Low pay gap SOE × Post	-0.025***	-0.026***	-0.053***	-0.114***	-0.077***	-0.009***
	(-3.167)	(-3.301)	(-3.001)	(-5.169)	(-3.230)	(-4.084)
Size	0.013***	0.003	0.068***	0.020**	0.046***	0.003**
	(2.827)	(0.717)	(6.796)	(2.080)	(3.945)	(2.528)
Leverage	-0.018	-0.037**	-0.079**	0.032	0.072*	-0.004
	(-1.077)	(-2.072)	(-2.168)	(0.864)	(1.702)	(-0.874)
Largest shareholder stake	-0.057*	-0.029	-0.024	-0.012	-0.132*	0.027***
	(-1.736)	(-1.071)	(-0.383)	(-0.210)	(-1.812)	(3.360)
Cash flow	-0.001	0.012	-0.061	0.035	-0.078	0.005
	(-0.054)	(0.499)	(-1.239)	(0.717)	(-1.304)	(0.948)
ROA	-0.039	-0.042	0.363***	0.115*	-0.029	0.041***
	(-0.907)	(-1.336)	(5.188)	(1.801)	(-0.327)	(5.531)
Board size	-0.005	-0.010	0.000	-0.006	0.025	-0.005***
	(-0.496)	(-1.450)	(0.009)	(-0.380)	(1.329)	(-3.147)

Independent director ratio	-0.004	0.052**	-0.012	0.053	0.054	0.005
	(-0.160)	(2.316)	(-0.241)	(1.056)	(0.880)	(0.980)
Firm age	0.016	-0.026	0.040	0.108*	0.002	-0.033***
	(0.445)	(-0.748)	(0.522)	(1.846)	(0.026)	(-3.785)
Constant	-0.268*	0.364***	-1.366***	-0.589**	4.349***	0.065
	(-1.881)	(2.599)	(-4.464)	(-2.237)	(12.481)	(1.640)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.127	0.273	0.365	0.120	0.479	0.460
N	17,342	13,273	15,584	17,342	17,342	17,332

Appendix IA.11. Heterogeneity by SOE political connections

This table investigates the influence of political connections on the relationship between executive pay caps and corporate behavior. *PC SOE* refers to whether managers are politically connected, measured by whether they are former government employees (Griffin et al., 2022). Controls are the same as in Eq. (1). All variables are defined in the Appendix. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. The t-statistics are presented in parentheses, with standard errors clustered at the firm level. ***, **, * indicate significance at 1%, 5%, and 10%, respectively.

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Investor Interactions (0/1)	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)	(6)
PC SOE × Post	-0.025	0.017	-0.068***	-0.130***	-0.106**	-0.017***
	(-1.350)	(1.196)	(-2.779)	(-3.947)	(-2.384)	(-4.429)
Non-PC SOE × Post	-0.025***	-0.021***	-0.069***	-0.121***	-0.079***	-0.010***
	(-3.608)	(-3.177)	(-4.585)	(-6.679)	(-4.193)	(-5.511)
Size	0.013***	0.005	0.070***	0.021**	0.047***	0.003**
	(2.844)	(1.082)	(7.267)	(2.311)	(4.189)	(2.513)
Leverage	-0.017	-0.034**	-0.078**	0.022	0.059	-0.005
	(-1.072)	(-2.033)	(-2.205)	(0.620)	(1.471)	(-1.043)
Largest shareholder stake	-0.057*	-0.035	-0.026	-0.025	-0.122*	0.029***
	(-1.826)	(-1.333)	(-0.423)	(-0.443)	(-1.759)	(3.714)
Cash flow	-0.000	0.005	-0.061	0.031	-0.073	0.008
	(-0.019)	(0.218)	(-1.293)	(0.645)	(-1.286)	(1.532)
ROA	-0.027	-0.044	0.364***	0.118*	-0.045	0.040***
	(-0.683)	(-1.416)	(5.391)	(1.891)	(-0.533)	(5.611)
Board size	-0.004	-0.012*	-0.002	-0.005	0.025	-0.005***
	(-0.434)	(-1.672)	(-0.109)	(-0.333)	(1.360)	(-3.374)
Independent director ratio	-0.014	0.045**	-0.018	0.022	0.057	0.006
	(-0.630)	(2.070)	(-0.371)	(0.476)	(0.997)	(1.140)
Firm age	0.027	-0.019	0.040	0.097*	-0.003	-0.031***
	(0.759)	(-0.571)	(0.550)	(1.683)	(-0.036)	(-3.711)

Constant	-0.286**	0.317**	-1.409***	-0.579**	4.354***	0.062
	(-2.086)	(2.329)	(-4.772)	(-2.253)	(12.931)	(1.631)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.129	0.276	0.365	0.120	0.487	0.465
N	18,524	14,203	16,688	18,524	18,524	18,513

Appendix IA.12. Instrumental variable estimates using exposure to the pay-cap regulation

This table reports the 2SLS estimates based on differential exposure to the 2015 executive pay-cap regulation. The instrumental variable exploits cross-firm variation in the pre-policy executive-to-worker pay multiple, helping to distinguish compensation compression from average SOE policy exposure. The endogenous regressor is *PayCut*, defined as $-\Delta Ln(Compensation)$, with larger values indicating larger compensation cuts. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. All variables are defined in the Appendix. Standard errors are clustered at the firm level, and t-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Investor Interactions (0/1)	Unrelated Acquisition (0/1)	Risk Tone	Investment	Cash	Tobin's Q
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\widehat{PayCut}$	-0.055	-0.024	-0.592***	-0.830***	-0.624**	-0.080***	0.207***	-2.475***
	(-0.498)	(-0.293)	(-2.838)	(-3.143)	(-2.059)	(-3.055)	(4.001)	(-4.763)
Size	0.011**	0.007	0.085***	0.040**	0.056***	0.006***	0.002	-0.720***
	(2.101)	(1.351)	(5.899)	(2.430)	(3.267)	(2.918)	(0.438)	(-14.874)
Leverage	0.001	-0.043**	-0.119**	-0.003	0.066	-0.011	-0.251***	0.720***
	(0.086)	(-2.024)	(-2.139)	(-0.042)	(1.149)	(-1.505)	(-13.687)	(3.843)
Largest shareholder stake	-0.097**	-0.048	-0.098	-0.131	-0.223**	0.015	0.060**	-0.848***
	(-2.453)	(-1.614)	(-1.169)	(-1.484)	(-2.405)	(1.451)	(2.166)	(-3.225)
Cash flow	-0.011	0.011	-0.081	-0.019	-0.099	-0.003	0.219***	0.460*
	(-0.410)	(0.394)	(-1.032)	(-0.204)	(-1.142)	(-0.357)	(9.066)	(1.808)
ROA	-0.101	-0.085	-0.173	-0.690**	-0.605**	-0.034	0.224***	-0.334
	(-0.903)	(-1.034)	(-0.829)	(-2.541)	(-1.974)	(-1.274)	(3.922)	(-0.550)
Board size	0.012	-0.007	0.145***	0.176***	0.157**	0.012*	-0.037***	0.554***
	(0.439)	(-0.401)	(2.621)	(2.653)	(2.297)	(1.940)	(-2.637)	(3.869)
Independent director ratio	-0.021	0.058	-0.241**	-0.161	-0.114	-0.021*	0.040	-0.508*
	(-0.474)	(1.637)	(-2.374)	(-1.430)	(-0.975)	(-1.888)	(1.540)	(-1.861)
Firm age	0.018	-0.028	0.132	0.165*	0.009	-0.030***	-0.142***	0.749***
	(0.430)	(-0.758)	(1.343)	(1.841)	(0.079)	(-2.697)	(-5.260)	(2.976)

First-stage results								
Treat × pay gap$_{2014}$ × Post	0.008***	0.008***	0.007***	0.008***	0.008***	0.008***	0.008***	0.008***
	(6.349)	(4.972)	(5.134)	(6.349)	(6.349)	(6.338)	(6.349)	(6.322)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap rk Wald F statistic	40.305	24.720	26.359	40.305	40.305	40.170	40.305	39.961
N	15,823	12,133	14,325	15,823	15,823	15,816	15,823	15,389

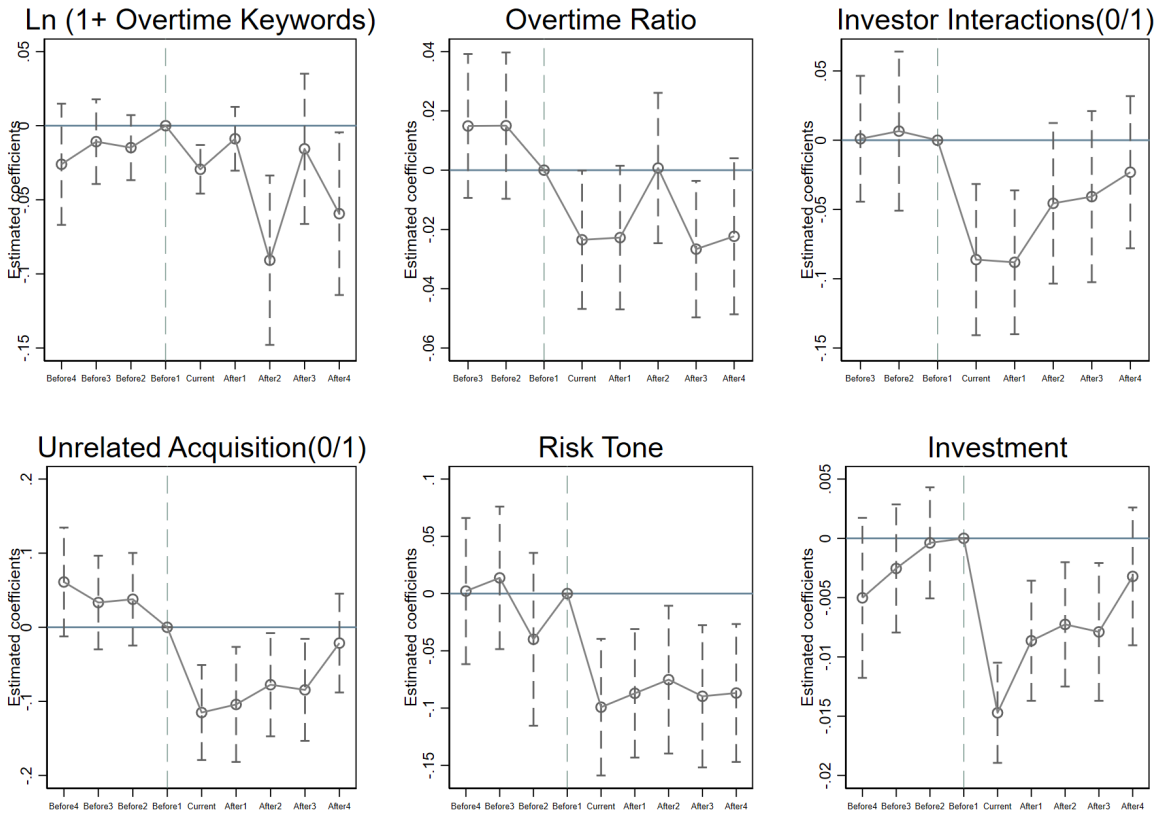
Appendix IA.13. Diagnostic tests of the entropy-balancing reweighted sample

This table reports univariate comparisons between treated and control firms before and after entropy balancing. U denotes the unweighted sample, while M denotes the entropy-balanced reweighted sample. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5% and 10% level, respectively.

		Treatment firms	Control firms		
Variables		Mean	Mean	Mean-Diff	T value
Size	U	22.852	21.867	0.985***	53.86
	M	22.852	22.852	0.000	0
Leverage	U	0.514	0.385	0.130***	42.75
	M	0.514	0.514	0.000	0
Largest shareholder stake	U	0.401	0.321	0.081***	37.31
	M	0.401	0.401	0.000	0
Cash flow	U	0.044	0.040	0.004***	4.06
	M	0.044	0.044	0.000	0
ROA	U	0.030	0.035	-0.005***	-5.67
	M	0.030	0.030	0.000	0
Board size	U	2.362	2.244	0.118***	32.44
	M	2.362	2.362	0.000	0
Independent director ratio	U	0.370	0.387	-0.017***	-15.78
	M	0.370	0.370	0.000	0
Firm age	U	2.897	2.763	0.134***	25.56
	M	2.897	2.897	0.000	0

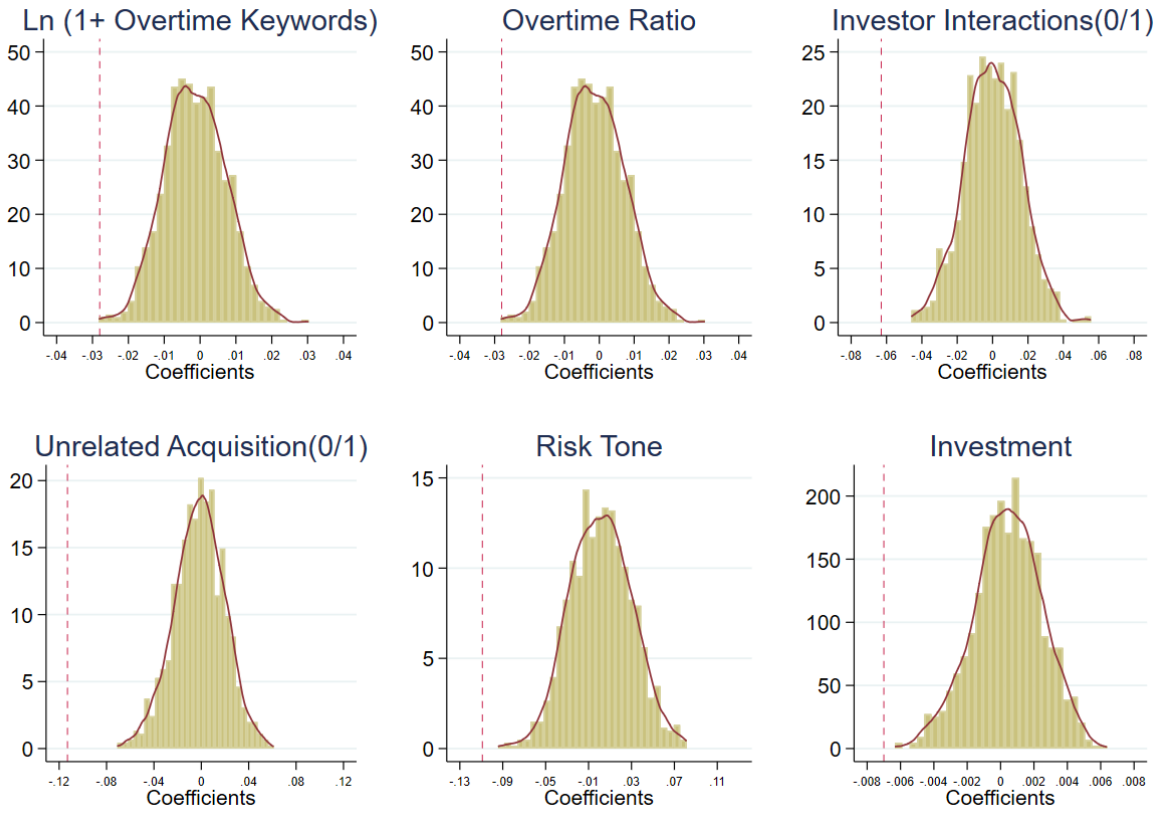
Appendix IA.14. Dynamic effects using the entropy-balancing reweighted sample

This figure illustrates the time-varying effects of the executive pay-cap regulation on six direct behavioral outcomes using the entropy-balancing reweighted sample. Definitions of all dependent variables are provided in the Appendix. The year 2014 serves as the baseline, so each coefficient reflects the change in the respective outcome relative to 2014. The statistically insignificant coefficients before 2015 suggest that the treatment and control groups followed parallel pre-trends. In contrast, the negative and statistically significant coefficients after 2015 indicate a notable decline in these behavioral outcomes following the implementation of the regulation. Vertical lines represent 95% confidence intervals.



Appendix IA.15. Placebo tests using the entropy-balancing reweighted sample

This figure displays the kernel density of 1,000 placebo estimates for the coefficient on *Treat_F* × *Post*, based on the entropy-balancing reweighted sample. The placebo estimates are generated by randomly reassigning the treatment status across firms. Placebo tests are conducted for all six direct behavioral outcomes. In each specification, the distribution of placebo estimates is centered around zero, while the actual estimate lies outside this distribution, indicating that the observed effects are unlikely to be driven by random chance.



Appendix IA.16. Robustness tests: Controlling for time-varying financing conditions

This table examines whether our baseline results are robust to controlling for time-varying financing conditions. Columns (1)–(6) re-estimate the main specifications while additionally controlling for *Cost of Debt*, measured as interest expense scaled by average debt, and *Credit Spread*, measured as bond yield spreads relative to matched China Government Bonds. The regressions also include issuer-by-year fixed effects to absorb issuer-specific time-varying shocks in the bond market. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. Standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Investor Interactions (0/1)	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	-0.025***	-0.016**	-0.069***	-0.117***	-0.081***	-0.011***
	(-3.540)	(-2.521)	(-4.714)	(-6.772)	(-4.636)	(-5.761)
Size	0.013***	0.006	0.070***	0.019**	0.049***	0.003**
	(2.897)	(1.419)	(7.353)	(2.125)	(4.419)	(2.481)
Leverage	-0.014	-0.032*	-0.073**	0.017	0.061	0.000
	(-0.903)	(-1.869)	(-2.071)	(0.476)	(1.434)	(0.074)
Largest shareholder stake	-0.050	-0.029	-0.021	-0.022	-0.113*	0.030***
	(-1.615)	(-1.097)	(-0.351)	(-0.385)	(-1.674)	(3.765)
Cash flow	-0.004	0.002	-0.071	0.041	-0.074	0.010*
	(-0.158)	(0.094)	(-1.517)	(0.856)	(-1.343)	(1.935)
ROA	-0.027	-0.048	0.367***	0.121**	-0.050	0.038***
	(-0.694)	(-1.581)	(5.535)	(1.995)	(-0.617)	(5.336)
Board size	-0.006	-0.013*	-0.003	-0.004	0.023	-0.005***
	(-0.618)	(-1.915)	(-0.212)	(-0.224)	(1.284)	(-3.198)
Independent director ratio	-0.012	0.049**	-0.014	0.013	0.061	0.005
	(-0.533)	(2.269)	(-0.296)	(0.287)	(1.107)	(1.006)
Firm age	0.022	-0.017	0.045	0.094*	0.007	-0.031***
	(0.613)	(-0.500)	(0.613)	(1.647)	(0.072)	(-3.698)

Cost of debt	0.016	-0.017	-0.065	0.029	-0.027	-0.046***
	(0.293)	(-0.298)	(-0.484)	(0.293)	(-0.153)	(-3.067)
Credit Spread	0.001	-0.000	-0.002	0.013	0.013*	-0.001
	(0.316)	(-0.031)	(-0.392)	(1.291)	(1.918)	(-1.594)
Constant	-0.283**	0.272**	-1.437***	-0.545**	4.268***	0.064*
	(-2.077)	(2.006)	(-4.893)	(-2.143)	(12.852)	(1.694)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Issuer × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.134	0.274	0.364	0.118	0.489	0.463
N	18,524	14,203	16,933	18,524	18,524	18,513

Appendix IA.17. The effect of the executive pay caps on cost of debt

This table estimates the impact of the 2015 executive pay caps on firms' financing costs. The dependent variable is the cost of debt, measured as interest expense scaled by average total debt. The specification includes firm fixed effects as well as province-by-year and industry-by-year fixed effects. Standard errors are clustered at the firm level. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5% and 10% level, respectively.

Dependent variable =	Cost of debt
	(1)
Treat × Post	-0.012***
	(-9.216)
Size	-0.004***
	(-3.734)
Leverage	0.089***
	(21.125)
Largest shareholder stake	-0.011*
	(-1.911)
Cash flow	0.016***
	(3.719)
ROA	0.002
	(0.267)
Board size	-0.001
	(-0.461)
Independent director ratio	0.003
	(0.872)
Firm age	0.048***
	(5.836)
Constant	-0.077**
	(-2.466)
Firm, Province × Year FE, and Industry × Year FE	Yes
Adj R^2	0.615
N	18,524

Appendix IA.18. Bond-Market Exposure and Triple-Difference Tests

This table examines whether the real-side effects of the pay caps vary by firms' ex-ante bond-market exposure. Issuer is an indicator for firms with bond-market access prior to the policy. The specification includes the triple interaction Issuer × Treat× Post, along with firm fixed effects and province-by-year and industry-by-year fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5% and 10% level, respectively.

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Investor Interactions (0/1)	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(3)	(4)	(5)
Issuer × Treat × Post	0.000	-0.008	0.043	-0.008	-0.013	0.003
	(0.011)	(-0.623)	(1.575)	(-0.239)	(-0.398)	(0.872)
Issuer × Post	0.006	-0.002	-0.025	0.002	0.007	-0.008***
	(0.510)	(-0.234)	(-1.251)	(0.088)	(0.422)	(-2.854)
SOE × Post	-0.025***	-0.013*	-0.081***	-0.117***	-0.078***	-0.010***
	(-3.187)	(-1.777)	(-4.757)	(-5.952)	(-3.973)	(-4.810)
Size	0.012***	0.006	0.069***	0.020**	0.049***	0.004***
	(2.775)	(1.250)	(7.255)	(2.141)	(4.433)	(2.719)
Leverage	-0.014	-0.034**	-0.085**	0.021	0.059	-0.004
	(-0.898)	(-2.066)	(-2.468)	(0.605)	(1.501)	(-0.825)
Largest shareholder stake	-0.049	-0.030	-0.018	-0.021	-0.115*	0.030***
	(-1.594)	(-1.125)	(-0.299)	(-0.378)	(-1.694)	(3.760)
Cash flow	-0.001	0.003	-0.072	0.040	-0.071	0.009*
	(-0.048)	(0.147)	(-1.530)	(0.837)	(-1.281)	(1.715)
ROA	-0.028	-0.048	0.364***	0.123**	-0.050	0.037***
	(-0.729)	(-1.583)	(5.490)	(2.021)	(-0.617)	(5.317)
Board size	-0.005	-0.013*	-0.002	-0.004	0.023	-0.005***
	(-0.580)	(-1.905)	(-0.151)	(-0.236)	(1.319)	(-3.265)
Independent director ratio	-0.012	0.049**	-0.016	0.013	0.058	0.005
	(-0.538)	(2.247)	(-0.334)	(0.280)	(1.034)	(0.966)

Firm age	0.024	-0.016	0.037	0.096*	0.004	-0.034***
	(0.676)	(-0.466)	(0.507)	(1.676)	(0.048)	(-4.031)
Constant	-0.268**	0.292**	-1.371***	-0.537**	4.298***	0.065*
	(-1.969)	(2.162)	(-4.698)	(-2.123)	(12.920)	(1.717)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.134	0.275	0.364	0.119	0.489	0.462
N	18,524	14,203	16,933	18,524	18,524	18,513

Appendix IA.19. Overtime Effects in Shift-intensive versus Day-shift Industries

This table examines whether the estimated effects of the executive pay-cap regulation are stronger in industries where night-shift production is more plausible. The sample is partitioned into shift-intensive industries and day-shift industries. The dependent variables are listed in the column headings. The coefficient on Treat × Post captures the differential post-policy change for treated firms relative to control firms within each subsample. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. t-statistics are reported in parentheses, with standard errors clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable =	Overtime Ratio	
	shift-intensive industries	day-shift industries
	(1)	(2)
Treat × Post	-0.038***	-0.012*
	(-2.848)	(-1.653)
Size	-0.005	0.009*
	(-0.492)	(1.654)
Leverage	-0.010	-0.040**
	(-0.288)	(-2.023)
Largest shareholder stake	-0.024	-0.032
	(-0.449)	(-0.993)
Cash flow	0.002	-0.000
	(0.050)	(-0.001)
ROA	-0.041	-0.053
	(-0.622)	(-1.531)
Board size	-0.025*	-0.010
	(-1.802)	(-1.250)
Independent director ratio	0.060	0.050**
	(1.315)	(1.978)
Firm age	-0.059	0.006
	(-0.815)	(0.153)
Constant	0.673**	0.158
	(2.307)	(0.997)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes
Adj R^2	0.252	0.279
N	3,536	10,870

Appendix IA.20. Executive pay caps and managerial effort using alternative proxies

This table presents the estimated effects of the executive pay-cap regulation on managerial effort, using alternative proxies for managerial engagement. Columns (1) and (2) use *Ln*(1+*Board Meetings*) and Ln(1+*Investor Interactions*), defined respectively as the natural logarithm of one plus the number of board meetings and the number of investor interaction activities attended by the CEO. All variable definitions are provided in the Appendix. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5% and 10% level, respectively.

Dependent variable =	Ln (1+Board Meetings)	Ln (1+Investor Interactions)
	(1)	(2)
Treat × Post	-0.030**	-0.050***
	(-2.449)	(-4.090)
Size	0.101***	0.065***
	(13.096)	(7.237)
Leverage	0.089***	-0.018
	(3.168)	(-0.582)
Largest shareholder stake	0.041	-0.095
	(0.829)	(-1.637)
Cash flow	-0.189***	-0.058
	(-4.881)	(-1.262)
ROA	0.132***	0.412***
	(2.638)	(6.407)
Board size	0.195***	-0.006
	(15.096)	(-0.406)
Independent director ratio	-0.025	0.088*
	(-0.648)	(1.791)
Firm age	0.065	0.119*
	(1.167)	(1.805)
Constant	-0.576**	-1.595***
	(-2.550)	(-6.002)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes
Adj R^2	0.511	0.377
N	18,825	18,825

Appendix IA.21. Baseline results with alternative model specifications

This table presents robustness checks using alternative estimation models to assess the sensitivity of the baseline results to model specification. Columns (1) and (4) report Poisson regressions with firm fixed effects and province-by-year and industry-by-year fixed effects. Column (2) and (3) reports a logit model with firm, year, and industry fixed effects. Standard errors are clustered at the firm level. t-statistics are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable =	Poisson Ln(1+ Overtime Keywords) (1)	Logit Unrelated Acquisition (0/1) (2)	Logit Investor Interactions (0/1) (3)	Poisson Risk Tone (4)
Treat × Post	-1.246***	-1.068***	-1.013***	-0.015***
	(-3.889)	(-7.554)	(-5.801)	(-4.423)
Size	0.517***	0.141*	0.736***	0.008***
	(2.835)	(1.871)	(7.739)	(4.254)
Leverage	-0.972	0.123	-1.040***	0.010
	(-1.426)	(0.425)	(-3.053)	(1.428)
Largest shareholder stake	-1.281	-0.230	0.135	-0.020
	(-1.207)	(-0.488)	(0.215)	(-1.606)
Cash flow	-0.503	0.438	-0.428	-0.015
	(-0.451)	(1.051)	(-0.801)	(-1.434)
ROA	0.066	0.641	2.990***	-0.009
	(0.052)	(1.256)	(4.618)	(-0.635)
Board size	-0.033	-0.019	0.007	0.005
	(-0.114)	(-0.141)	(0.042)	(1.375)
Independent director ratio	-1.527*	0.100	-0.467	0.011
	(-1.663)	(0.240)	(-0.966)	(1.061)
Firm age	1.363	0.797	-0.655	-0.001
	(1.318)	(1.445)	(-1.034)	(-0.034)
Firm, Year, and Industry FE	-	Yes	Yes	-
Firm, Province × Year FE, and Industry × Year FE	Yes	-	-	Yes
Pseudo R^2	0.542	0.033	0.094	0.007
N	18,825	11,590	8,220	18,825

Appendix IA.22. Robustness tests using Double Machine Learning (DML)

This table reports robustness test results using the DML framework with Lasso as the machine learning method. Columns (1) to (6) correspond to the six main outcome variables used in the baseline regressions. All variables are defined in the Appendix. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. t-statistics are based on heteroscedasticity-robust standard errors clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Investor Interactions (0/1)	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	-0.024***	-0.014***	-0.075***	-0.108***	-0.070***	-0.010***
	(-4.550)	(-2.914)	(-7.316)	(-10.135)	(-5.528)	(-8.900)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	18,825	14,406	16,933	18,825	18,825	18,814

Appendix IA.23. Robustness to excluding industries with high revenue-weighted SOE presence

This table re-estimates the main specifications after excluding industries with high pre-reform SOE presence. Specifically, we compute each industry's SOE share in 2014 using firms' operating revenue as weights, and exclude industries in the top quartile of this distribution. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. t-statistics are reported in parentheses, with standard errors clustered at the firm level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Investor Interactions (0/1)	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	-0.026***	-0.023***	-0.079***	-0.134***	-0.079***	-0.009***
	(-3.116)	(-2.975)	(-4.498)	(-6.431)	(-3.463)	(-4.629)
Size	0.011**	0.008	0.064***	0.023**	0.042***	0.004***
	(2.081)	(1.467)	(5.687)	(2.247)	(2.933)	(2.810)
Leverage	-0.014	-0.039*	-0.098**	0.055	0.076	-0.003
	(-0.694)	(-1.810)	(-2.312)	(1.300)	(1.624)	(-0.642)
Largest shareholder stake	-0.040	-0.006	-0.067	0.057	-0.138	0.026***
	(-1.067)	(-0.185)	(-0.947)	(0.830)	(-1.499)	(2.952)
Cash flow	-0.020	0.003	-0.079	-0.020	-0.107*	0.012**
	(-0.629)	(0.101)	(-1.446)	(-0.360)	(-1.733)	(2.171)
ROA	-0.020	-0.019	0.380***	0.133*	-0.035	0.031***
	(-0.374)	(-0.537)	(4.736)	(1.800)	(-0.394)	(4.006)
Board size	0.007	-0.012	-0.008	0.009	0.030	-0.005***
	(0.611)	(-1.394)	(-0.425)	(0.448)	(1.483)	(-2.771)
Independent director ratio	-0.050*	0.056**	-0.060	-0.025	0.043	-0.000
	(-1.801)	(2.116)	(-1.078)	(-0.449)	(0.610)	(-0.008)
Firm age	-0.010	-0.060	0.081	0.167**	0.084	-0.030***
	(-0.215)	(-1.496)	(0.960)	(2.520)	(0.685)	(-3.236)

Constant	-0.165	0.346**	-1.348***	-0.869***	4.244***	0.038
	(-0.985)	(2.065)	(-3.857)	(-2.945)	(9.459)	(0.912)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.140	0.282	0.363	0.119	0.481	0.495
N	13,365	10,003	12,051	13,365	13,365	13,355

Appendix IA.24. Triple-difference analysis accounting for contemporaneous policies

This table examines whether our baseline estimates are confounded by other contemporaneous SOE reforms, in particular the nationwide anti-corruption campaign and the deleveraging policy implemented around the same period. We augment the baseline specification with triple-difference interactions that allow treatment effects to vary with provincial anti-corruption intensity and firm-level deleveraging exposure. Anti-corruption intensity is proxied by the number of anti-corruption cases in each province in 2014, with data drawn from provincial procuratorate work reports and the China Procuratorial Yearbook, which is manually compiled. Deleveraging exposure is proxied by firms' leverage ratios in 2014. Both measures are standardized (z-scored) before constructing the interaction terms. Accordingly, the coefficient on Treat × Post captures the reform effect at the average level of anti-corruption intensity and deleveraging exposure. All specifications include firm fixed effects as well as province-by-year and industry-by-year fixed effects. T-statistics are computed using heteroscedasticity-robust standard errors clustered at the firm level. Statistical significance is denoted by ***, **, and * at the 1%, 5%, and 10% levels, respectively.

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Investor Interactions (0/1)	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)	(6)
Anti-corruption intensity × Treat × Post	-0.004	-0.004	-0.002	-0.029*	-0.036**	-0.001
	(-0.569)	(-0.710)	(-0.180)	(-1.904)	(-2.510)	(-0.439)
Anti-corruption intensity × Post	0.009	-0.003	-0.065	-0.029	-0.007	0.004
	(0.713)	(-0.169)	(-1.543)	(-0.745)	(-0.299)	(0.886)
Deleveraging exposure × Treat × Post	0.002	0.006	0.001	-0.009	-0.014	0.003*
	(0.307)	(0.996)	(0.087)	(-0.588)	(-0.801)	(1.928)
Deleveraging exposure × Post	0.008*	-0.006	0.001	0.010	-0.009	-0.003**
	(1.787)	(-1.392)	(0.083)	(1.196)	(-0.904)	(-2.119)
Treat × Post	-0.030***	-0.016**	-0.071***	-0.122***	-0.071***	-0.011***
	(-3.787)	(-2.403)	(-4.580)	(-6.720)	(-3.939)	(-5.758)
Size	0.012***	0.005	0.070***	0.021**	0.047***	0.003**
	(2.774)	(1.089)	(7.272)	(2.278)	(4.195)	(2.563)
Leverage	-0.008	-0.041**	-0.079**	0.031	0.050	-0.006
	(-0.500)	(-2.319)	(-2.170)	(0.835)	(1.268)	(-1.290)
Largest shareholder stake	-0.056*	-0.034	-0.021	-0.033	-0.133*	0.029***
	(-1.786)	(-1.256)	(-0.346)	(-0.576)	(-1.908)	(3.649)

Cash flow	-0.005	0.005	-0.061	0.030	-0.072	0.009*
	(-0.191)	(0.206)	(-1.294)	(0.621)	(-1.274)	(1.711)
ROA	-0.026	-0.046	0.365***	0.122*	-0.046	0.039***
	(-0.658)	(-1.479)	(5.401)	(1.958)	(-0.546)	(5.557)
Board size	-0.004	-0.012*	-0.003	-0.005	0.024	-0.005***
	(-0.392)	(-1.736)	(-0.213)	(-0.305)	(1.312)	(-3.450)
Independent director ratio	-0.014	0.047**	-0.020	0.018	0.055	0.006
	(-0.626)	(2.128)	(-0.409)	(0.379)	(0.956)	(1.229)
Firm age	0.035	-0.021	0.037	0.105*	-0.011	-0.034***
	(0.944)	(-0.630)	(0.497)	(1.803)	(-0.121)	(-4.003)
Constant	-0.301**	0.324**	-1.399***	-0.594**	4.390***	0.069*
	(-2.175)	(2.360)	(-4.723)	(-2.321)	(12.938)	(1.804)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.130	0.276	0.365	0.121	0.487	0.466
N	18,463	14,148	16,633	18,463	18,463	18,452

Appendix IA.25. Construction of the composite behavioral index

This table reports the construction of the composite behavioral index (*PC1*). The index is constructed from six standardized behavioral outcomes: *Ln(1 + Overtime Keywords)*, *Overtime Ratio*, *Investor Interactions (0/1)*, *Unrelated Acquisition (0/1)*, *Risk Tone*, and *Investment*. Principal-component weights are estimated using complete-case observations from the 2011–2014 pre-treatment period only. The sign of the index is normalized so that a higher value indicates greater managerial engagement (i.e., less disengagement). The table reports the *PC1* loadings, the eigenvalue of the first principal component, the share of total variance explained by *PC1*, and the sample loss from restricting the analysis to the complete-case intersection sample.

Variable	PC1 loading	Value
Ln (1+ Overtime Keywords)	0.350	
Overtime Ratio	0.110	
Investor Interactions (0/1)	0.644	
Unrelated Acquisition (0/1)	0.498	
Risk Tone	0.393	
Investment	0.221	
PC1 eigenvalue		1.208
Share of variance explained (%)		20.128
Union sample (N)		18,825
Intersection sample (N)		14,396
Sample loss (N)		4429
Sample loss (%)		23.527

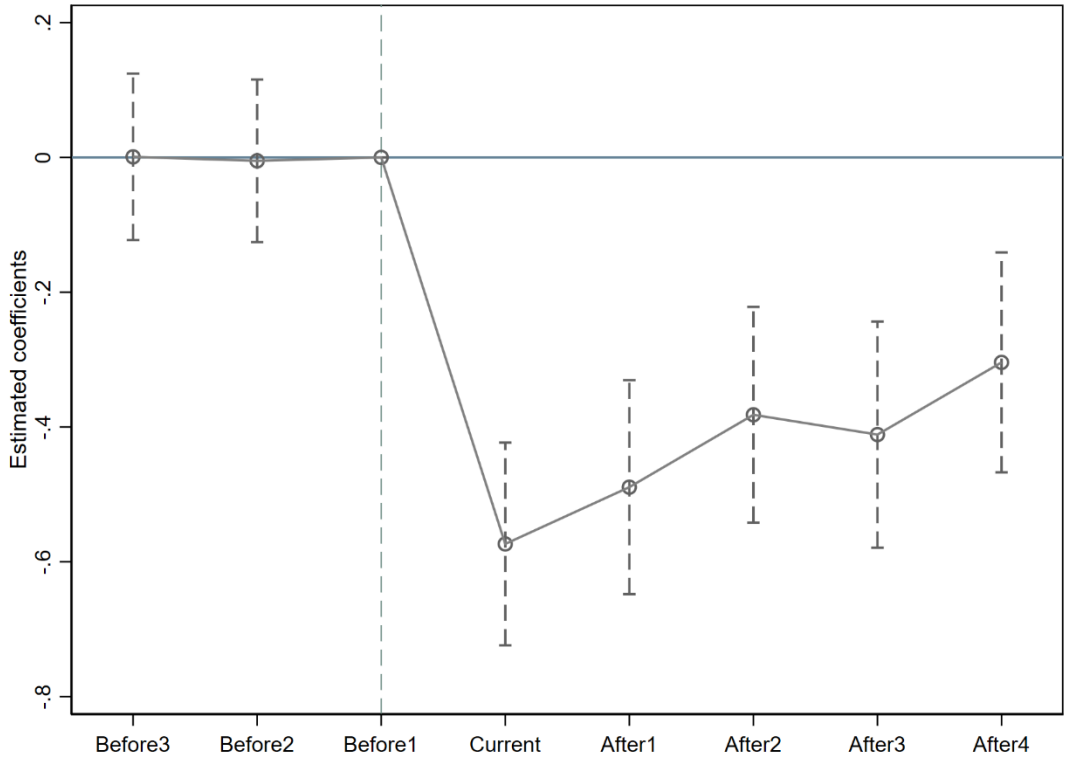
Appendix IA.26. Robustness using the composite behavioral index as the outcome variable

This table reports additional evidence on the composite behavioral index (*PC1*). *PC1* is constructed from six standardized behavioral outcomes using principal-component weights estimated from the 2011–2014 pre-treatment period. The sign of the index is normalized so that a higher value indicates greater managerial engagement (i.e., less disengagement). Panel A reports the baseline DID estimates of the effect of the 2015 executive pay-cap regulation on the composite PC1 index. Panel B plots the dynamic treatment effects of the executive pay-cap regulation on the composite PC1 index. The year 2014 is omitted as the reference year. Pre-2015 coefficients are used to assess the parallel-trends assumption, while post-2015 coefficients trace the evolution of managerial disengagement following the reform. Vertical lines represent 95% confidence intervals. Panel C presents placebo tests for the composite PC1 index. Placebo treatment status is randomly reassigned across firms 1,000 times, and the distribution of placebo estimates for the coefficient on Treat × Post is plotted. The dashed vertical line indicates the true estimate from the baseline regression. Panel D reports IV estimates for the composite PC1 index. The endogenous regressor is *PayCut*, defined as $-\Delta Ln(Compensation)$, so that a larger value indicates a larger compensation cut. The excluded instrument is Treat × pay gap_{2014} × Post, where pay gap_{2014} is the firm's pre-policy executive–worker pay multiple. The first-stage coefficient on the excluded instrument and the corresponding first-stage F-statistic are reported at the bottom of the table. All regressions are estimated on the same sample used in both stages. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. Standard errors are clustered at the firm level. t-statistics are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in the Appendix.

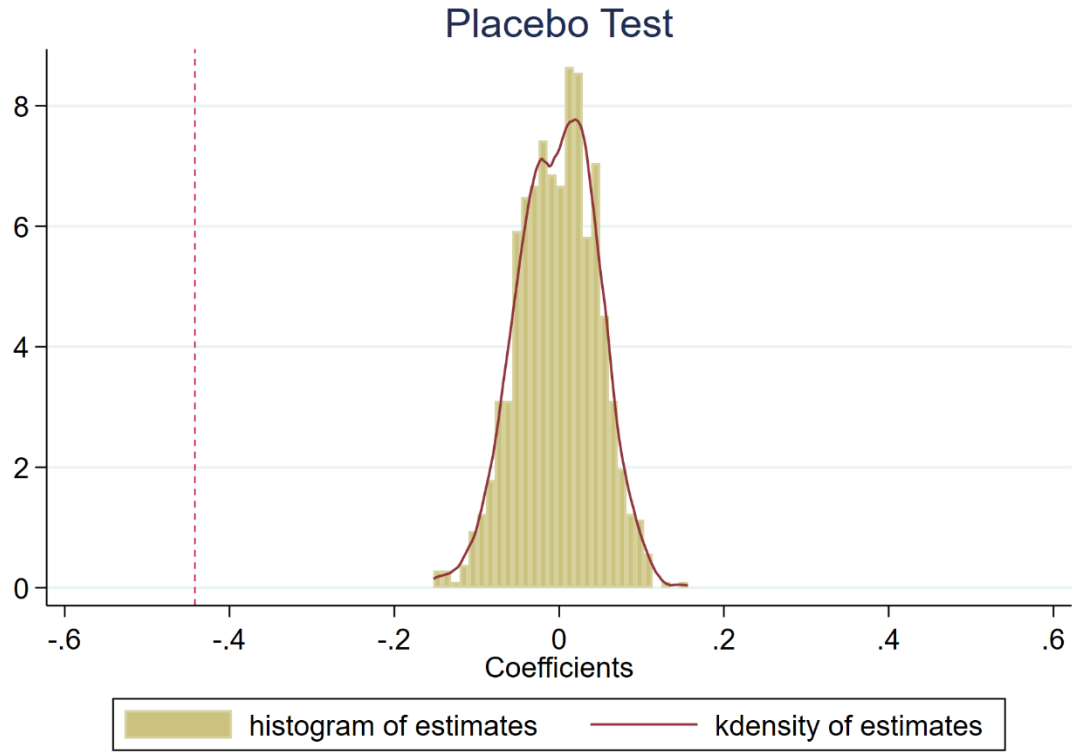
Panel A. Baseline Estimates for the composite behavioral index (*PC1*)

Dependent variable =	Composite behavioral index
	(1)
Treat × Post	-0.442***
	(-7.307)
Size	0.212***
	(6.548)
Leverage	-0.100
	(-0.875)
Largest shareholder stake	-0.062
	(-0.318)
Cash flow	-0.151
	(-0.993)
ROA	0.985***
	(4.737)
Board size	-0.057
	(-1.151)
Independent director ratio	0.141
	(0.941)
Firm age	0.153
	(0.619)
Constant	-4.793***
	(-4.920)
Firm, Province × Year FE, and Industry × Year FE	Yes
Adj R^2	0.315
N	14,396

Panel B. Dynamic effects of executive pay caps on the composite behavioral index (*PC1*)



Panel C. Placebo tests for the composite behavioral index (*PC1*)



Panel D. Two-stage estimation results for the composite behavioral index (*PC1*)

Dependent variable =	PC1
	(1)
$\widehat{PayCut}$	-2.582**
	(-2.430)
Size	0.299***
	(5.053)
Leverage	-0.339
	(-1.479)
Largest shareholder stake	-0.621*
	(-1.898)
Cash flow	-0.439
	(-1.299)
ROA	-1.355
	(-1.252)
Board size	0.516**
	(2.071)
Independent director ratio	-0.647
	(-1.442)
Firm age	0.515
	(1.324)
First-stage results	
Treat × pay gap_{2014} × Post	0.008***
	(4.968)
Firm, Province × Year FE, and Industry × Year FE	Yes
Kleibergen-Paap rk Wald F statistic	24.683
N	12,127

Appendix IA.27. Heterogeneity analysis with the composite behavioral index as the outcome

This table explores heterogeneity in the effect of the executive pay-cap regulation on the composite behavioral index (*PC1*). *PC1* is constructed from six standardized behavioral outcomes using principal-component weights estimated from the 2011–2014 pre-treatment period. The sign of the index is normalized so that a higher value indicates greater managerial engagement (i.e., less disengagement). Panel A investigates the moderating role of CEO promotion incentives, proxied by *CEO age*, under the assumption that older CEOs face fewer career concerns and thus weaker incentives for promotion. "Old CEO" firms are those with a CEO aged above the median in 2014, the year before the policy's implementation. Panel B explores the influence of industry competitiveness, measured using the Herfindahl-Hirschman Index (HHI). Firms in more competitive industries (i.e., those with below-median 2014 HHI values) are assumed to place greater reliance on performance-based incentives. Panel C investigates the role of internal governance quality, proxied by the Internal Control Index (INC). "Weak governance" firms are defined as those with INC values below the 2014 median. Across all panels, control variables align with those specified in Eq. (1) and are detailed in the Appendix. The models include firm fixed effects as well as province-by-year and industry-by-year fixed effects, and t-statistics are computed using heteroscedasticity-robust standard errors clustered at the firm level. Statistical significance is denoted by ***, **, and * at the 1%, 5%, and 10% levels, respectively.

Panel A. The role of CEO promotion incentives

Dependent variable =	Composite behavioral index (PC1)
	(1)
Treat × Post × Old CEO	-0.738***
	(-7.446)
Post × Old CEO	0.021
	(0.441)
Treat × Post	-0.107**
	(-2.080)
Controls	Yes
Firm, Province × Year FE, and Industry × Year FE	Yes
Adj R^2	0.325
N	13,949

Panel B. The role of industry competition

Dependent variable =	Composite behavioral index (PC1)
	(1)
Treat × Post × Competitive industries	-0.801***
	(-7.231)
Post × Competitive industries	0.229
	(1.007)
Treat × Post	0.018
	(0.328)
Controls	Yes
Firm, Province × Year FE, and Industry × Year FE	Yes
Adj R^2	0.322
N	14,193

Panel C. The role of governance quality

Dependent variable =	Composite behavioral index (PC1)
	(1)
Treat × Post × Weak governance	-0.660***
	(-6.487)
Post × Weak governance	-0.050
	(-1.023)
Treat × Post	-0.150***
	(-2.620)
Controls	Yes
Firm, Province × Year FE, and Industry × Year FE	Yes
Adj R^2	0.324
N	13,585

Appendix IA.28. CEO turnover propensity score matching and covariate balance

This table reports the propensity score matching procedure based on the predicted likelihood of CEO turnover. Following the measures in Nanda et al. (2024) and Lin et al. (2022), we estimate propensity scores using probit models over the 2011–2014 sample to predict the likelihood of CEO turnover in 2015. We implement one-to-three nearest-neighbor matching with replacement and impose a caliper of 0.25 to ensure high-quality matches. Panel A reports the propensity score estimation results, while Panel B presents univariate comparisons between the treated and matched control groups. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5% and 10% level, respectively.

Panel A. Probit estimates of ex-ante CEO turnover risk (period 2011-2014)

Dependent variable =	CEO turnover (Nanda et al., 2024)	CEO turnover (Lin et al., 2022)
	(1)	(2)
Size	-0.173***	-0.066**
	(-6.569)	(-2.107)
Leverage	0.198	-0.447**
	(1.293)	(-2.316)
Largest shareholder stake	0.255	0.503**
	(1.476)	(2.521)
Cash flow	0.316	-0.071
	(0.848)	(-0.154)
ROA	-0.780	2.123***
	(-1.562)	(2.732)
Board size	0.861***	0.797***
	(7.548)	(5.805)
Independent director ratio	-0.412	-1.118***
	(-1.193)	(-2.609)
Firm age	0.072	0.105
	(0.973)	(1.145)
CEO age	0.920***	0.841***
	(4.435)	(3.292)
CEO female(0/1)	0.055	0.100
	(0.601)	(0.928)
CEO dual(0/1)	0.103*	0.123*
	(1.905)	(1.924)
LnGDP	0.012	0.013
	(0.159)	(0.148)
LnPopulation	0.022	0.071
	(0.460)	(1.210)
Ln(City employment)	-0.006	-0.040
	(-0.082)	(-0.479)
Fiscal pressure	0.312	-0.089
	(1.431)	(-0.333)

Constant	-4.277***	-6.135***
	(-3.088)	(-3.727)
Industry and Year FE	Yes	Yes
Pseudo R^2	0.065	0.067
N	8,009	7,884

Panel B. Univariate analysis

Variables		Treatment firms	Control firms	Mean-Diff	T value
		Mean	Mean		
$CEO\ turnover(\widehat{Nanda\ et\ al.,2024})_{2015}$	U	0.051	0.048	0.003*	1.7
	M	0.051	0.051	0.000	0.02
$CEO\ turnover\widehat{(Lin\ et\ al.,2022)}_{2015}$	U	0.029	0.025	0.004***	4.8
	M	0.029	0.029	0.000	0

Appendix IA.29. Within-incumbent responses to the executive pay caps

This table examines within-incumbent responses based on a subsample of firms whose CEO remained unchanged between 2014 and 2015. All variables are defined in the Appendix. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. The t-statistics are presented in parentheses, with standard errors clustered at the firm level. ***, **, * indicate significance at 1%, 5%, and 10%, respectively.

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Investor Interactions (0/1)	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	-0.015	-0.012	-0.047**	-0.111***	-0.120***	-0.014***
	(-1.550)	(-1.408)	(-2.335)	(-4.862)	(-5.478)	(-6.138)
Size	0.017**	0.004	0.077***	0.045***	0.030	0.001
	(1.967)	(0.492)	(4.520)	(2.851)	(1.235)	(0.290)
Leverage	0.007	-0.024	-0.040	0.011	0.087	0.003
	(0.299)	(-0.975)	(-0.706)	(0.179)	(1.419)	(0.482)
Largest shareholder stake	0.001	0.009	-0.007	0.065	-0.044	0.030***
	(0.018)	(0.210)	(-0.073)	(0.659)	(-0.457)	(2.688)
Cash flow	0.008	0.009	-0.086	0.046	-0.142	-0.004
	(0.230)	(0.252)	(-1.140)	(0.667)	(-1.428)	(-0.620)
ROA	0.109*	0.005	0.486***	-0.050	0.043	0.037***
	(1.941)	(0.096)	(4.096)	(-0.475)	(0.288)	(3.325)
Board size	-0.017	-0.009	0.014	-0.020	0.042	0.000
	(-1.567)	(-0.816)	(0.547)	(-0.827)	(1.556)	(0.003)
Independent director ratio	-0.017	0.007	-0.105	0.073	-0.044	0.009
	(-0.532)	(0.218)	(-1.405)	(1.095)	(-0.525)	(1.342)
Firm age	0.077*	-0.032	0.122	0.160*	-0.039	-0.037***
	(1.849)	(-0.621)	(1.071)	(1.855)	(-0.321)	(-3.312)

Constant	-0.522**	0.363*	-1.796***	-1.275***	4.780***	0.124**
	(-2.369)	(1.684)	(-3.697)	(-3.083)	(7.901)	(2.214)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.118	0.289	0.407	0.150	0.501	0.506
N	9,834	7,733	9,000	9,834	9,834	9,831

Appendix IA.30. Heterogeneous effects by local ESG mandate intensity

This table examines whether the effects of the executive pay caps on firms' political and social objectives vary with provincial ESG mandate intensity (*ESG mandate*), measured as the proportion of green-related word frequency in prefecture-level city government work reports. Columns (1) and (2) examine firms' ESG and CSR performance, measured by Bloomberg ESG scores and Hexun CSR scores, respectively. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5% and 10% level, respectively.

Dependent variable =	ESG	CSR
	(1)	(2)
Treat × Post × ESG mandate	-3.124**	1.021
	(-1.973)	(1.342)
Post × ESG mandate	0.712	-0.120
	(0.685)	(-0.263)
Treat × Post	2.904***	-0.093
	(2.714)	(-0.195)
Size	1.110***	1.272***
	(4.667)	(13.002)
Leverage	-1.756**	-4.046***
	(-2.058)	(-10.956)
Largest shareholder stake	2.128*	2.836***
	(1.663)	(5.208)
Cash flow	0.113	1.846***
	(0.122)	(3.713)
ROA	2.596**	62.612***
	(2.035)	(60.354)
Board size	-0.155	-0.484***
	(-0.592)	(-3.351)
Independent director ratio	-0.006	0.854**
	(-0.008)	(2.018)
Firm age	0.060	-2.463***
	(0.038)	(-4.167)
Constant	-5.688	-8.431***
	(-0.787)	(-3.089)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes
Adj R^2	0.777	0.808
N	7,384	18,228

Appendix IA.31. Heterogeneous effects by local employment mandate intensity

This table examines whether the effects of the executive pay caps on firms' political and social objectives vary with prefecture-level employment mandate intensity. *Employment mandate* is measured as the frequency of employment-related terms in prefecture-level city government work reports. Column (1) examines *Employment*, measured as the logarithm of the number of employees; Column (2) examines *Labor productivity*, measured as sales in millions divided by the number of employees; and Column (3) examines *Wage-bill intensity*, measured as total salary expenses divided by total assets. All regressions include firm fixed effects as well as province-by-year and industry-by-year fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * denote statistical significance at the 1%, 5% and 10% level, respectively.

Dependent variable =	Employment	Labor productivity	Wage-bill intensity
	(1)	(2)	(3)
Treat × Post × Employment mandate	-0.000	0.001	0.000
	(-0.207)	(0.130)	(0.478)
Post × Employment mandate	0.001	-0.007	0.000
	(0.621)	(-1.104)	(0.018)
Treat × Post	0.085***	-0.238**	-0.004**
	(2.591)	(-2.453)	(-2.150)
Size	0.618***	0.346***	-0.017***
	(26.027)	(4.890)	(-12.235)
Leverage	0.187**	0.487**	0.013***
	(2.423)	(2.400)	(3.289)
Largest shareholder stake	0.055	0.404	0.013**
	(0.396)	(0.922)	(2.010)
Cash flow	0.243***	0.602**	0.022***
	(3.728)	(1.995)	(7.084)
ROA	0.031	1.874***	-0.015**
	(0.296)	(5.986)	(-2.466)
Board size	-0.008	0.101	0.001
	(-0.395)	(1.587)	(0.483)
Independent director ratio	-0.010	0.087	0.002
	(-0.168)	(0.450)	(0.645)
Firm age	0.336***	-0.070	0.011*
	(3.124)	(-0.187)	(1.905)
Constant	-7.107***	-6.448***	0.397***
	(-11.922)	(-3.611)	(11.521)
Firm, Province × Year FE, and Industry × Year FE	Yes	Yes	Yes
Adj R^2	0.930	0.773	0.854
N	17,635	17,635	17,236