The wages of austerity: Executive pay caps and the quiet life

Lihan Chen
Shanghai University of Finance and Economics

Shaojie Lai
Jiangxi University of Finance and Economics

P. Raghavendra Rau
University of Cambridge

Qing Sophie Wang
University of Canterbury

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Abstract

Executive pay caps are increasingly popular worldwide, but their real effects remain unclear. Using China's 2015 binding pay ceiling for state-owned enterprises, which sharply reduced executive compensation relative to non-SOEs, we document systematic managerial disengagement. Treated firms show lower work effort, reduced investment, fewer risky acquisitions, and declines in Tobin's Q. Rather than increasing perks or tunneling, executives retreat into a "quiet life", redirecting limited effort toward political objectives while accumulating excess cash. Effects are strongest for older CEOs and competitive industries where monetary incentives matter most. High-performer turnover rises, suggesting that adverse selection compounds incentive problems. Our findings show that when cash incentives collapse, relational motivations cannot maintain managerial engagement, providing evidence directly relevant to bonus caps and pay-ratio proposals under consideration in the EU, UK, and the United States.

Keywords: Executive compensation, Pay regulation, Managerial incentives, Corporate investment, Firm performance, Difference-in-differences

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* Chen: School of Finance, Shanghai University of Finance and Economics, China, E: lihanchen1999@163.com; Lai: School of Finance, Jiangxi University of Finance and Economics, China, E: shaojielai@jxufe.edu.cn; Rau: Cambridge Judge Business School, University of Cambridge, Trumpington Street, Cambridge, CB2 1AG, UK. E: r.rau@jbs.cam.ac.uk; Wang, Department of Economics and Finance, University of Canterbury, New Zealand, E: sophie.wang@canterbury.ac.nz.

1. Introduction

Should governments cap executive pay? Policymakers across developed economies clearly think so. The European Union capped bankers' bonuses at 100% of base salary in 2014.[1] In the United States, legislative proposals would impose punitive taxes on firms whose CEO-to-median-worker pay ratio exceeds 50:1[2], while states like Oregon and California contemplate their own ratio-based surtaxes.[3] These interventions share a common premise: that excessive executive compensation undermines fairness and social cohesion (Murphy, 2013; Cebon and Hermalin, 2015; Murphy and Jensen, 2018; Chaigneau et al., 2025) and that government action can restore fairness without significant economic costs.

However, the economic consequences of binding pay limits remain poorly understood. Advocates argue that capping executive compensation reduces inequality and may even improve performance by eliminating distortive incentives for short-term risk-taking. Critics counter that restricting performance-based pay dulls managerial effort and drives talent away from regulated firms. This debate matters enormously — executive compensation structures shape corporate investment, innovation, and ultimately economic growth. Unfortunately, the empirical evidence on what actually happens inside firms when governments regulate pay remains frustratingly thin.

The challenge is that most Western pay regulations are not actually binding. The U.S. experience with the 1993 Omnibus Budget Reconciliation Act's (OBRA) $1 million deductibility cap simply shifted compensation toward stock options (Rose and Wolfram, 2000, 2002). The EU's banker bonus cap led to higher base salaries and deferred compensation in Austria rather than lower total pay (Bornemann et al., 2023). Say-on-pay votes rarely fail, and when they do, boards rapidly adjust the structure rather than the level of compensation (Ferri

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

[2] 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).

[3] 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).

and Maber, 2013). Even seemingly strict interventions leave multiple escape routes, such as equity grants, perks, or pensions, that preserve incentives while circumventing headline restrictions. To understand what happens when monetary incentives truly collapse, we need a shock that is sudden, binding, and leaves few substitutes.

China's 2015 executive pay ceiling for state-owned enterprises provides exactly such a laboratory. The policy, implemented as part of President Xi Jinping's anti-corruption and austerity campaign, capped SOE executive compensation at fixed multiples of average employee wages. Evidence in Zhang et al. (2025) shows that, for central SOEs, average top executive pay fell from about 12 times to 7–8 times average employee wages, with the largest pay cuts of 40–70% concentrated in the major financial SOEs. Unlike Western reforms, this intervention was immediate, credible, and comprehensive. Equity compensation remained negligible in Chinese SOEs, perks were simultaneously restricted by anti-corruption enforcement, and the state's dual role as regulator and controlling shareholder ensured compliance. The reform thus created an exceptionally clean quasi-natural experiment to observe how managers respond when performance-based monetary rewards essentially disappear.

Why should evidence from Chinese SOEs inform debates in developed economies? Two factors make this setting broadly relevant. First, the mechanism we study — how managers respond when monetary performance pay collapses while career concerns, reputational rewards, and corporate oversight remain — exists in all corporations, not just SOEs. Even in high-governance U.S. firms, executives derive substantial utility from status, internal prestige, and future board appointments. The sign of the real effects is not predetermined. Lower pay could curb rent extraction and empire building. Strong career concerns could sustain effort. Political and social objectives could redirect managerial time toward visible employment rather than operational efficiency. The reform therefore offers a clean test of how managers reallocate effort and how corporate policies adjust when performance pay collapses. Second, regulated utilities, systemically important banks, and other quasi-public firms in developed economies share key features with SOEs, including political oversight and mixed objectives. Our estimates provide an upper bound on the likely direction and order of magnitude of policy-induced

incentive erosion in those contexts. The results inform ongoing debates about banker bonus caps and public-sector pay guidelines by quantifying the trade-off between inequality and productive effort when compensation becomes low powered.

Using a difference-in-differences design comparing SOEs to private firms around the 2015 reform, we document striking changes in managerial behavior and firm performance. In particular, we find evidence of a distinct and novel response: executives do not actively substitute perks or pursue empire-building, but rather systematically disengage, opting for a managerial 'quiet life' characterized by passive withdrawal from productive effort. This disengagement occurs even in the presence of meaningful reputational and relational incentives, challenging the assumption that non-pecuniary motivations alone can sustain managerial productivity.

Specifically, we find that treated firms experienced (i) reduction in work effort, measured by overtime at the firm reported in media mentions (Yang et al., 2025) and a 5.4% less work overtime by satellite-detected night-time corporate activity; (ii) a 76.2% decline in unrelated acquisitions (Bernile et al., 2017; Rau et al., 2025) and significantly more conservative risk disclosures (Ru et al., 2025; Gao et al., 2023); (iii) a 21.7% drop in capital investment, with particularly sharp contractions in R&D and M&A activity; (iv) slower adjustment toward optimal cash levels, with excess cash accumulating rather than being deployed, and a 26-cent lower value to each additional dollar of cash in treated firms post-reform; and (v) a 16.2% decline in Tobin's Q and measurable drops in total factor productivity. We also examine managerial engagement beyond core operational decisions, specifically focusing on interactions with boards and investors. Investor relations require voluntary, effort-intensive participation and are vulnerable to erosion when managerial motivation or perceived personal benefit declines. Following the policy, managers become 18.8% less likely to participate in CEO-investor interactions such as investor site visits or Q&A sessions.

These effects are not uniform. The behavioral changes are most pronounced among older CEOs with limited promotion prospects, in competitive industries where performance pressure is highest, and in firms with weak internal governance. Importantly, we find that perks and tunneling, the usual suspects when executive pay is restricted, actually decline rather than

increase, likely due to concurrent anti-corruption enforcement. Instead, executives redirect effort toward politically palatable but economically marginal activities: ESG initiatives, employment preservation, and other social objectives that generate "warm glow" utility without requiring intensive managerial effort.

Mechanism analysis suggests that CEO turnover increases by 35.8%, with high-performing executives particularly likely to exit, suggesting adverse selection alongside the incentive effects. Pay-performance sensitivity, already weak in SOEs, declines further, confirming that the policy prioritized equity over efficiency in compensation design.

Our findings contribute to several strands of literature. First, we provide new evidence on the effort margin of pay regulation, complementing studies of OBRA-1993 and EU bonus caps that focus primarily on risk-shifting or compensation structure rather than real operational outcomes. While Rose and Wolfram (2000, 2002) document how firms circumvented the $1 million cap through options, and recent work on EU banker bonuses emphasizes portfolio risk adjustments, we show what happens when such safety valves are blocked: managers do not work harder for less pay; they simply work less.

Second, we demonstrate that relational incentives such as career concerns, reputational capital, and internal prestige cannot fully substitute for monetary rewards, at least not at the margin relevant for policy. This insight matters for current proposals to implement ratio-based taxes or binding pay caps, which implicitly assume that non-monetary motivations will maintain effort. Our evidence suggests otherwise: when cash incentives collapse, even executives with strong career concerns reduce discretionary effort, and those effects compound in competitive industries where effort matters most.

Third, we contribute to the broader debate on optimal executive compensation by documenting an under-appreciated cost of pay compression: the erosion of managerial engagement even absent traditional agency problems. In this sense, our findings inform classic debates in corporate governance regarding managerial incentives (Jensen and Meckling, 1976) and the conditions under which executives may prefer a 'quiet life' to aggressive value creation (Bertrand and Mullainathan, 2003). Our findings complement and extend recent work on the consequences of pay regulation (e.g., Abudy et al., 2020; Bae et al., 2024; Balogh et al., 2024;

Nanda et al., 2024) by isolating a mechanism centered on incentive erosion and passive behavior, rather than active rent-seeking or perquisite substitution. Unlike typical agency problems involving active resource diversion, our results reveal a subtler form of managerial disengagement triggered solely by the removal of monetary incentives. This passive disengagement may be harder to detect than outright malfeasance, but it appears no less costly in terms of foregone investment, slower innovation, and reduced productivity.

Our results have several implications for Western policymakers. They suggest that binding pay caps, even if successful at reducing inequality and preventing some forms of rent extraction, may inadvertently trigger a broader retreat from value-creating activities. The 15-20% performance declines we document likely represent an upper bound given China's extreme intervention, but even proportionally smaller effects could meaningfully impact economic dynamism. Moreover, the adverse selection we observe, with high-performers disproportionately exiting, suggests that pay caps may particularly damage firms most in need of strong leadership.

We note that we do not imply that executive pay regulation is necessarily welfare-reducing. The social costs of inequality and the political economy benefits of perceived fairness may outweigh the efficiency losses we document. But our evidence suggests these trade-offs are real and quantitatively significant. Policymakers contemplating pay ratios, bonus caps, or punitive taxation should recognize that such interventions may purchase equity at the price of corporate dynamism, a deal their constituents may or may not wish to make.

2. Institutional Background and Conceptual Framework

2.1 Executive Pay Regulation in China

Governments around the world have repeatedly intervened in executive compensation, often in response to rising inequality or perceived excess. In developed markets, such interventions have included tax-based measures, disclosure mandates, and shareholder voting rules. For example, the 1993 OBRA limited the tax deductibility of executive pay unless linked to performance, prompting a shift toward stock options and bonus-based compensation. Austria

and Canada have taken similar steps, using tax policy to limit executive pay.[4] While these policies vary in scope and enforcement, they share a common motivation: to realign incentives and address public concern over inequality.

China's approach to executive compensation followed a different path. Prior to the 1990s, SOE pay was strictly controlled, with compensation largely tied to administrative rank rather than performance. This changed in the early 2000s with the establishment of the State-owned Assets Supervision and Administration Commission (SASAC), which introduced a more market-oriented framework. Firms gained the flexibility to reward executives based on firm size, profitability, and other performance metrics. However, as SOEs gained commercial autonomy, executive pay levels began to diverge sharply from those of rank-and-file employees. This divergence, particularly after the 2008 financial crisis, generated public criticism and triggered a wave of internal and external scrutiny. Earlier reforms, such as the 2009 guidelines, aimed to expand the definition of compensation and cap bonuses, but enforcement was weak, and executives often responded by shifting to perks, expense-based benefits, and off-balance-sheet forms of pay (Bae et al., 2024). The proliferation of workaround pay practices highlighted the limits of regulatory enforcement at the time and contributed to mounting political pressure, culminating in the 2013 anti-corruption campaign that targeted illicit compensation practices and tightened oversight of SOE executives (Griffin et al., 2022).

The 2015 executive pay ceiling marked a decisive reassertion of centralized control.[5] Zhang et al (2025) note that, positioned as part of a broader austerity and anti-corruption campaign, the policy capped executive pay at fixed multiples of average employee wages and sharply curtailed performance-linked bonuses. They document that the executive-to-employee pay multiple fell from roughly 12 times to 7–8 times employee pay, with the largest nominal cuts at the big state banks. While formally targeted at centrally administered state-owned enterprises (central SOEs), the regulation was widely applied across the broader state sector. [6]

[4] Rose and Wolfram (2000, 2002), Bird (2018), and Bornemann et al. (2023) examine the effects of pay regulation in the U.S., Canada, and Austria, respectively, documenting how formal constraints on executive compensation often led firms to adjust the structure rather than the level of pay, typically by increasing reliance on stock options or performance-based awards.

[5] The executive compensation reform for central SOEs was first approved by the Political Bureau of the CPC Central Committee on September 2, 2014 and formally implemented on January 1, 2015.

[6] Specifically, 72 central SOEs began implementing salary reductions and publishing executive salary information. Following the reform, the total income of these central SOE executives was capped at eight times the average salary of their employees, and their compensation was required to be publicly disclosed (see 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

Unlike pay reforms in market economies, which typically preserve performance-based channels or allow deferred compensation, the Chinese regulation significantly reduced total compensation with few viable substitutes. Moreover, because the state acts as both regulator and controlling shareholder, the policy was implemented with high credibility and limited room for managerial resistance. Descriptive trends suggest that pay compression in SOEs began prior to the policy and leveled off shortly afterward (see Figure 1), underscoring the need for a causal identification strategy to assess the policy's impact on managerial behavior and firm outcomes. Figure 1 also confirms the regulation's effectiveness in reducing executive compensation in SOEs relative to non-SOEs and in compressing internal pay disparities. Our design compares SOEs to non-SOEs and examines finance outcomes such as work effort and engagement, risk-taking, investment and its efficiency, cash policy and the value of cash, and firm valuation and productivity across both central and local SOEs, while related management work focuses on within-central-SOE outcomes using variation in cut size (e.g., Zhang et al., 2025).

Together, the policy's political impetus, credible enforcement, and limited scope for incentive substitution make China's 2015 executive pay ceiling a compelling setting for examining how binding compensation constraints operate in weak-contracting environments.

2.2 Incentives in Weak-Contracting Settings

In SOEs, managerial incentives are shaped by a fundamentally different contracting environment than in privately held firms. Formal pay-for-performance contracts are weak or absent, shareholder discipline is minimal, and boards often lack independence or monitoring capacity (Firth et al., 2015). As a result, executive effort is frequently sustained through relational mechanisms, such as the prospect of political promotion, internal prestige, or career advancement within the state apparatus. These soft incentives play a central role in motivating behavior in environments where monetary rewards are constrained and traditional corporate governance tools are ineffective (Jensen and Meckling, 1976). In the Chinese context, prior research has shown that political advancement is often linked to firm performance, making

releasing reform plans that restrict the compensation of executives in these firms. In most of these plans, the annual base salary of top executives was capped at no more than twice the average income of the company's employees (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). In addition, in accordance with established administrative practices, local authorities often tend to impose additional layers of restrictions, resulting in more stringent pay-control requirements. Nevertheless, it remains difficult to systematically collect and compare the specific stipulations across regions.

career concerns a de facto substitute for high-powered financial incentives (Cao et al., 2019). At the same time, the lack of market-based monitoring and diffuse accountability heightens the risk of disengagement when these relational levers are disrupted.

When formal compensation is further restricted by binding regulatory caps, the risk of disengagement is amplified. If executives no longer expect meaningful rewards, either through financial channels or career mobility, they may choose to exert minimal effort, avoid risk, and prioritize personal comfort. This behavioral response is consistent with the 'quiet life' hypothesis, which predicts that managers in low-powered environments shift toward passive oversight, underinvestment, and conservatism (Hicks, 1935; Rhoades and Rutz, 1982; Bertrand and Mullainathan, 2003). Importantly, this is not a rent-extraction story: disengagement arises not from a desire to divert resources but from a lack of incentive to deploy them. Under this mechanism, excess cash holdings and other proxies used in the literature for shareholder expropriation may emerge not as a tool for personal gain but as a byproduct of managerial passivity.

Nevertheless, alternative mechanisms may also explain firm responses to binding pay limits. The managerial power hypothesis suggests that executives may retain effort but redirect it toward private benefits, such as perk consumption, tunneling, or empire-building, particularly in weakly monitored firms (Adams et al., 2005; Bebchuk et al., 2011). Conversely, reputational governance or board monitoring may help sustain executive effort even under tighter pay restrictions (Ferri and Maber, 2013). Stronger internal controls or equity ownership can also align managerial behavior with organizational goals. A third possibility is selection: binding pay caps may prompt higher-ability executives to exit for better-paying private sector roles, leaving behind managers who are more willing to accept limited incentives and less inclined to exert discretionary effort. Each of these channels yields overlapping but distinct predictions about post-policy behavior, investment choices, and managerial turnover.

While non-monetary incentives such as political prestige or promotion opportunities can substitute for direct pay in some cases, their motivational power depends on credibility, salience, and a perceived connection to performance. For many SOE executives, particularly those nearing retirement, the prospect of career advancement may be limited, weakening the

effectiveness of these relational incentives. Moreover, a binding pay ceiling may signal a broader shift in the state's priorities away from rewarding performance, further diminishing the motivational force of both monetary and non-monetary rewards. If these soft incentives are no longer seen as credible or attainable, executives may rationally disengage. We therefore expect the quiet life response to be concentrated among CEOs with limited career horizons and in firms lacking internal accountability mechanisms.

Our empirical strategy is designed to distinguish between these competing explanations. We examine not only the average treatment effect of the 2015 pay limit on SOEs but also whether changes in behavior are concentrated among managers who remain in office post-reform, whether changes in managerial behavior reflect passivity or rent-seeking, and how the response varies with promotion incentives, governance strength, and industry competitiveness. This framework allows us to isolate the erosion of relational incentives from broader agency problems and sheds light on the conditions under which binding compensation regulation is most likely to induce disengagement and shape firm-level outcomes in weak-contracting environments.

3. Data, Sample, and Methodology

3.1 Data Sources

Our analysis uses data on all firms listed on the Shanghai and Shenzhen Stock Exchanges between 2011 and 2019, covering a four-year period before and after the 2015 executive pay limit regulation. The primary data source is the China Securities Markets and Accounting Research (CSMAR) database and the Chinese Research Data Services Platform (CNRDS), which provides comprehensive firm-level financial and governance information. We supplement this with additional data sources: one of our measures of overtime work is derived from NASA's VNP46A2 nighttime light dataset, and our risk tone variable is constructed through text analysis of corporate annual reports. Our sample comprises all non-financial A-share firms, excluding those with special treatment (ST) or particular transfer listings. We also remove firms that experienced a change in controlling ownership (private to SOE or SOE to private) during the sample period. To ensure the availability of pre-policy characteristics, we exclude firms listed after 2014 or those without observations beyond 2015. All continuous

variables are winsorized at the 1st and 99th percentiles to reduce the influence of outliers. Our final sample consists of 18,825 firm-year observations between 2011 and 2019, with a smaller sample size for overtime-related variables due to data limitations.

3.2 Key Variables and Statistics

Treatment and control groups

The policy intervention of interest, the 2015 SOE executive pay limits, provides a quasi-experimental setting to examine the causal effect of constrained executive compensation on corporate behavior. The treatment indicator, *Treat*, is a dummy variable equal to one for firms identified as SOEs in 2014 (the year before the policy implementation), and zero otherwise. We define SOEs based on ultimate control by central or local government entities. Table 1 reports the summary statistics. SOEs comprise 37.4% of our sample observations. We also test the robustness of our results using separate indicators for central and local SOEs.

Primary behavioral outcomes

We examine how pay constraints affect three core dimensions of managerial behavior: work effort, risk-taking, and investment decisions.

We develop two distinct measures for managerial work effort: (1) media-based overtime exposure and (2) satellite imagery-based overtime activity. Given the widespread practice of overtime in Chinese firms and the frequent coverage of overtime in media reports, we exploit this context to develop a textual measure of overtime. Specifically, we calculate the firm's overtime exposure ($Ln(1 + Overtime\ Keywords)$) as the natural logarithm of one plus the number of media articles mentioning overtime-related keywords in connection with that firm, following Yang et al. (2025). [7] In our sample, 2.9% of firms are identified as engaging in overtime by social media sources, closely aligning with the 3.5% reported in Yang et al. (2025).

To complement this measure, we utilize an independent proxy based on nighttime light intensity derived from NASA's VNP46A2 dataset, which provides daily surface brightness at high spatial resolution. Firms are mapped to their corresponding geographic grids. We identify overtime activity using satellite data by checking two conditions: whether a firm's location is brighter than surrounding areas (suggesting unusual activity), and whether this brightness

[7] The list of keywords is obtained from Yang et al. (2025). We thank Jingjing Yang for providing us with the overtime data.

exceeds the firm's median levels during holidays when facilities should be dark. This yields our main satellite-based measure, the *Overtime Ratio*, which reflects the proportion of days in a year characterized by elevated nighttime activity.[8] The satellite data are preprocessed to address issues such as outliers, cloud contamination, and misaligned grid data, consistent with established best practices. On average, the *Overtime Ratio* is 0.331, indicating that overtime activity occurs on approximately 33.1% of the non-legal holidays annually. These measures capture whether executives reduce their work intensity when monetary incentives disappear.

We also evaluate direct managerial engagement using two complementary indicators: the natural logarithm of one plus the number of board meetings (*Ln*(1+*Board Meetings*)) and the natural logarithm of one plus the number of CEO–investor interactions (*Ln*(1+*Investor Interactions*)) (e.g., site visits and investor inquiries). A higher frequency of board meetings typically indicates more intensive strategic deliberation and greater involvement from both the CEO and the board in addressing firm-specific challenges. This reflects a hands-on governance style and an active commitment to shaping the firm's direction. Likewise, CEO engagement with investors, via online channels or in-person visits, demonstrates a strong emphasis on transparent communication and effective management of external stakeholder relationships. The mean of *Ln*(1+*Board Meetings*) is 2.333, suggesting that, on average, firms hold about 8 to 9 board meetings per year, which aligns with typical corporate governance practices. The mean of *Ln*(1+*Investor Interactions*) is 0.175, which is quite low, suggesting that most CEOs have a small number of recorded investor research activities.

Similarly, we employ two proxies to measure corporate risk-taking behavior. The first is an unrelated acquisition, defined as acquisitions involving targets outside the firm's primary industry. Such transactions are inherently riskier due to limited operational synergies and the acquirer's potential unfamiliarity with the target's domain (Bernile et al., 2017; Rau et al., 2025). To capture this, we construct a binary indicator, *Unrelated Acquisition* (0/1), which equals one if a firm engages in an unrelated acquisition, and zero otherwise. This measure serves as a direct signal of operational risk-taking (Rau et al., 2025; Kravet and Muslu, 2013;

[8] The satellite nightlight data is only available after 2012. In addition, we removed firms located within the same grid. Therefore, there are fewer observations for this variable in our empirical analysis. 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.

John et al., 2008).

Our second measure, *Risk Tone*, quantifies the risk-related language in a firm's annual reports. Following Ru et al. (2025) and Gao et al. (2023), we compute *Risk Tone* as the number of risk-related keywords in the Management Discussion & Analysis (MD&A) section, scaled by the total word count and multiplied by 100. This textual metric reflects the firm's self-reported risk outlook. Descriptive statistics indicate that 14.7% of firms in our sample undertake unrelated acquisitions, consistent with prior findings on conglomerate merger frequency. The average *Risk Tone* is 5.430, corresponding to approximately 227 risk-related keywords per annual report. Together, these capture whether executives become more conservative in both actions and communications when pay is capped.

Investment efficiency and Capital allocation

To understand the quality of investment decisions under constrained incentives, we examine both capital allocation patterns and investment efficiency.

First, we measure the firm's capital investment (*Investment*) as the net cash outflow for long-term assets, calculated by subtracting cash received from the disposal of fixed assets, intangible assets, and other long-term assets from the cash paid for acquiring them, and scaling the result by total assets. The average value of *Investment* is 0.046, indicating that firms, on average, invest 4.6% of their total assets in long-term assets after accounting for asset disposals. The standard deviation of 0.047 suggests considerable variation in investment intensity across firms.

We next employ four distinct binary measures to capture corporate capital allocation decisions: (1) Share repurchase (*Repurchase* (0/1)), a dummy variable equal to one if a firm repurchased its own shares from the market in a given year, and zero otherwise; (2) R&D investment (*R&D* (0/1)), a dummy variable equal to one if a firm reported R&D expenditure in that year, and zero otherwise; (3) Merger and Acquisitions (*M&A* (0/1)), a dummy variable equal to one if a firm engaged in any M&A activity during the year, and zero otherwise; (4) Cash Dividends (*Dividend* (0/1)), a dummy variable equal to one if a firm distributed cash dividends in that year, and zero otherwise.

We also adopt two widely used measures of investment efficiency from the literature. The

first is investment-Q sensitivity, which reflects the extent to which firms align their capital investment with market valuation signals (Jayaraman and Wu, 2019; Hou et al., 2024). A higher sensitivity suggests more efficient capital allocation toward value-enhancing opportunities. The second measure is investment inefficiency based on Richardson (2006),[9] which evaluates the extent to which firms misallocate resources by comparing actual investment to predicted optimal levels based on firm fundamentals. Higher values indicate greater deviation from efficient investment. In our sample, the average investment inefficiency is 0.022, with a standard deviation of 0.023, which is slightly lower than the 0.046 reported in Wu et al. (2022), suggesting comparatively more efficient investment behavior for our sample.

Cash Management and Performance Outcomes

Corporate cash holdings are measured as the ratio of cash and cash equivalents to total assets (*Cash*), following Dessaint and Matray (2017). This serves as an indicator of firms' liquidity management and potential precautionary behavior in response to incentive constraints. Firms hold an average of 23.7% of total assets in cash and cash equivalents, with a median of 29.5% and a standard deviation of 14.0%, highlighting significant variation in cash management.

Firm performance is measured using two indicators: *Tobin's Q*, which serves as a proxy for firm growth and market valuation, and total factor productivity (*TFP*) using the Levinsohn and Petrin (2003) method, which captures the firm's underlying operational efficiency.

Mechanism variables

To understand the channels through which pay constraints affect firm outcomes, we examine CEO turnover, rent-seeking behavior, and social performance.

In analyzing CEO turnover, we adopt the definitions employed by Nanda et al. (2024) and Lin et al. (2022). First, following Nanda et al. (2024), we classify a turnover as voluntary if the CEO departs due to resignation, term expiration, or personal reasons (including those labeled as "others") according to the CSMAR classification.[10] We further manually exclude CEOs

[9] 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 terms of residual ε_{it} from the regression is the investment inefficiency measure. Positive (negative) ε_{it} denotes overinvestment (underinvestment).

[10] Distinguishing between voluntary and involuntary turnovers can be challenging, especially in China, where press reports rarely specify the nature of a departure. To overcome this, we rely on CSMAR classifications to categorize turnovers, consistent

moving to an affiliated corporate group as voluntary turnovers. In our sample, voluntary CEO turnover accounts for 5.3%.[11] Second, consistent with Lin et al. (2022), we construct an alternative CEO turnover measure: a dummy variable equal to one if a CEO was replaced despite good firm performance in the prior year. Good performance is defined as an ROA above the industry-adjusted median. This measure has a mean of 3.6% in our sample.

To assess managerial self-serving or opportunistic behavior, we employ two common measures of rent extraction. Our first proxy is the ratio of total perks to sales (Gul et al., 2011). For tunneling, we adopt net other receivables as a proxy, consistent with Jiang et al. (2010). The perk ratio averages 0.008, suggesting that firms spend approximately $8 on executive perquisites (such as travel, entertainment, and company vehicles) for every $1,000 in sales revenue. The logarithm of net other receivables (*Tunneling*) is 18.148, indicating substantial potential tunneling activities by controlling parties.

Finally, to capture firms' social performance in the post-reform period, we employ two measures: (1) ESG ratings from Bloomberg (ESG) and (2) CSR scores from Hexun.com (CSR). We further examine labor-related outcomes potentially affected by executive pay limits, namely: (1) job creation, measured as the natural logarithm of the number of employees (*Employment*) (Zhang et al., 2025); (2) operating efficiency, measured by total sales (in millions) divided by the number of employees (*Labor productivity*); and (3) employee pay, measured by total salary expenses relative to total assets (*Wage bill intensity*).

In our sample, the average ESG score is 20.419 with a standard deviation of 6.829, suggesting a moderate degree of variation across firms, with most scores concentrated between approximately 16 and 23. CSR scores have a mean of 13.556 and a standard deviation of 6.500, with most values falling in the range of 9.580 to 18.260, indicating a comparable level of dispersion. *Employment* has a mean of 7.705 (equivalent to roughly 6,432 employees) and a standard deviation of 1.326, reflecting differences in firm size that are present but not extreme. The mean of *Labor productivity* is 1.610, while *Wage bill intensity* averages 0.064.

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with existing literature (e.g., Kato and Long, 2006; Nanda et al., 2024). We classify a turnover as voluntary if the CEO departs due to resignation, term expiration, or personal reasons according to the CSMAR classification, excluding moves to affiliated corporate groups which often represent promotions rather than true departures.

[11] Our first CEO turnover measure yields 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.

Control variables

We include firm-, CEO-, and macro-level characteristics in our analysis. Firm-level characteristics include company size (*Size*), long-term debt ratio (*Leverage*), the proportion of shares held by the largest shareholders, operating cash flows (*Cash flow*), return on assets (*ROA*), number of directors on board (*Board size*), the percentage of independent directors on board (*Independent director ratio*), and firm age. CEO-level characteristics consist of CEO age, gender (*CEO female* (0/1)), and whether the CEO is also the chairman (*CEO dual* (0/1)). Macro-level characteristics are local GDP (*Ln GDP*), regional employment level (*Ln Employment*), population (*Ln Population*), and local economic pressure (*Fiscal pressure*). Our firm characteristics are consistent with existing literature (Xu et al., 2016; Chang et al., 2021), showing an average ROA of 3.3%, leverage of 43.3%, and the largest shareholder owning an average of 35.1% of the firm. The average CEO age is 49, and only 6.5% of CEOs are female. Approximately 25.8% of CEOs also serve as chairmen of the same firm.

3.3 Methodology

To estimate the causal effect of the 2015 executive pay limit regulation on corporate behavior and outcomes, we implement a DID design that compares changes in managerial behavior and firm outcomes for SOEs relative to matched non-SOEs before and after the policy. Our baseline specification is:

$$Y = \alpha + \beta(Treat_i \times Post_t) + \gamma' Z_{it} + \delta_i + \phi_t + \varepsilon_{it} \qquad (1)$$

where Y denotes corporate behavior and outcomes such as work effort, risk-taking, investment, managerial engagement, cash holdings, firm performance, rent seeking, CEO turnover, pay-performance sensitivity, and social performance for company i in year t. $Treat_i$ is a treatment indicator equal to one for SOEs in 2014, the year prior to the policy implementation. $Post_t$ is an indicator for the post-policy period (2015 onward). The coefficient of interest, β, captures the differential change in corporate behavior and performance for treated SOEs relative to matched non-SOEs after the policy. Z_{it} includes firm-level controls. δ_i and ϕ_t denote firm and year fixed effects, respectively. Standard errors are clustered at the firm level.

4. Executive pay limits and their impact on corporate behavior and outcomes

4.1 Managerial effort and engagement

When executive compensation is subject to binding caps, the marginal return to additional managerial effort declines. To test whether this leads to reduced work intensity, we examine changes in corporate overtime — an observable proxy for work effort — among treated firms.

As presented in Columns (1) and (2) of Table 2, the coefficient for *Treat* × *Post* is consistently negative and statistically significant at the 1% level. In particular, Column (2) reports a coefficient of –0.018 ($p < 0.01$), corresponding to a 5.4% reduction in annual overtime relative to the sample mean of 0.331. These results provide empirical support for the hypothesis that binding compensation limits can attenuate managerial effort.

Columns (3) and (4) of Table 2 confirm this pattern. The *Treat* × *Post* coefficients are both negative and statistically significant, indicating that treated firms experienced declines in both board meetings and investor interactions post-policy. The magnitude differs markedly: investor interactions fell by 18.8% (0.033/0.175), while board meetings declined by only 1.0% (0.021/2.333). This contrast reflects an important distinction. Board meetings are often mandated by legal or regulatory requirements, making them less susceptible to managerial discretion (Adams and Ferreira, 2007). Executives may continue attending at baseline frequency despite diminished incentives, though possibly with reduced engagement. In contrast, investor relations are largely discretionary and effort-intensive, making them more sensitive to weakened motivation.

4.2 Risk-taking behavior

When compensation is capped, the upside from high-risk projects diminishes while downside risks remain salient. This asymmetry should increase managerial risk aversion (Kahneman and Tversky, 1979; Jensen and Meckling, 1976; Smith and Stulz, 1985). We test this prediction by examining changes in unrelated acquisitions and risk disclosures.

Table 3 shows that the coefficient for *Treat* × *Post* is consistently negative and statistically significant at the 1% level in both columns. Column (1) reports that SOEs became 76.2% less likely to engage in unrelated mergers and acquisitions, based on a coefficient of –0.112 relative to a baseline mean of 0.147. Column (2) shows a parallel decline in risk-related language in annual reports. These findings indicate that binding pay caps significantly constrain risk-taking behavior, with executives becoming more conservative in both actions and communications.

4.3 Investment decisions and capital allocation

Executive pay limits reshape corporate investment through multiple channels. Capping performance-based pay reduces the marginal gain from ambitious projects, weakening pay-for-performance mechanisms and encouraging conservatism (Smith and Stulz, 1985). Column (1) of Table 4 shows that treated firms reduced overall investment by 21.7% (coefficient of –0.010 relative to a mean of 0.046). This aggregate decline, however, masks important compositional shifts in capital allocation.

Columns (2) through (5) of Table 4 reveal systematic reallocation away from discretionary, high-risk uses toward safer distributions. The coefficients for *Treat* × *Post* are negative and significant for R&D investment (β = –0.032), M&A (β = –0.020), and share repurchases (β = –0.059), representing declines of 3.2%, 2.0%, and 5.9% respectively. In contrast, cash dividends increased by 6.4% (β = 0.064). This pattern suggests executives responded to capped incentives by prioritizing visible, low-risk capital distributions over long-term value creation.

These changes in investment quantity and composition translate into reduced investment efficiency using two complementary measures: investment-Q sensitivity and a residual-based efficiency metric. Table 5 examines whether pay caps help firms avoid wasteful overinvestment or hinder the pursuit of value-creating opportunities. Column (1) shows a negative and significant coefficient for *Treat* × *Post* × *Tobin's Q*, indicating that firms became less responsive to investment opportunities post-policy. Column (2) reports a positive and significant *Treat* × *Post* coefficient for *Investment inefficiency*, implying that actual investments increasingly diverged from optimal levels based on firm fundamentals.

Critically, Columns (3) and (4) reveal that this divergence is driven entirely by underinvestment. The significantly positive coefficient appears only in the underinvestment subsample, with no comparable effect for overinvesting firms. This suggests pay caps did not curtail excessive spending but rather constrained firms from pursuing beneficial investments, consistent with executives avoiding complex or risky decisions when personal incentives are weakened (Hicks, 1935).

4.4 Cash management

The passive approach to investment extends to cash management. If constrained incentives

reduce managerial motivation to deploy resources actively, we should observe three patterns: higher cash holdings, slower adjustment toward optimal levels, and lower market valuation of cash.

Panel A of Table 6 confirms the first prediction. The coefficient on Treat × Post is consistently positive and highly significant across specifications. Column (3), which includes firm and year fixed effects, shows that SOEs increased cash holdings by 11.5% more than non-SOEs post-policy. This accumulation suggests risk-averse managers prefer holding cash buffers rather than deploying capital toward uncertain investments.

Panel B examines whether this cash accumulation reflects passive hoarding or active management. Following Jiang and Lie (2016), we estimate how quickly firms adjust cash holdings toward optimal levels using the partial adjustment model:

$$\begin{aligned}\Delta\text{Cash}_{it} = \beta_0 &+ \beta_1\left(\text{Treat}_i \times \text{Post}_t \times \left(\text{Cash}^*_{it} - \text{Cash}_{i,t-1}\right)\right) + \beta_2\left(\text{Cash}^*_{it} - \text{Cash}_{i,t-1}\right) \\ &+ \beta_3(\text{Treat}_i \times \text{Post}_t) + \beta_4\left(\text{Treat}_i \times \left(\text{Cash}^*_{it} - \text{Cash}_{i,t-1}\right)\right) \\ &+ \beta_5\left(\text{Post}_t \times \left(\text{Cash}^*_{it} - \text{Cash}_{i,t-1}\right)\right) + \varepsilon_{it} \quad (2)\end{aligned}$$

where ΔCash_{it} is the change in cash holdings from year $t-1$ to t, and Cash^*_{it} is the estimated optimal cash level.[12] β_1 captures whether SOEs adjust more slowly toward this target post-policy compared to matched non-SOEs.

Column (1) shows that the triple interaction coefficient is negative and significant, indicating that SOEs reduced their adjustment speed following the policy. Columns (2) and (3) document that this slowdown is concentrated among firms with excess cash, while those with cash shortfalls show no change. Managers under constrained incentives appear less inclined to optimize cash management when liquidity is already sufficient.

Most tellingly, Panel C shows that investors recognized this inefficiency. We estimate the marginal value of cash following Faulkender and Wang (2006), regressing excess stock returns on changes in cash holdings:

[12] We discuss the calculation of optimal cash levels in Appendix IA.1.

$$r_{it} - R_{mt} = \beta_0 + \beta_1(\Delta\text{Cash}_{it} \times \text{Treat}_i \times \text{Post}_t) + \beta_2(\Delta\text{Cash}_{it} \times \text{Treat}_i) + \beta_3(\Delta\text{Cash}_{it} \times \text{Post}_t) + \beta_4(\text{Treat}_i \times \text{Post}_t) + \beta_5\Delta\text{Cash}_{it} + \beta_6\left(\frac{\Delta E_{it}}{M_{i,t-1}}\right) + \beta_7\left(\frac{\Delta NA_{it}}{M_{i,t-1}}\right) + \beta_8\left(\frac{\Delta I_{it}}{M_{i,t-1}}\right) + \beta_9\text{Cash}_{i,t-1} + \beta_{10}\left(\frac{NF_{it}}{M_{i,t-1}}\right) + \beta_{11}\left(\frac{L_{it}}{M_{i,t-1}}\right) + \beta_{12}(L_{it} \times \Delta\text{Cash}_{it}) + \beta_{13}(\text{Cash}_{i,t-1} \times \Delta\text{Cash}_{it}) + \beta_{14}\left(\frac{\text{DIV}_{it}}{M_{i,t-1}}\right) + \varepsilon_{it} \quad (3)$$

where the dependent variable is the firm's excess stock return over the market return in year *t*, and the key term $\Delta\text{Cash}_{it} \times \text{Treat}_i \times \text{Post}_t$ captures the change in the marginal value of cash for SOEs in the post-policy period. Control variables include changes in earnings, net assets, interest expenses, dividends, leverage, net financing, and lagged cash, with firm-specific variables scaled by lagged market equity, following Chang et al. (2021).

Across all specifications, the coefficient on *Treat* × *Post* × Δ*Cash* is negative and statistically significant at the 5% level, suggesting that treated firms have a lower value per dollar following the policy. Column (3) indicates the marginal value of cash declined by $0.263 per dollar for treated firms, suggesting markets correctly anticipated that accumulated cash would not be deployed productively.

4.5 Firm Performance

These behavioral changes — reduced effort, increased conservatism, underinvestment, and passive cash management — culminate in measurable performance declines. Table 7 presents results for two performance measures: Tobin's Q (market valuation) and total factor productivity (operational efficiency).

The coefficients on *Treat* × *Post* are negative and statistically significant at the 5% level in both specifications. Column (1) shows that SOEs experienced a 16.2% decline in Tobin's Q (0.327/2.013), while Column (2) documents a 1.17% drop in TFP (0.199/16.87) relative to control firms. These magnitudes, while economically significant, likely understate long-term costs as the full effects of reduced innovation and chronic underinvestment materialize over time.

Taken together, our results paint a consistent picture of managerial disengagement

following binding pay constraints. Executives do not appear to work harder for less pay. Instead, they retreat into a 'quiet life' characterized by reduced effort, heightened conservatism, and passive resource management. The consequences extend beyond individual behavior to affect capital allocation, operational efficiency, and ultimately shareholder value.

5 Heterogeneity and Robustness Tests

5.1 Heterogeneous Effects: When Does the Quiet Life Dominate?

Our baseline results document systematic managerial disengagement following binding pay caps. However, the magnitude of this response likely varies across firms depending on alternative sources of motivation and discipline. We examine four dimensions that shape how executives respond to constrained compensation: CEO career horizons, industry competition, internal governance, and SOE characteristics.[13]

We begin by examining the CEO's career concerns. Executives with strong promotion prospects may remain motivated despite compensation caps, as they value future opportunities over current pay (Cao et al., 2019). In China's SOE context, where executives often hold dual roles as political cadres, career advancement depends partly on signaling competence to political superiors (Guo et al., 2025). We proxy for career horizons using CEO age, with older CEOs having limited promotion prospects and thus weaker non-monetary incentives (Belenzon et al., 2019).

Panel A of Table 8 shows that firms led by older CEOs (above the sample median age in 2014) experienced significantly larger declines in effort, risk-taking, and investment post-policy. The triple interaction *Treat* × *Post* × *Old CEO* is negative and significant in four of five specifications. This pattern suggests that when career advancement prospects are limited, the loss of monetary incentives has a more pronounced demotivating effect. The one exception, Column (2) on overtime ratio, may reflect measurement differences between media-reported and satellite-detected overtime, but the overall evidence strongly supports differential responses based on career horizons.

We next examine whether the effects of executive pay limits vary with industry

[13] We construct our proxy variables based on their 2014 values. As a result, firms lacking data on *CEO age*, *HHI*, or *INC* values in 2014 are excluded from the analysis. Consequently, the number of observations varies slightly across panels.

competition. In competitive markets, executives face intense performance pressure and typically receive compensation tied closely to outcomes (Gartenberg and Wulf, 2020). Capping variable pay in such settings may be particularly demotivating, as it constrains rewards for managers operating under scrutiny with attractive outside options. Conversely, executives in monopolistic industries may already enjoy a 'quiet life' (Hicks, 1935), making pay caps less consequential at the margin. Panel B of Table 8 confirms this prediction. Using the Herfindahl-Hirschman Index to measure competition (with lower values indicating more competitive markets), we find that SOEs in competitive industries (industries with below median HHI values in 2024) show significantly stronger behavioral responses to pay caps. The triple interaction *Treat* × *Post* × *Competitive industries* coefficients are negative and significant across most specifications, indicating that uniform compensation policies generate heterogeneous effects depending on market discipline.

Third, we examine governance quality. Strong internal monitoring may partially substitute for monetary incentives by maintaining managerial discipline through alternative mechanisms (Bertrand and Mullainathan, 2003). We measure governance quality using the Internal Control Index (*INC*), which captures the effectiveness of firms' control systems in promoting discipline and preventing value diversion (Chen et al., 2020). Panel C reports that firms with weak governance (below median *INC* in 2014) experienced sharper declines in effort and risk-taking following pay caps. The coefficient on *Treat* × *Post* × *Weak governance* is negative and significant in most specifications, though interestingly not for investment levels (Column 5). This suggests that while weak governance amplifies the behavioral effects of constrained compensation, investment decisions may be subject to additional constraints or approval processes that limit managerial discretion regardless of governance quality.

Variation within SOEs

Cheung et al. (2010) show that the identity of the state owner and the political affiliations of directors shape behavior in Chinese listed firms. In related-party deals, firms tied to local governments expropriate minority investors, whereas firms tied to the central government tend to create value. These patterns are not explained by the level of state ownership, by industry mix, or by inferior operating performance of local SOEs, and they are consistent with weaker

scrutiny and greater administrative protection at the local level. This evidence implies that SOEs are not homogeneous across ownership tiers and motivates tests of heterogeneity by central versus local control in our setting.

We therefore split SOEs into central and local groups and re-estimate all outcomes. Internet Appendix IA.2 shows that both types respond to the 2015 cap in the same direction. The *Treat* × *Post* coefficients are negative and statistically significant for both central and local SOEs across effort, risk-taking, investment, and related M&A activity, with only modest differences in magnitude. [14] Combined with evidence that the reform diffused beyond centrally administered firms to provincial SOEs, the results suggest uniform enforcement through administrative guidance rather than selective application. The similarity of responses across ownership tiers indicates that the main effects we document are unlikely to be driven by governance weaknesses specific to local SOEs.

Internet Appendix IA.3 documents that pre-existing pay gaps between executives and employees do not appear to moderate the response. Firms with both high and low initial pay disparities show similar behavioral changes. This implies the absolute loss of compensation, rather than relative pay compression, drives disengagement. Most surprisingly, Internet Appendix IA.4 shows that politically connected CEOs (those with prior government experience) respond no differently than non-connected executives. This uniform response suggests that all SOE executives, regardless of formal political ties, are part of the political-administrative elite and equally responsive to centrally imposed reforms.

5.2 Robustness Tests and Identification

We next conduct an extensive series of tests to show that our results reflect causal effects of pay caps rather than confounding factors or methodological artifacts. These tests address potential concerns regarding parallel trends, model specification, and alternative explanations.

The validity of our difference-in-differences design rests on the parallel trends assumption. Hence, we begin by evaluating the parallel trends assumption underpinning our DID design.

[14] In unreported results, we extend the analysis across all outcome variables to compare CSOEs and LSOEs, finding that both types of SOEs are influenced by the 2015 pay limit policy. LSOEs exhibit a slightly more pronounced decline in board meetings, R&D investment, and pay–performance sensitivity, whereas CSOEs show a larger decline in M&A activity, value of cash, and experience slightly higher rates of CEO turnover.

Figure 2 presents event-study estimates with 95% confidence intervals, showing coefficients indistinguishable from zero before 2015 but significant negative effects afterward. This pattern - flat pre-trends followed by sharp post-treatment divergence, supports a causal interpretation.

Next, we conduct a placebo test. We randomly reassign the treatment status across firms 1,000 times and re-estimate the baseline model in each iteration. This generates a distribution of placebo treatment effects under the null hypothesis of no policy impact. Figure 3 plots the resulting distribution. The actual treatment effects lie far in the tails of these null distributions, with p-values below 0.001 for all main outcomes. This rules out random variation as an explanation for our findings.[15]

Our results are also robust across multiple estimation approaches. Internet Appendix IA.5 confirms that Poisson and logit models for count and binary outcomes yield similar conclusions. Internet Appendix IA.6 shows that Double Machine Learning with Lasso selection, which addresses omitted variable concerns through data-driven covariate selection, produces comparable estimates.

To address potential selection concerns, we implement propensity score matching (1:3 nearest neighbor with 0.25 caliper). Internet Appendix IA.7 confirms satisfactory covariate balance post-matching. Dynamic effects using the matched sample (IA.8) mirror our main results, with no pre-trends and significant post-treatment effects. Placebo tests on the matched sample (IA.9) again place actual effects in the extreme tails of null distributions.

Finally, Figure 4 presents our most comprehensive robustness check using a specification curve. Specifically, we re-estimate the difference-in-differences models in 120 different specifications, varying fixed effects and clustering, adding or dropping control variables, tightening the sample, using additional matching methods, such as one-to-one PSM, and explicitly controlling for concurrent reforms such as the 2012 anti-corruption campaign (using province-level intensity measures) and 2015 deleveraging initiatives (using firm leverage interactions). Figure 4 plots the *Treat* × *Post* estimate from each specification with 90, 95, and 99 percent confidence intervals against a zero line. Across all versions, the point estimates

[15] We also test for pre-policy anticipatory behavior by simulating a hypothetical policy shock in 2013 using only pre-policy data (2011–2014). When we re-estimate the model using this restricted sample and assign treatment in 2013, the interaction term is statistically insignificant, indicating that our results are not driven by latent shifts prior to the actual policy implementation. Results are available upon request.

remain negative and statistically significant at the one percent level, and the magnitudes are stable.

To interpret the figure, each dot can be viewed as one credible way to run the model. If our results were fragile, some estimates would flip sign or their confidence intervals would cross zero. Instead, *every* estimate lies on the negative side of zero and the uncertainty bands do not cross it in any of the 120 runs. The curve indicates that the link between binding pay caps and managerial disengagement is not driven by a particular modeling choice and is unlikely to be a methodological artifact.

Taken together, these analyses demonstrate that the 'quiet life' response to pay caps is both heterogeneous — stronger where alternative incentives are weaker — and robust to identification concerns. The evidence points to a fundamental trade-off between pay compression and managerial motivation that policymakers cannot easily escape through better design or implementation.

6. Mechanisms: Selection, Incentives, and Reallocation

The behavioral changes documented in Section 4 raise several questions about the underlying mechanisms. Does the 'quiet life' emerge because capable executives exit, leaving behind less motivated managers? Do formal incentive structures weaken, disconnecting pay from performance? When traditional compensation channels close, do executives pursue alternative forms of value extraction or redirect effort toward non-financial objectives? This section examines these mechanisms to understand how binding pay caps transform the internal dynamics of affected firms.

6.1 Executive Selection: The Exit of Talent

When performance-based rewards disappear, high-ability executives face a straightforward choice: accept dramatically reduced compensation or seek opportunities elsewhere. The private sector offers market-aligned pay structures that reward talent and effort — precisely what the pay caps eliminate (Nanda et al., 2024). Anecdotal evidence suggests this dynamic, with reports of executives transitioning from SOEs to privately controlled firms with

more flexible governance and incentive systems.[16] If capable executives disproportionately exit SOEs, the observed behavioral changes may reflect selection effects alongside incentive effects.

To test this selection mechanism, we examine CEO turnover patterns following the policy. Table 9 presents results using two approaches. First, following Nanda et al. (2024), we classify turnovers as voluntary when CEOs depart due to resignation, term expiration, or personal reasons according to CSMAR classification, excluding moves to affiliated firms, which often represent promotions. Column (1) shows that voluntary turnover probability increased significantly post-policy. Column (2) quantifies this effect: a 1.9% increase, representing a 35.8% rise from the baseline rate of 5.3%. This substantially exceeds the 20% increase documented by Nanda et al. (2024) following China's 2009 pay regulation, suggesting the 2015 intervention had more severe consequences for executive retention.[17]

Second, we adopt Lin et al.'s (2022) performance-based definition, classifying exits as voluntary when CEOs leave despite strong firm performance (above-median industry-adjusted ROA in year *t*-1). Columns (3) and (4) confirm that high-performing executives were particularly likely to exit following pay caps. This pattern — successful managers leaving when rewards vanish — suggests adverse selection compounds the incentive problems.

Could these turnover patterns themselves explain our main results? To address this concern, we implement propensity score matching based on ex-ante turnover probability during 2011-2014. Internet Appendix IA.10 details the matching procedure, which creates balanced samples of firms with similar baseline turnover risk. Table 10 re-estimates our main specifications using these matched samples. Crucially, the negative effects on effort, risk-taking, and investment persist regardless of turnover patterns. This indicates that while talent flight may amplify the consequences of pay caps, the incentive effects operate independently among executives who remain.

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[16] A notable 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).

[17] 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. In addition, Nanda et al. (2024) estimate both the turnover of CEO and chairman.

6.2 Incentive structure: The severing of pay from performance

The behavioral disengagement we document implies a fundamental weakening of performance incentives. But does this reflect an unintended consequence or deliberate policy design? China's pay reform faced a choice between two objectives: improving incentive efficiency by better aligning pay with performance, or promoting equity by compressing compensation regardless of outcomes. The policy's actual impact on pay-performance sensitivity reveals which goal dominated.

Following Bae et al. (2024), we estimate a triple-difference specification examining how pay-performance sensitivity changed for SOEs relative to control firms:

$$\begin{aligned} LnCompensation_{it} = {} & \lambda_0 + \lambda_1(Treat_i \times Post_t \times Performance_{it}) \\ & + \lambda_2(Post_t \times Performance_{it}) + \lambda_3(Treat_i \times Performance_{it}) \\ & + \lambda_4(Treat_i \times Post_t) + \lambda_5 Performance_{it} + \lambda_6 Z_{it} + \delta_i + \phi_t + \varepsilon_{it} \end{aligned} \quad (4)$$

where the dependent variable is the logarithm of CEO compensation (*LnCompensation*), and *Performance* is measured by *ROA* or return on sales (*ROS*). The coefficient λ_1 captures the change in pay-performance sensitivity among SOEs relative to control firms following the policy. Z includes firm and CEO level characteristics. δ and ϕ denote firm and year fixed effects, respectively.

Table 11 shows that CEO compensation became significantly less responsive to firm performance in SOEs post-policy. The coefficient on *Treat* × *Post* × *Performance* is negative whether using ROA (Column 1) or ROS (Column 2) as performance measures. This severing of the pay-performance link contradicts any efficiency rationale and confirms that equity considerations — reducing inequality regardless of merit — dominated policy design. By prioritizing fairness over incentives, the reform inadvertently created the conditions for managerial disengagement.

6.3 Alternative Channels: Rent-Seeking

When formal compensation channels close, executives might pursue informal value extraction through perquisites or tunneling. Prior research documents exactly this substitution following China's 2009 pay regulation (Bae et al., 2024). The concern is particularly acute in weak-governance environments where monitoring is limited and informal benefits are difficult

to detect. To investigate whether the 2015 policy elicited similar responses, we examine two prevalent channels of rent extraction: (1) the log of total perks per executive (Gul et al., 2011), and (2) net other receivables, a standard measure of tunneling (Jiang et al., 2010).

Table 12 presents surprising results: both perk consumption and tunneling actually declined following the 2015 pay caps. The coefficient on *Treat* × *Post* is negative and significant for both perks (measured as discretionary expenses per sales dollar) and tunneling (proxied by net other receivables). Perks fell by approximately 0.1% while tunneling dropped by 44.1% in treated firms.

This unexpected pattern likely reflects the deterrent effect of concurrent anti-corruption enforcement. The Party's Eight-Point Frugality Code, introduced in 2012 and intensified through 2015, dramatically increased the political and legal risks of rent extraction. Unlike the 2009 reform, which operated in isolation, the 2015 pay caps coincided with a broader crackdown on executive excess. Executives faced a double bind: formal compensation was capped, while informal channels became dangerous to pursue.

The absence of rent-seeking reinforces our interpretation of managerial disengagement. Unable to extract value through either formal or informal channels, executives retreated into passivity—the 'quiet life' emerged not as a choice but as the only remaining option.

6.4 Effort Reallocation to Political Objectives

While executives disengage from profit-maximizing activities, they do not appear to become entirely passive. Instead, they redirect their efforts toward alternative goals that yield reputational or political returns. In SOEs, these goals frequently align with broader state priorities, particularly in areas such as ESG performance and CSR involvement.

Table 13 provides clear evidence of such reallocation. Both ESG ratings and CSR scores increased significantly in treated firms following the implementation of pay limits. The positive and significant *Treat* × *Post* coefficients suggest that executives shifted their limited effort away from financial outcomes and toward activities that generate political capital. These results demonstrate that executive behavior adapts strategically under altered incentive structures.

This reallocation reflects rational optimization under altered incentives. With monetary rewards capped and rent-seeking dangerous, executives seek alternative sources of utility —

what Zhang et al. (2025) term “warm glow” benefits from social contributions. In China’s political economy, maintaining employment and achieving environmental goals earn political credit that may substitute for financial success. Cheng et al. (2024) document similar behavior among politically ambitious SOE executives who prioritize social objectives over profitability. Importantly, this shift represents a form of agency problem distinct from traditional rent-seeking. Executives are not expropriating shareholder wealth directly but rather repurposing corporate resources toward objectives that benefit their political standing while potentially destroying shareholder value. The pivot to social goals allows executives to maintain legitimacy and job security while avoiding the risks and effort required for competitive business success — a sophisticated version of the quiet life that appears socially beneficial while undermining economic efficiency.

6.5 Executive pay limits and labor adjacent outcomes

Building on Zhang et al. (2025), we next examine how salary cuts affect outcomes that are directly linked to labor, including job creation, labor productivity, and wage-bill intensity. Labor outcomes represent another dimension through which executives may reallocate effort, reflecting the political and social salience of employment stability in China. Our analysis incorporates firm and provincial × year fixed effects to account for unobserved heterogeneity at both the firm and regional labor market levels.

The results, presented in Table 14, are broadly consistent with Zhang et al. (2025). Specifically, executive pay limits are associated with an increase in the number of employees. In contrast, columns (2) and (3) indicate that both labor productivity and wage-bill intensity decline following the reform. The pattern is consistent with executives shifting effort toward politically salient job creation while letting operational efficiency slip. Though headcount expands in our sample firms, output per worker falls, and the share of assets devoted to payroll shrinks, suggesting hiring at the margin into lower-wage or lower-productivity roles rather than productivity-enhancing initiatives. This interpretation aligns with earlier evidence in the paper that executives reallocate effort to social objectives (ESG/CSR) under weakened monetary incentives. The specification absorbs regional labor-market shocks with province-year effects, which reduces the likelihood that local macro conditions alone explain the results. Overall, the

labor results also support a quiet-life response: more visible jobs, less intensive effort, and lower efficiency.

Overall, the mechanisms examined in this section — talent exodus, severed pay-performance links, blocked rent-seeking channels, and effort reallocation — appear to work in concert to produce the quiet life. High-ability executives exit, leaving firms with less capable managers or less motivated to drive performance. Those who remain face weakened incentives, with pay disconnected from outcomes. Traditional safety valves of perks and tunnelling are sealed by anti-corruption enforcement. The only remaining strategy is to minimize effort while maintaining political legitimacy through social objectives.

7. Conclusion

This study exploits China's 2015 SOE executive pay ceiling, a binding, comprehensive cap that produced a sharp drop in compensation, to examine what happens when monetary incentives collapse under a binding, inescapable cap. Our evidence documents a fundamental behavioral response: executives do not appear to work harder for less pay, nor do they simply extract value through alternative channels. Instead, they retreat into what Hicks (1935) termed the 'quiet life' — a systematic disengagement from effort-intensive, risk-bearing activities that drive corporate dynamism.

The pattern we document is both broad and deep. Treated firms show a 5.4% reduction in work effort, a 76.2% decline in unrelated acquisitions, a 21.7% drop in capital investment, and a 16.2% decrease in Tobin's Q. These effects are concentrated where alternative motivations are weakest: among older CEOs with limited promotion prospects, in competitive industries where performance pressure is highest, and in firms with poor internal governance. However, even younger executives with strong career concerns reduce effort, revealing the limits of relational incentives and reputational capital as substitutes for monetary rewards.

Our mechanism analysis also documents several novel results that distinguish this episode from previous pay regulations. First, the exodus of talent is substantial — voluntary CEO turnover increased by 35.8%, with high performers particularly likely to exit. This adverse selection compounds the incentive effects among those who remain. Second, contrary to experiences with earlier reforms, we find no increase in perks or tunneling. The concurrent

anti-corruption campaign sealed these traditional safety valves, leaving executives with neither formal nor informal channels for value extraction. Third, and perhaps most revealing, executives redirected their limited effort toward politically palatable but economically marginal activities: ESG initiatives, employment preservation, and other social objectives that generate "warm glow" utility without requiring intensive managerial effort or risk-taking.

This behavioral transformation from value maximization to political legitimacy represents a sophisticated form of agency problem that is distinct from traditional rent-seeking. Here, executives do not appear to expropriate shareholders directly. Instead, they reallocate corporate resources toward objectives that preserve their positions while avoiding the demands of competitive business. The result is a managerial culture that prioritizes survival over success, stability over growth, and political safety over economic value creation.

Three implications stand out for Western policymakers considering ratio-based taxes, bonus caps, or binding pay ceilings. First, while our results likely represent an upper bound given China's extreme intervention, they demonstrate that the behavioral response to pay constraints is real and economically significant. Even proportionally smaller restrictions in market economies could meaningfully impact corporate dynamism and innovation. Second, the mechanism we identify — the failure of non-monetary incentives to maintain effort when monetary rewards vanish — is not unique to SOEs or weak-governance environments. All executives, even in high-governance U.S. firms, balance financial and non-financial motivations. Our evidence suggests that when monetary incentives collapse, alternative motivations cannot fully compensate, particularly for the high-effort, high-risk activities that drive long-term value.

Third, and perhaps most importantly, the costs of pay regulation manifest not through spectacular governance failures but through subtle disengagement. The 'quiet life' does not involve empire-building or outright theft, traditional agency problems that governance systems are designed to detect and prevent. Instead, it emerges as foregone opportunities: delayed decisions, avoided risks, uninitiated innovations, and accumulated cash that sits idle rather than funding growth. These passive costs may be harder to observe than active malfeasance, but our evidence suggests they are no less real.

These trade-offs merit closer study as societies weigh inequality against efficiency. Our evidence does not imply that pay regulation is inherently welfare-reducing, but it shows the efficiency costs are real and potentially large. Current proposals in the EU, UK, and US often assume managers will sustain effort under compressed pay — an assumption our results challenge. For boards, the findings highlight the fragility of incentive systems, with even voluntary pay ratios or stakeholder-oriented compensation risking similar behavioral shifts. Markets appear to recognize this link, as reflected in lower cash valuations for affected firms. More broadly, when political, social, or regulatory pressures limit monetary incentives, organizations must either accept reduced managerial effort or design new governance models. The ‘quiet life’ we document is avoidable, but only with deliberate substitutes for lost financial motivation.

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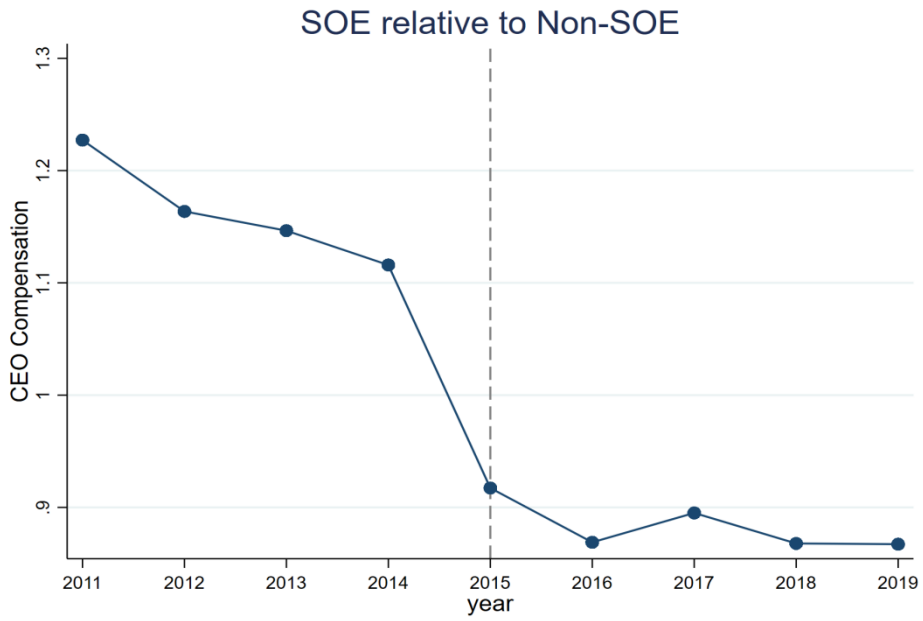
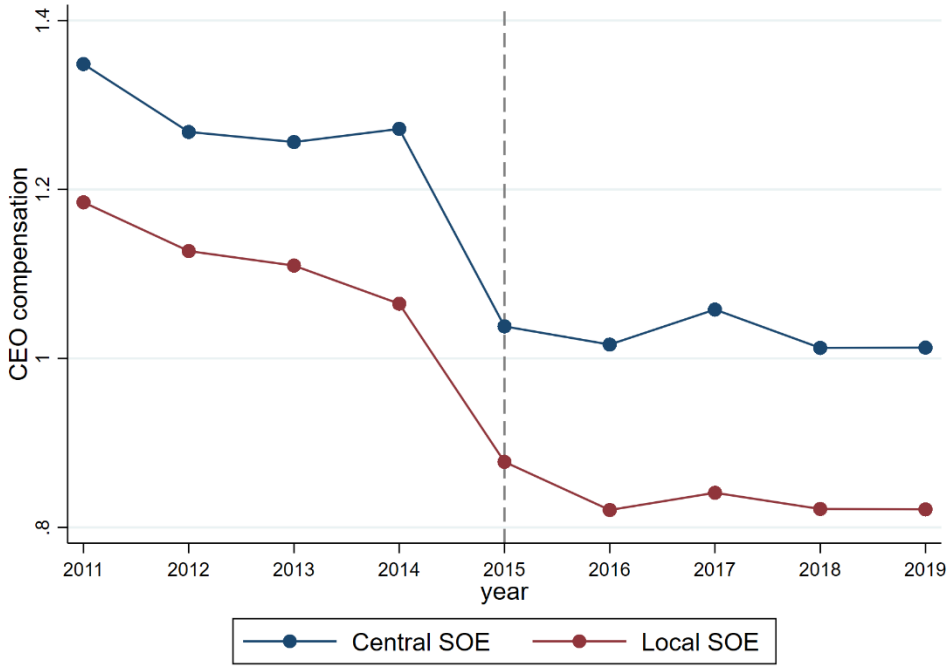
Appendix. Variable definitions

Variable	Description
SOE	A dummy variable that equals 1 if a firm is ultimately controlled by the government, and 0 otherwise.
Central SOE	A dummy variable that equals 1 if a firm is ultimately controlled by the central government, and 0 otherwise.
Local SOE	A dummy variable that equals 1 if a firm is ultimately controlled by local governments, and 0 otherwise.
Post	A dummy variable that equals 1 for 2015 and subsequent years, otherwise 0.
Corporate behavior	
Ln(1+Overtime Keywords)	The 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.
Ln (1+Board Meetings)	The natural logarithm of one plus the number of board meetings
Ln (1+Investor Interactions)	The natural logarithm of one plus the number of CEO–investor interactions
Unrelated Acquisition(0/1)	A dummy variable that equals one if the firm engages in a conglomerate merger, and zero otherwise.
Risk Tone	The natural logarithm of one plus the number of risk-related keywords extracted from annual reports.
Investment	The cash paid for fixed assets, intangible assets, and other long-term assets minus the cash received from their disposal, scaled by total assets.
Repurchase(0/1)	A dummy variable equal to one if a firm repurchased its own shares from the market in a given year, and zero otherwise.
R&D(0/1)	A dummy variable equal to one if a firm reported R&D expenditure in that year, and zero otherwise.
M&A(0/1)	A dummy variable equal to one if a firm engaged in any M&A activity during the year, and zero otherwise.
Dividend (0/1)	A dummy variable equal to one if a firm distributed cash dividends in that year, and zero otherwise.
Investment inefficiency	The absolute terms of residual ε from Richardson (2006). Positive (negative) ε denotes overinvestment (underinvestment).
Cash	The ratio of cash and cash equivalents to net assets.
Tobin's Q	The market value of equity plus the book value of debt divided by the book value of total assets.
TFP	Total factor productivity using the Levinsohn and Petrin (2003) method.
CEO turnover (Nanda et al., 2024)	A dummy equal to one if the CEO leaves the firm due to resignation, term expiration, or personal reasons.
CEO turnover (Lin et al., 2022)	A dummy variable equal to one if a CEO was replaced despite good firm performance in the prior year. Good performance is defined as an ROA above the industry-adjusted median.
Ln(Compensation)	The logarithm of CEO compensation.
Perk	The ratio of discretionary expenses to sales. These expenses are disclosed in the "Cash Payments for Expenses Related to Operating Activities" section of the financial statement footnotes.
Tunneling	The logarithm of net other receivables.
ESG	Bloomberg ESG scores.
CSR	Corporate social responsibility scores sourced from Hexun.com.
Employment	The natural logarithm of a firm's total number of employees.
Labor productivity	Total sales (in millions) divided by the number of employees
Wage bill intensity	The ratio of total salary expenses to total assets
Size	The natural logarithm of total assets.
Leverage	Total liabilities divided by total assets.
Largest shareholder stake	The percentage of total shares held by the largest shareholder.
Cash flow	Operating cash flow divided by total assets.
ROA	Net income divided by total assets.
ROS	Net income divided by sales.
Board size	The natural logarithm of the number of board directors.

Independent director ratio	The number of independent directors divided by the total number of board members.
Firm age	The natural logarithm of the number of years since the firm’s establishment.
CEO age	The natural logarithm of the CEO's age.
CEO female(0/1)	A dummy variable equal to 1 if the CEO of firm i is female, and 0 otherwise.
CEO dual(0/1)	A dummy variable that equals 1 if firm i's CEO is also the chairman of the board of the same firm, and 0 otherwise.
LnGDP	The natural logarithm of local GDP at the city level.
LnPopulation	The natural logarithm of local population at the city level.
LnEmployment	The natural logarithm of working population at the city level.
Fiscal pressure	The ratio of the difference between expenditure and revenue to local GDP at the city level.
ΔEarnings	The 1-year change in the ratio of earnings before extraordinary items to market valuations.
ΔNet Assets	The 1-year change in the ratio of net assets to market value, where net assets are defined as total book assets minus cash and cash equivalents.
ΔInterest Expenses	The 1-year change in the ratio of interest expenses to market valuations.
Net Financing	The ratio of financing cash flows to market valuations.
ΔDividends	The 1-year change in the ratio of cash dividends to market valuations.
ΔCash	The 1-year change in the ratio of cash and cash equivalents to net assets.



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



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

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 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.

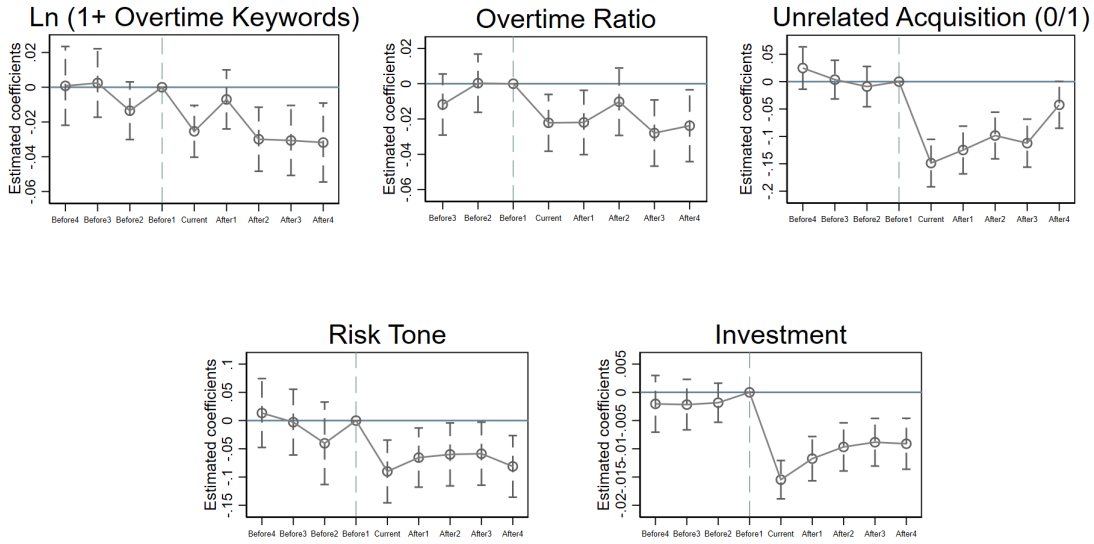


Figure 2. Dynamic effects of executive pay limit regulation on firm behavior

Note: This figure illustrates the time-varying effects of the executive pay limit 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.

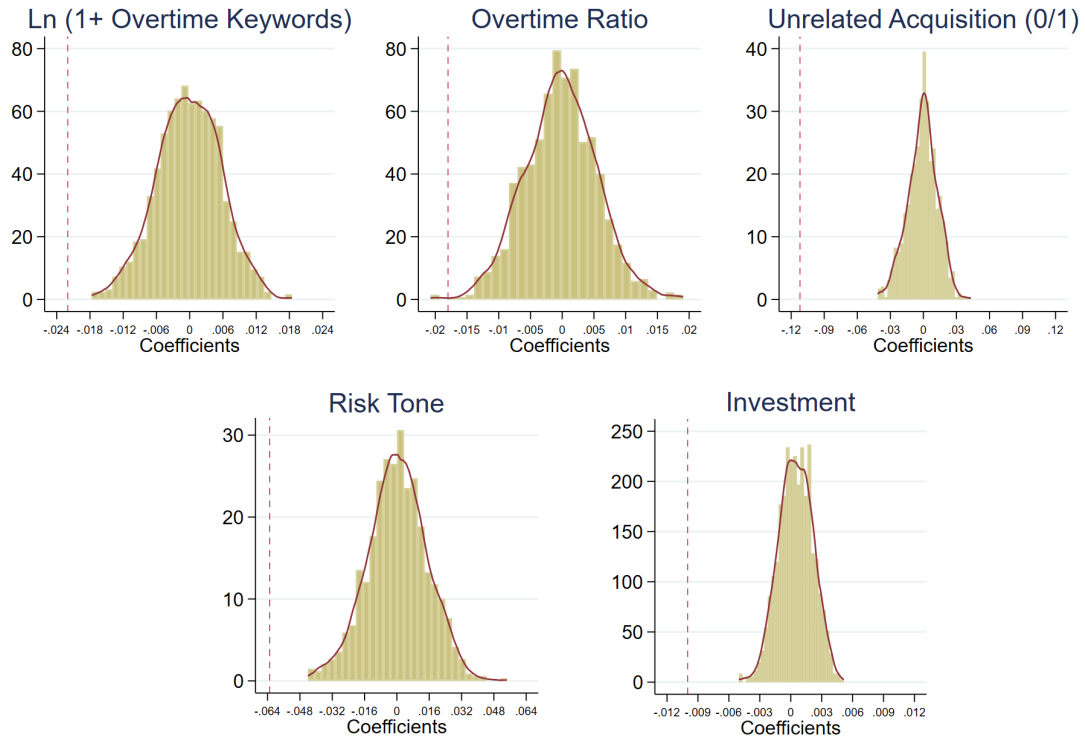


Figure 3. Placebo tests for five direct behavioral outcomes

Note: 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.

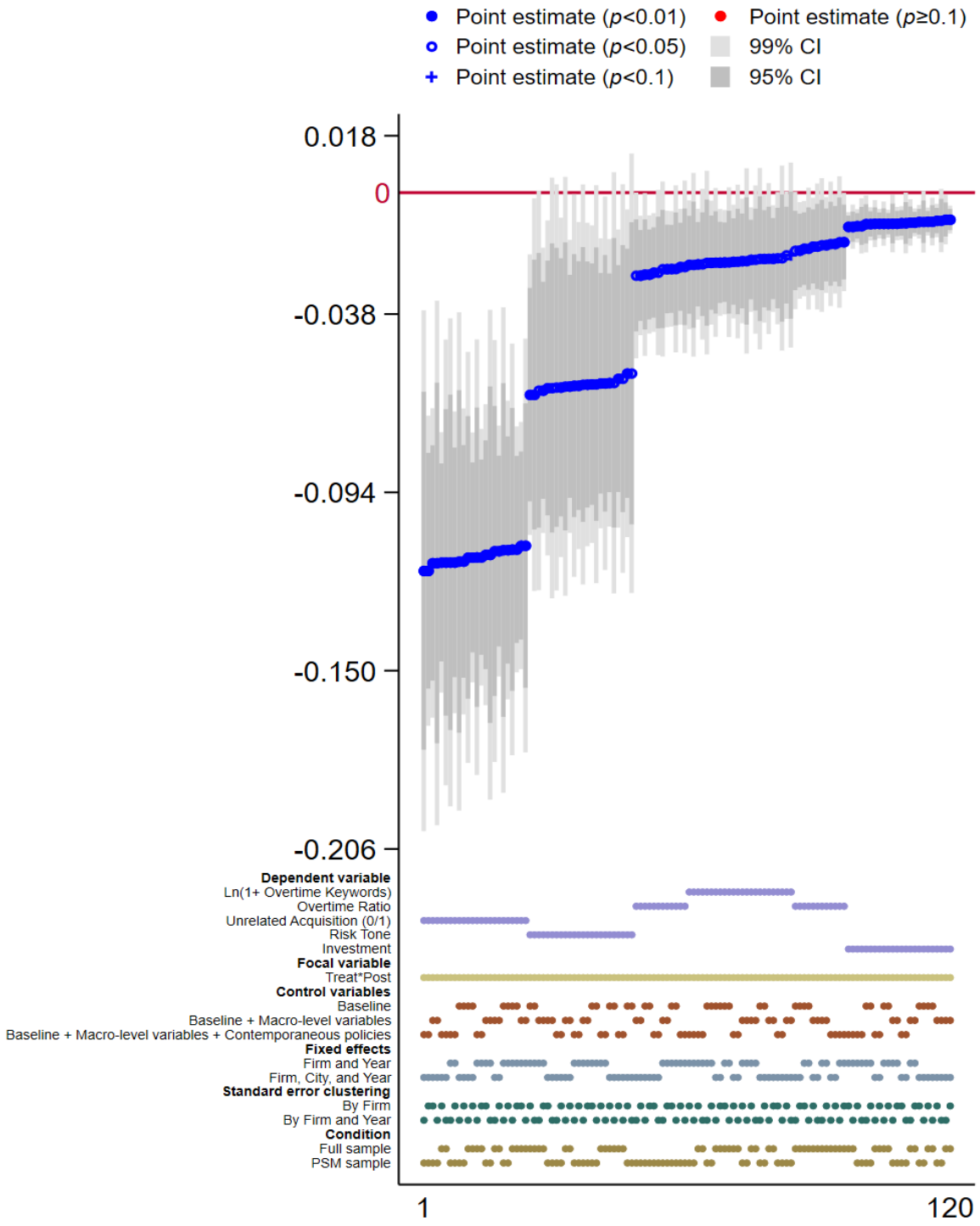


Figure 4. Specification curve analysis

Note: This figure displays the results of a specification curve analysis, which showcases the impact of the executive pay limit regulation on measures of work effort, risk-taking, and investment activity across 120 model specifications. The dependent variables include (*Ln* (1+*Overtime Keywords*), *Overtime Ratio*, *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 presents variable statistics. Definitions for all variables are provided in the Appendix. Continuous variables are winsorized at the 1st and 99th percentiles to mitigate the influence of outliers.

Variable	N	Mean	SD	p25	p50	p75
Ln(1+Overtime Keywords)	18,825	0.026	0.168	0	0	0
Overtime Ratio	14,406	0.331	0.156	0.214	0.328	0.442
Ln(1+Board Meetings)	18,825	2.333	0.357	2.079	2.303	2.565
Ln(1+Investor Interactions)	18,825	0.175	0.424	0	0	0
Unrelated Acquisition(0/1)	18,825	0.147	0.354	0	0	0
Risk Tone	18,825	5.430	0.499	5.187	5.489	5.71
Investment	18,814	0.046	0.047	0.013	0.034	0.066
Repurchase(0/1)	18,825	0.039	0.193	0	0	0
R&D(0/1)	18,825	0.190	0.392	0	0	0
M&A(0/1)	18,825	0.091	0.288	0	0	0
Dividend(0/1)	18,825	0.712	0.453	0	1	1
Investment inefficiency	15,395	0.022	0.023	0.007	0.015	0.028
Cash	18,825	0.237	0.295	0.075	0.14	0.266
Tobin’s Q	18,325	2.013	1.33	1.212	1.565	2.277
TFP	18,814	16.87	1.102	16.105	16.777	17.547
Perk	17,779	0.008	0.011	0.002	0.005	0.01
Tunneling	18,815	18.148	2.07	16.837	18.211	19.542
CEO turnover (Nanda et al., 2024)	18,457	0.053	0.225	0	0	0
CEO turnover (Lin et al., 2022)	18,457	0.036	0.185	0	0	0
Employment	18,817	7.705	1.326	6.682	7.644	8.494
Labor productivity	18,817	1.610	2.281	0.531	0.887	1.635
Wage bill intensity	18,401	0.064	0.046	0.031	0.053	0.085
ESG	7,639	20.419	6.829	15.803	19.835	23.140
CSR	18,801	13.556	6.500	9.580	14.200	18.260
Ln(Compensation)	17,996	13.222	0.771	12.771	13.221	13.676
Treat	18,825	0.374	0.484	0	0	1
Post	18,825	0.563	0.496	0	1	1
Size	18,825	22.236	1.305	21.312	22.063	22.99
Leverage	18,825	0.433	0.211	0.263	0.427	0.592
Largest shareholder stake	18,825	0.351	0.149	0.233	0.33	0.45
Cash flow	18,825	0.041	0.071	0.003	0.042	0.083
ROA	18,825	0.033	0.063	0.012	0.033	0.062
ROS	18,824	0.063	0.189	0.023	0.064	0.131
Board size	18,825	2.288	0.249	2.197	2.303	2.485
Independent director ratio	18,825	0.38	0.072	0.333	0.364	0.429
Firm age	18,825	2.813	0.353	2.639	2.833	3.045
CEO age	18,457	49.276	6.534	45	50	54

CEO female(0/1)	18,459	0.065	0.247	0	0	0
CEO dual(0/1)	18,342	0.258	0.438	0	0	1

Table 2. Executive pay limits and managerial effort

This table presents the estimated effects of the executive pay limit 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. Columns (3) and (4) measure managerial engagement using *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 times the CEO participates in investor interaction activities. All variable definitions are provided in the Appendix. All regressions control for firm and year-fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * stand for statistical significance at the 1%, 5% and 10% level, respectively.

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Ln (1+Board Meetings)	Ln (1+Investor Interactions)
	(1)	(2)	(3)	(4)
Treat×Post	-0.022***	-0.018***	-0.021*	-0.033***
	(-3.435)	(-3.027)	(-1.927)	(-3.324)
Size	0.014***	0.005	0.109***	0.067***
	(3.115)	(1.075)	(14.434)	(8.236)
Leverage	-0.013	-0.040**	0.091***	-0.013
	(-0.854)	(-2.424)	(3.241)	(-0.441)
Largest shareholder stake	-0.046	-0.029	0.049	-0.087*
	(-1.540)	(-1.097)	(1.013)	(-1.671)
Cash flow	-0.002	-0.001	-0.194***	-0.008
	(-0.081)	(-0.029)	(-5.070)	(-0.210)
ROA	-0.020	-0.050*	0.127**	0.409***
	(-0.532)	(-1.687)	(2.570)	(6.686)
Board size	-0.004	-0.011	0.199***	-0.009
	(-0.470)	(-1.639)	(15.497)	(-0.615)
Independent director ratio	-0.011	0.049**	-0.024	-0.002
	(-0.476)	(2.269)	(-0.624)	(-0.041)
Firm age	0.026	-0.037	0.024	0.141***
	(0.720)	(-1.121)	(0.460)	(2.651)
Constant	-0.326**	0.375***	-0.646***	-1.662***
	(-2.322)	(2.875)	(-3.029)	(-7.308)
Firm and Year FE	Yes	Yes	Yes	Yes
Adj R^2	0.134	0.255	0.504	0.353
N	18,825	14,406	18,825	18,825

Table 3. Executive pay limits and risk-taking behavior

This table examines the impact of the executive pay limit regulation on corporate risk-taking. Column (1) employs *Unrelated Acquisition* (0/1), a binary indicator equal to one if the firm engages in a conglomerate merger, and zero otherwise. Column (2) uses *Risk Tone*, the natural logarithm of one plus the number of risk-related keywords extracted from annual reports. Variable definitions are provided in the Appendix. All regressions control for firm and year-fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * stand for statistical significance at the 1%, 5% and 10% level, respectively.

Dependent variable =	Unrelated Acquisition(0/1)	Risk Tone
	(1)	(2)
Treat×Post	-0.112***	-0.063***
	(-7.480)	(-3.713)
Size	0.015*	0.051***
	(1.718)	(4.740)
Leverage	0.007	0.035
	(0.212)	(0.938)
Largest shareholder stake	-0.019	-0.108
	(-0.353)	(-1.537)
Cash flow	0.030	-0.074
	(0.648)	(-1.330)
ROA	0.100*	-0.057
	(1.681)	(-0.739)
Board size	-0.005	0.028
	(-0.306)	(1.456)
Independent director ratio	0.006	0.067
	(0.125)	(1.166)
Firm age	0.077	-0.010
	(1.437)	(-0.112)
Constant	-0.374	4.280***
	(-1.566)	(12.574)
Firm and Year FE	Yes	Yes
Adj R^2	0.114	0.483
N	18,825	18,825

Table 4. Executive pay limits and investment allocation

This table examines the impact of the executive pay limit 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 control for firm and year-fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * stand for 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***	-0.032***	-0.020**	-0.059***	0.064***
	(-5.753)	(-4.954)	(-2.355)	(-10.285)	(4.918)
Size	0.005***	0.010**	0.035***	0.031***	0.103***
	(3.596)	(1.997)	(5.096)	(6.580)	(10.792)
Leverage	-0.005	0.032*	0.123***	-0.071***	-0.244***
	(-1.001)	(1.738)	(4.581)	(-3.733)	(-7.054)
Largest shareholder stake	0.028***	-0.079**	0.091**	-0.098***	0.179***
	(3.464)	(-2.353)	(2.258)	(-3.213)	(2.864)
Cash flow	0.011**	0.033	-0.001	-0.003	0.073
	(2.111)	(1.099)	(-0.035)	(-0.115)	(1.441)
ROA	0.037***	-0.029	0.264***	0.066*	1.984***
	(5.224)	(-0.758)	(5.638)	(1.794)	(29.027)
Board size	-0.004***	0.002	-0.007	-0.021**	-0.031**
	(-2.596)	(0.295)	(-0.591)	(-2.484)	(-1.974)
Independent director ratio	0.003	0.003	-0.046	0.010	0.045
	(0.694)	(0.106)	(-1.192)	(0.377)	(0.954)
Firm age	-0.043***	0.319***	-0.061	0.127***	-0.276***
	(-5.329)	(8.735)	(-1.469)	(3.204)	(-4.651)
Constant	0.063*	-0.926***	-0.565***	-0.884***	-0.777***
	(1.677)	(-6.021)	(-2.950)	(-5.886)	(-2.924)
Firm and Year FE	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.449	0.849	0.107	0.206	0.479
N	18,814	18,825	18,825	18,825	18,825

Table 5. Executive pay limits and corporate investment efficiency

This table reports the effect of the executive pay limit 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 residuals from Column (2), with Column (3) focusing on over-investing firms and Column (4) on under-investing firms. Variable definitions are provided in the Appendix. All regressions control for firm and year-fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * stand for statistical significance at the 1%, 5% and 10% level, respectively.

Dependent variable =	Investment	Investment inefficiency		
	Full sample	Full sample	Over investment	Under investment
	(1)	(2)	(3)	(4)
Treat×Post	0.005	0.004***	0.000	0.006***
	(1.384)	(4.503)	(0.252)	(7.002)
Treat×Post×Tobin's Q	-0.008***			
	(-4.989)			
Tobin's Q	-0.001*			
	(-1.702)			
Treat×Tobin's Q	0.005***			
	(3.370)			
Post×Tobin's Q	0.003***			
	(3.666)			
Size	0.006***	0.000	0.001	-0.001
	(3.819)	(0.228)	(0.466)	(-1.123)
Leverage	-0.004	-0.001	-0.003	-0.001
	(-0.912)	(-0.485)	(-0.483)	(-0.412)
Largest shareholder stake	0.026***	0.009**	0.010	0.006
	(3.239)	(2.109)	(1.021)	(1.529)
Cash flow	0.012**	0.003	0.014*	-0.009***
	(2.226)	(0.897)	(1.747)	(-2.940)
ROA	0.038***	0.012***	0.035***	-0.000
	(5.144)	(2.798)	(3.525)	(-0.002)
Board size	-0.005***	0.000	-0.000	0.001
	(-2.764)	(0.289)	(-0.100)	(0.672)
Independent director ratio	0.003	-0.007**	-0.013*	-0.004
	(0.620)	(-2.047)	(-1.854)	(-1.305)
Firm age	-0.042***	-0.010**	-0.012	-0.011**
	(-5.163)	(-2.305)	(-1.317)	(-2.567)
Constant	0.041	0.045**	0.048	0.066***
	(1.055)	(2.178)	(1.097)	(3.231)

Firm and Year FE	Yes	Yes	Yes	Yes
Adj R^2	0.452	0.205	0.186	0.201
N	18,302	15,373	5,708	9,177

Table 6. Executive pay limits and corporate cash management

This table presents the impact of the executive pay limit regulation on cash management. Panel A examines the effect of executive pay limits on corporate cash level, measured by the ratio of cash and cash equivalents to net assets (*Cash*). Columns (1) to (3) present the regression results that include industry-fixed effects, industry and year-fixed effects, and firm-fixed effects, respectively. Panel B reports the results of the impact of executive pay limits on the adjustment speed of corporate cash holdings. The dependent variable is Δ*Cash*, the one-year change in cash holdings. The key variable of interest is $SOE \times Post \times (Cash_t^* - Cash_{t-1})$, where $Cash_t^* - Cash_{t-1}$ is the deviation from the target cash ratio at the beginning of the year. Column (1) displays results for the full sample. Columns (2) and (3) show subsample results, divided by excess cash and cash shortfall. Observations above (below) the median of the change in cash holdings ($Cash_t^* - Cash_{t-1}$) are categorized as excess cash (shortfall). Panel C reports the results of the impact of executive pay limits 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 limits on corporate cash holdings

Dependent variable=	Cash	Cash	Cash
	(1)	(2)	(3)
Treat × Post	0.122***	0.127***	0.115***
	(12.975)	(13.396)	(11.820)
Treat	-0.045***	-0.054***	
	(-4.478)	(-5.331)	
Post	-0.121***		
	(-14.776)		
Size	-0.015***	-0.012***	0.009
	(-3.959)	(-3.228)	(1.054)
Leverage	-0.459***	-0.467***	-0.524***
	(-19.192)	(-19.415)	(-18.113)
Largest shareholder stake	0.093***	0.094***	0.064
	(3.711)	(3.740)	(1.279)
Cash flow	0.348***	0.383***	0.430***
	(8.583)	(9.430)	(12.423)
ROA	0.323***	0.260***	0.038
	(6.680)	(5.265)	(0.870)
Board Size	-0.007	0.004	0.014
	(-0.622)	(0.386)	(1.411)
Independent director ratio	-0.102***	-0.049	-0.095***
	(-2.811)	(-1.342)	(-3.136)
Firm age	-0.063***	-0.039***	-0.467***
	(-4.883)	(-2.887)	(-7.936)

Constant	0.993***	0.763***	1.526***
	(12.312)	(8.998)	(6.307)
Firm FE	No	No	Yes
Year FE	No	Yes	Yes
Industry FE	Yes	Yes	No
Adj R^2	0.264	0.281	0.565
N	18,825	18,825	18,825

Panel B. Impact of executive pay limits 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 × ($Cash_t^*$–$Cash_{t-1}$)	-0.167***	-0.099***	0.017
	(-5.702)	(-2.956)	(0.773)
$Cash_t^*$–$Cash_{t-1}$	0.491***	0.968***	0.894***
	(27.467)	(41.156)	(65.320)
Treat×Post	0.010*	0.001	-0.012***
	(1.867)	(0.232)	(-2.955)
Treat×($Cash_t^*$–$Cash_{t-1}$)	0.055*	-0.019	-0.003
	(1.704)	(-0.549)	(-0.133)
Post×($Cash_t^*$–$Cash_{t-1}$)	0.255***	0.097***	0.027*
	(11.300)	(3.583)	(1.960)
Constant	-0.019***	0.094***	-0.097***
	(-15.196)	(43.763)	(-134.747)
Firm and Year FE	Yes	Yes	Yes
Adj R^2	0.446	0.761	0.866
N	16,387	8,040	8,347

Panel C. Marginal value of cash

Dependent variable=	r-R	r-R	r-R
	(1)	(2)	(3)
Treat×Post×ΔCash	-0.284**	-0.332***	-0.263*
	(-2.331)	(-2.694)	(-1.944)
Post×Cash	0.059	0.075	0.011
	(1.048)	(1.358)	(0.180)
Treat×ΔCash	0.072	0.089	-0.009
	(0.817)	(1.006)	(-0.088)
Treat×Post	-0.061***	-0.062***	-0.056***
	(-5.253)	(-5.325)	(-4.504)
Treat	0.017*	0.017*	
	(1.804)	(1.779)	
Post	0.012		
	(1.539)		
ΔCash	0.196***	0.181***	0.289***
	(2.986)	(2.779)	(3.923)
ΔEarnings	0.558***	0.579***	0.524***
	(12.991)	(13.482)	(12.015)
ΔNet Assets	0.288***	0.277***	0.293***
	(19.844)	(19.366)	(18.290)
ΔInterest Expenses	-0.488*	-0.347	-0.109
	(-1.799)	(-1.302)	(-0.398)
$Cash_{t-1}$	0.033**	0.035**	0.062***
	(2.216)	(2.401)	(2.952)
Net Financing	-0.130***	-0.124***	-0.140***
	(-5.722)	(-5.535)	(-5.640)
Leverage	-0.055***	-0.052***	-0.105***
	(-17.153)	(-16.278)	(-17.381)
Leverage×ΔCash	0.341***	0.320***	0.325***
	(7.534)	(7.147)	(6.769)
$Cash_{t-1}$×ΔCash	-0.174***	-0.157***	-0.195***
	(-3.025)	(-2.753)	(-3.153)
ΔDividends	1.430***	1.475***	1.524***
	(7.033)	(7.315)	(7.285)
Constant	0.045***	0.052***	0.090***
	(5.900)	(11.072)	(14.045)
Firm FE	No	No	Yes
Year FE	No	Yes	Yes
Industry FE	Yes	Yes	No
Adj R^2	0.121	0.131	0.096
N	16,323	16,323	16,323

Table 7. Executive pay limits and firm performance

This table reports the regression results on the impact of the executive pay limit on firm valuation and productivity. The dependent variables are *Tobin's Q* in Column (1) and total factor productivity (*TFP*) in Column (2), respectively. Definitions for all other variables are provided in the Appendix. All regressions control for firm and year-fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * stand for statistical significance at the 1%, 5% and 10% level, respectively.

Dependent variable=	Tobin's Q	TFP
	(1)	(2)
Treat×Post	-0.327***	-0.199***
	(-8.708)	(-8.818)
Leverage	-0.209	0.694***
	(-1.626)	(8.740)
Largest shareholder stake	-0.529***	0.356**
	(-2.843)	(2.431)
Cash flow	1.112***	0.594***
	(7.482)	(7.260)
Board size	-0.056	0.094***
	(-1.270)	(3.880)
Independent director ratio	0.463***	-0.041
	(3.853)	(-0.627)
Firm age	0.712***	0.137
	(4.153)	(1.226)
Constant	0.261	15.876***
	(0.535)	(49.808)
Firm and year FE	Yes	Yes
Adj R^2	0.633	0.883
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 and 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	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)
Treat× Post×Old CEO	-0.031***	-0.005	-0.153***	-0.096***	-0.020***
	(-2.591)	(-0.419)	(-5.263)	(-2.952)	(-6.091)
Post×Old CEO	0.001	-0.009	-0.030**	-0.004	0.009***
	(0.222)	(-1.247)	(-1.972)	(-0.234)	(4.133)
Treat× Post	-0.008	-0.015*	-0.036**	-0.017	-0.002
	(-1.159)	(-1.871)	(-2.499)	(-0.773)	(-0.727)
Controls	Yes	Yes	Yes	Yes	Yes
Firm and Year FE	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.130	0.256	0.123	0.478	0.453
N	18,194	13,958	18,194	18,194	18,184

Panel B. The role of industry competition

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)
Treat× Post×Competitive industries	-0.025**	-0.034***	-0.183***	-0.111***	-0.018***
	(-1.977)	(-2.878)	(-5.982)	(-3.328)	(-5.417)
Post×Competitive industries	0.000	0.003	-0.031**	0.002	0.005**
	(0.005)	(0.353)	(-2.080)	(0.142)	(2.140)
Treat× Post	-0.011	-0.002	-0.038**	-0.015	-0.002
	(-1.522)	(-0.240)	(-2.574)	(-0.678)	(-0.779)
Controls	Yes	Yes	Yes	Yes	Yes
Firm and Year FE	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.129	0.257	0.124	0.482	0.454
N	18,524	14,203	18,524	18,524	18,513

Panel C. The role of governance quality

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)
Treat× Post×Weak governance	-0.020*	-0.025**	-0.203***	-0.092***	-0.002
	(-1.666)	(-2.150)	(-6.719)	(-2.835)	(-0.607)
Post× Weak governance	-0.007	-0.013*	-0.039***	0.005	-0.003
	(-1.011)	(-1.842)	(-2.642)	(0.322)	(-1.159)
Treat× Post	-0.014	-0.008	-0.030**	-0.022	-0.010***
	(-1.592)	(-0.975)	(-2.062)	(-0.931)	(-4.295)
Controls	Yes	Yes	Yes	Yes	Yes
Firm and Year FE	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.131	0.255	0.128	0.476	0.450
N	17,827	13,595	17,827	17,827	17,816

Table 9. Executive pay limit and CEO turnover

This table reports the effects of the executive pay limit regulation on CEO turnover. In Columns (1) and (2), the dependent variable is a dummy equal to one if the CEO voluntarily leaves the firm due to resignation, term expiration, or personal reasons based on CSMAR classification (Nanda et al., 2024). In Columns (3) and (4), the dependent variable is a dummy equal to one if the CEO was replaced despite good performance (Lin et al., 2022), irrespective of their CSMAR classification. Firm performance is proxied by the prior year's ROA, with "good performance" defined as having an ROA above the industry-adjusted median. Definitions for all other variables are provided in the Appendix. All regressions control for firm and year-fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * stand for statistical significance at the 1%, 5% and 10% level, 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	0.587***	0.019***	0.637***	0.017***
	(3.323)	(2.623)	(3.207)	(2.832)
Size	-0.148	-0.011*	0.017	0.002
	(-1.532)	(-1.833)	(0.136)	(0.478)
Leverage	0.048	0.010	-1.175**	-0.034**
	(0.126)	(0.426)	(-2.429)	(-2.165)
Largest shareholder stake	0.565	0.045	2.210***	0.100***
	(0.893)	(1.096)	(2.655)	(3.099)
Cash flow	1.267**	0.070**	0.604	0.013
	(2.124)	(2.102)	(0.777)	(0.498)
ROA	-0.961	-0.059	0.519	0.026
	(-1.328)	(-1.226)	(0.469)	(0.776)
Board size	3.925***	0.246***	3.480***	0.139***
	(16.587)	(17.062)	(12.438)	(12.537)
Independent director	-4.360***	-0.250***	-5.977***	-0.219***
ratio	(-6.200)	(-7.534)	(-7.201)	(-7.894)
Firm age	1.764**	0.102***	0.262	0.028
	(2.281)	(3.329)	(0.312)	(1.126)
CEO age	-0.864**	-0.073***	-0.942**	-0.035*
	(-2.226)	(-2.761)	(-1.976)	(-1.699)
CEO female(0/1)	0.094	0.011	0.589**	0.027**
	(0.467)	(0.730)	(2.271)	(2.001)
CEO dual(0/1)	0.617***	0.042***	0.899***	0.033***
	(4.491)	(4.828)	(5.304)	(4.743)
Ln(GDP)	-0.218	-0.007	-0.183	-0.003
	(-0.703)	(-0.503)	(-0.492)	(-0.262)
Ln(Population)	0.076	0.010	0.514	0.016
	(0.208)	(0.508)	(1.137)	(0.975)

Ln(Employment)	0.361	0.016*	0.045	0.001
	(1.557)	(1.650)	(0.142)	(0.134)
Fiscal pressure	1.450*	0.054	-0.676	-0.034
	(1.774)	(1.151)	(-0.899)	(-0.859)
Constant		-0.219		-0.275
		(-0.798)		(-1.330)
Firm and Year FE	Yes	Yes	Yes	Yes
Adj R^2/ Pseudo R^2	0.171	0.086	0.144	0.046
N	5,922	17,978	4,291	17,978

Table 10. Mitigating the impact of ex-ante CEO turnover risk

This table isolates the policy's effect by controlling for potential CEO transitions. We use a matched subsample of firms to compare the likelihood of CEO turnover following the policy implementation. The matching procedure ensures that the treated group (firms experiencing CEO turnover) and the control group (firms without turnover) are comparable in terms of their ex-ante turnover risk. Panel A estimates turnover likelihood using the methodology from Nanda et al. (2024), while Panel B uses the approach from Lin et al. (2022). Matching details are presented in Appendix IA.10. Definitions for all other variables are provided in the Appendix. All regressions control for firm and year-fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * stand for 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	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)
Treat×Post	-0.023***	-0.015**	-0.105***	-0.066***	-0.009***
	(-3.065)	(-2.321)	(-6.421)	(-3.515)	(-4.927)
Controls	Yes	Yes	Yes	Yes	Yes
Firm and Year FE	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.102	0.265	0.114	0.468	0.452
N	14,516	11,130	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	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)
Treat×Post	-0.020***	-0.016**	-0.105***	-0.066***	-0.010***
	(-2.778)	(-2.403)	(-6.253)	(-3.480)	(-5.337)
Controls	Yes	Yes	Yes	Yes	Yes
Firm and Year FE	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.108	0.258	0.114	0.459	0.453
N	14,423	11,054	14,423	14,423	14,415

Table 11. Executive pay limits 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. All regressions include firm and year-fixed effects. Definitions for all other variables are provided in the Appendix. All regressions control for firm and year-fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * stand for statistical significance at the 1%, 5% and 10% level, respectively.

Dependent variable=	Ln(Compensation)	Ln(Compensation)
	(1)	(2)
Treat×Post×Performance	-0.756*	-0.327**
	(-1.811)	(-2.422)
Post×Performance	-0.442*	-0.114
	(-1.940)	(-1.598)
Treat×Performance	1.746***	0.604***
	(4.730)	(4.730)
Treat×Post	-0.210***	-0.212***
	(-7.936)	(-8.706)
Size	0.202***	0.205***
	(10.774)	(10.786)
Leverage	-0.058	-0.120*
	(-0.891)	(-1.873)
Largest shareholder stake	-0.003	0.017
	(-0.025)	(0.145)
Cash flow	0.052	0.152*
	(0.662)	(1.928)
ROA	1.011***	
	(4.698)	
ROS		0.210***
		(3.187)
Board size	-0.189***	-0.192***
	(-6.846)	(-6.981)
Independent director ratio	0.435***	0.437***
	(5.588)	(5.599)
Firm age	-0.028	-0.026
	(-0.256)	(-0.234)
CEO age	0.460***	0.462***
	(6.891)	(6.905)
CEO female(0/1)	-0.058	-0.054
	(-1.373)	(-1.272)
CEO dual(0/1)	0.039*	0.041*

	(1.692)	(1.760)
Constant	7.300***	7.273***
	(12.592)	(12.432)
Firm and year FE	Yes	Yes
Adj R^2	0.655	0.653
N	17,889	17,888

Table 12. Executive pay limits and rent-seeking behavior

This table reports the effect of executive pay limits 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 control for firm and year-fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * stand for statistical significance at the 1%, 5% and 10% level, respectively.

Dependent variable=	Perk	Tunneling
	(1)	(2)
Treat×Post	-0.001***	-0.441***
	(-4.304)	(-11.485)
Size	-0.002***	0.915***
	(-7.749)	(26.335)
Leverage	-0.003**	2.625***
	(-2.289)	(20.462)
Largest shareholder stake	-0.003**	-0.471**
	(-2.094)	(-2.153)
Cash flow	-0.003***	0.277**
	(-3.032)	(2.049)
ROA	-0.015***	-0.146
	(-7.934)	(-0.776)
Board size	0.000	0.100**
	(0.276)	(2.323)
Independent director ratio	-0.001	-0.163
	(-1.271)	(-1.220)
Firm age	-0.001	1.029***
	(-0.577)	(5.653)
CEO age	-0.003***	0.173*
	(-3.074)	(1.715)
CEO female(0/1)	0.000	-0.013
	(0.522)	(-0.234)
CEO dual(0/1)	0.001***	-0.019
	(2.796)	(-0.546)
Constant	0.077***	-6.811***
	(9.210)	(-6.782)
Firm and year FE	Yes	Yes
Adj R^2	0.726	0.853
N	17,320	18,330

Table 13. Executive pay limits and diverted objectives

This table reports regression results on the impact of executive pay limits 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. Firm and year fixed effects are included, 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	1.091***	0.423***
	(3.581)	(3.347)
Size	1.047***	1.287***
	(4.768)	(13.338)
Leverage	-1.162	-4.155***
	(-1.437)	(-11.290)
Largest shareholder stake	1.001	2.777***
	(0.864)	(5.074)
Cash flow	-0.203	2.120***
	(-0.232)	(4.358)
ROA	2.296*	62.924***
	(1.864)	(62.840)
Board size	-0.222	-0.442***
	(-0.871)	(-3.088)
Independent director ratio	0.268	0.849**
	(0.334)	(2.016)
Firm age	1.328	-3.318***
	(0.900)	(-5.668)
Constant	-7.319	-6.473**
	(-1.101)	(-2.420)
Firm and year FE	Yes	Yes
Adj R^2	0.766	0.806
N	7,639	18,801

Table 14. Executive pay limits and labor-adjacent outcomes

This table reports regression results on the impact of executive pay limits on labor-adjacent outcomes, including *Employment* (log number of employees), *Labor productivity* (sales (in millions)/ number of employee), and *Wage-bill intensity* (total salary expenses / total assets). Definitions of all variables are provided in the Appendix. Regressions include firm and province×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=	Employment	Labor productivity	Wage-bill intensity
	(1)	(2)	(3)
Treat×Post	0.087***	-0.214***	-0.004***
	(3.750)	(-3.246)	(-2.985)
Size	0.635***	0.348***	-0.016***
	(25.705)	(5.231)	(-12.423)
Leverage	0.188**	0.425**	0.013***
	(2.302)	(2.077)	(3.363)
Largest shareholder stake	0.013	0.349	0.013**
	(0.095)	(0.830)	(1.962)
Cash flow	0.227***	0.836***	0.024***
	(3.394)	(2.920)	(7.568)
ROA	0.049	1.823***	-0.017**
	(0.426)	(5.484)	(-2.557)
Board size	-0.001	0.113*	0.001
	(-0.051)	(1.757)	(1.020)
Independent director ratio	-0.061	0.224	0.001
	(-1.006)	(1.118)	(0.366)
Firm age	0.250**	-0.161	0.010*
	(2.284)	(-0.451)	(1.760)
Constant	-7.218***	-6.381***	0.385***
	(-11.715)	(-3.825)	(11.789)
Firm and Province×Year FE	Yes	Yes	Yes
Adj R^2	0.925	0.746	0.842
N	18,817	18,817	18,401

Internet Appendix IA.1. Estimating the optimal cash holdings

This table presents the estimation of the optimal cash holdings (*Cash**). This step is based on the work of Opler et al. (1999) and Megginson et al. (2014). The dependent variable is corporate cash holdings (*Cash*). All other variables are defined in the Appendix. Year and Firm effects are controlled. The t-statistics are presented in parentheses, with standard errors clustered at the firm level. ***, **, * indicates significance at 1%, 5%, and 10%, respectively.

Dependent variable =	Cash
	(1)
Size	-0.003
	(-0.371)
Leverage	-0.545***
	(-18.458)
Largest shareholder stake	0.123**
	(2.474)
Cash flow	0.418***
	(12.032)
ROA	0.066
	(1.528)
Board size	0.016
	(1.523)
Independent director ratio	-0.109***
	(-3.550)
Firm age	-0.549***
	(-9.190)
Constant	2.026***
	(8.534)
Firm and Year FE	Yes
Adj R^2	0.557
N	18,825

Appendix IA.2. The effect on central and local SOEs

This table examines whether the estimated effects of the executive pay limit 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 and 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	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)
Central SOE× Post	-0.034***	-0.005	-0.143***	-0.075*	-0.010***
	(-2.755)	(-0.489)	(-5.148)	(-1.923)	(-3.872)
Local SOE× Post	-0.017***	-0.021***	-0.101***	-0.059***	-0.010***
	(-2.699)	(-3.389)	(-6.187)	(-3.679)	(-5.164)
Size	0.014***	0.005	0.015*	0.051***	0.005***
	(3.100)	(1.090)	(1.695)	(4.733)	(3.589)
Leverage	-0.013	-0.040**	0.008	0.035	-0.005
	(-0.836)	(-2.442)	(0.232)	(0.944)	(-0.998)
Largest shareholder stake	-0.045	-0.029	-0.017	-0.107	0.028***
	(-1.511)	(-1.126)	(-0.317)	(-1.536)	(3.468)
Cash flow	-0.002	-0.001	0.031	-0.074	0.011**
	(-0.070)	(-0.024)	(0.662)	(-1.326)	(2.113)
ROA	-0.020	-0.051*	0.101*	-0.057	0.037***
	(-0.520)	(-1.702)	(1.698)	(-0.733)	(5.227)
Board size	-0.004	-0.011*	-0.004	0.028	-0.004***
	(-0.445)	(-1.664)	(-0.271)	(1.469)	(-2.590)
Independent director ratio	-0.011	0.049**	0.004	0.066	0.003
	(-0.502)	(2.277)	(0.096)	(1.160)	(0.689)
Firm age	0.029	-0.040	0.085	-0.007	-0.043***
	(0.813)	(-1.195)	(1.564)	(-0.084)	(-5.285)
Constant	-0.333**	0.381***	-0.391	4.274***	0.063*
	(-2.387)	(2.929)	(-1.643)	(12.735)	(1.667)
Firm and Year FE	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.134	0.256	0.114	0.483	0.449
N	18,825	14,406	18,825	18,825	18,814

Appendix IA.3. The role of the pay gap in SOE

This table presents a robustness check examining whether the estimated effects of the executive pay limit regulation differ by the size of a firm's executive-employee 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. Every regression includes firm and 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	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)
High pay gap SOE× Post	-0.021***	-0.026***	-0.108***	-0.055**	-0.008***
	(-2.913)	(-3.593)	(-5.773)	(-2.555)	(-3.875)
Low pay gap SOE× Post	-0.028***	-0.010	-0.124***	-0.065**	-0.013***
	(-2.877)	(-1.297)	(-5.578)	(-2.530)	(-5.929)
Size	0.015***	0.003	0.015	0.048***	0.005***
	(3.130)	(0.662)	(1.634)	(4.180)	(3.263)
Leverage	-0.015	-0.041**	0.018	0.042	-0.006
	(-0.920)	(-2.348)	(0.490)	(1.063)	(-1.187)
Largest shareholder stake	-0.052	-0.033	-0.009	-0.129*	0.026***
	(-1.639)	(-1.226)	(-0.156)	(-1.708)	(3.148)
Cash flow	-0.002	0.010	0.021	-0.076	0.008
	(-0.095)	(0.426)	(0.437)	(-1.264)	(1.577)
ROA	-0.028	-0.045	0.100	-0.042	0.041***
	(-0.672)	(-1.442)	(1.595)	(-0.498)	(5.418)
Board size	-0.003	-0.009	-0.010	0.030	-0.004**
	(-0.353)	(-1.206)	(-0.593)	(1.465)	(-2.327)
Independent director ratio	-0.000	0.053**	0.045	0.072	0.003
	(-0.011)	(2.381)	(0.904)	(1.157)	(0.490)
Firm age	0.016	-0.052	0.081	-0.008	-0.043***
	(0.418)	(-1.495)	(1.479)	(-0.084)	(-5.120)
Constant	-0.321**	0.446***	-0.389	4.324***	0.068*
	(-2.162)	(3.321)	(-1.565)	(11.955)	(1.725)
Firm and Year FE	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.128	0.253	0.115	0.474	0.446
N	17,342	13,273	17,342	17,342	17,332

Appendix IA.4. The role of SOE political connections

This table investigates the influence of political connections on the relationship between executive pay limits 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. Year and firm fixed effects are controlled. The t-statistics are presented in parentheses, with standard errors clustered at the firm level. ***, **, * indicates significance at 1%, 5%, and 10%, respectively.

Dependent variable =	Ln (1+ Overtime Keywords)	Overtime Ratio	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)
PC SOE× Post	-0.017	0.014	-0.097***	-0.081*	-0.014***
	(-1.002)	(1.068)	(-3.141)	(-1.897)	(-3.777)
Non-PC SOE× Post	-0.023***	-0.023***	-0.115***	-0.061***	-0.010***
	(-3.505)	(-3.731)	(-7.109)	(-3.426)	(-5.689)
Size	0.015***	0.004	0.016*	0.049***	0.004***
	(3.193)	(0.866)	(1.818)	(4.484)	(3.299)
Leverage	-0.015	-0.040**	0.008	0.036	-0.006
	(-0.997)	(-2.382)	(0.228)	(0.961)	(-1.309)
Largest shareholder stake	-0.053*	-0.034	-0.024	-0.118	0.028***
	(-1.755)	(-1.299)	(-0.429)	(-1.625)	(3.442)
Cash flow	-0.002	0.003	0.023	-0.075	0.011**
	(-0.084)	(0.125)	(0.480)	(-1.319)	(2.082)
ROA	-0.019	-0.047	0.094	-0.049	0.040***
	(-0.486)	(-1.531)	(1.543)	(-0.613)	(5.487)
Board size	-0.003	-0.010	-0.007	0.029	-0.004***
	(-0.321)	(-1.399)	(-0.449)	(1.476)	(-2.608)
Independent director ratio	-0.012	0.045**	0.015	0.065	0.004
	(-0.526)	(2.099)	(0.319)	(1.130)	(0.711)
Firm age	0.029	-0.040	0.079	-0.015	-0.042***
	(0.783)	(-1.191)	(1.461)	(-0.163)	(-5.119)
Constant	-0.345**	0.400***	-0.401*	4.328***	0.066*
	(-2.425)	(3.040)	(-1.653)	(12.477)	(1.745)
Firm and Year FE	Yes	Yes	Yes	Yes	Yes
Adj R^2	0.129	0.257	0.115	0.480	0.452
N	18,524	14,203	18,524	18,524	18,513

Appendix IA.5. 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 (3) report results from Poisson regressions, while Column (2) presents results from a logit model. All variables are defined in the Appendix. Firm and year fixed effects are included in all specifications. 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.

	Poisson	Logit	Poisson
Dependent variable =	Ln(1+ Overtime Keywords)	Unrelated Acquisition(0/1)	Risk Tone
	(1)	(2)	(3)
Treat× Post	-0.786***	-1.063***	-0.011***
	(-3.224)	(-7.942)	(-3.547)
Size	0.504***	0.119	0.009***
	(3.760)	(1.571)	(4.542)
Leverage	-0.383	0.111	0.006
	(-0.682)	(0.357)	(0.815)
Largest shareholder stake	-1.538	-0.239	-0.018
	(-1.584)	(-0.476)	(-1.381)
Cash flow	-0.289	0.398	-0.015
	(-0.313)	(0.966)	(-1.479)
ROA	-0.165	0.628	-0.010
	(-0.166)	(1.104)	(-0.680)
Board size	-0.146	-0.027	0.005
	(-0.514)	(-0.186)	(1.511)
Independent director ratio	-0.232	0.035	0.012
	(-0.273)	(0.087)	(1.179)
Firm age	1.502*	0.760	-0.005
	(1.653)	(1.547)	(-0.274)
Firm and Year FE	Yes	Yes	Yes
Pseudo R^2	0.142	0.029	0.007
N	3,315	11,590	18,825

Appendix IA.6. Robustness tests using Double Machine Learning (DML)

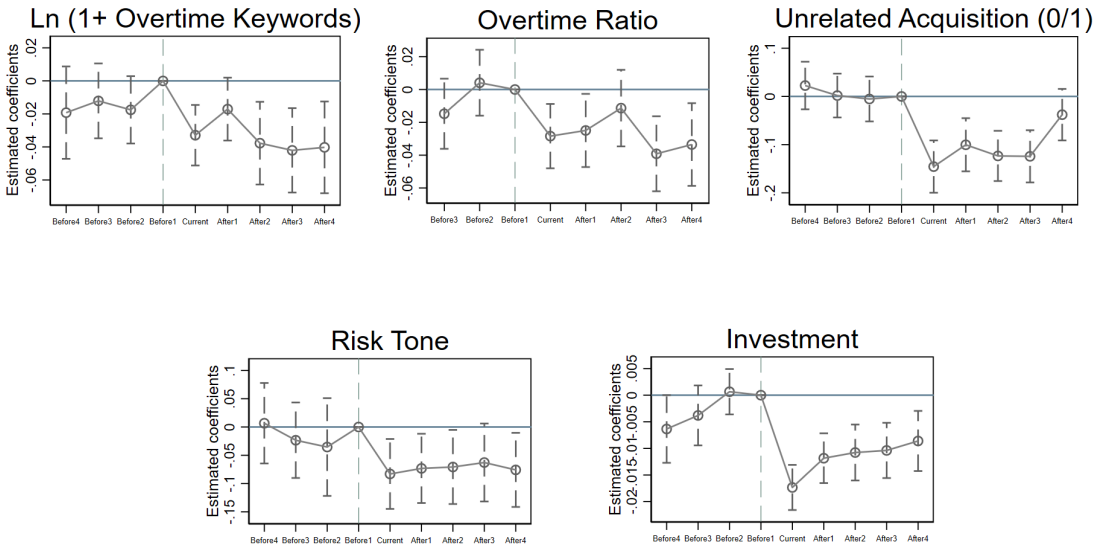
This table reports robustness test results using the DML framework with Lasso as the machine learning method. Columns (1) to (5) correspond to the five main outcome variables used in the baseline regressions. All variables are defined in the Appendix. All regressions include firm and year fixed effects. 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	Unrelated Acquisition (0/1)	Risk Tone	Investment
	(1)	(2)	(3)	(4)	(5)
Treat× Post	-0.022	-0.017***	-0.110***	-0.067***	-0.010***
	(-4.630)	(-3.782)	(-11.355)	(-5.474)	(-9.244)
Controls	Yes	Yes	Yes	Yes	Yes
Firm and Year FE	Yes	Yes	Yes	Yes	Yes
N	18,825	14,406	18,825	18,825	18,814

Appendix IA.7. PSM between treated and control firms

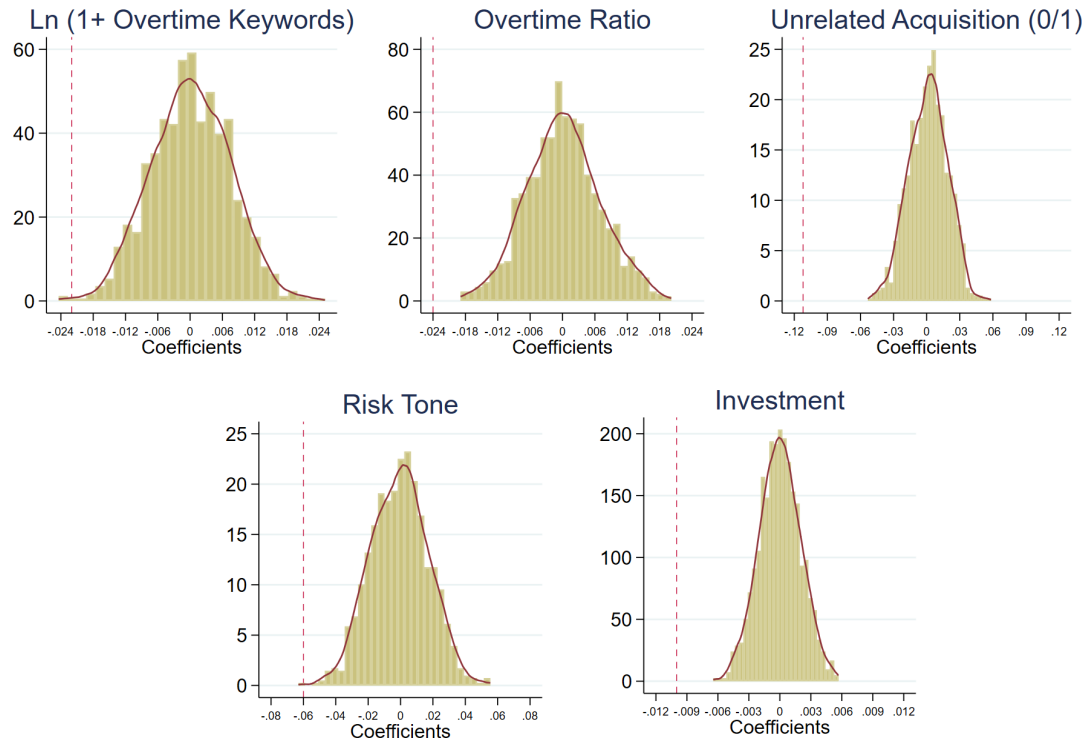
To construct a credible counterfactual group, we use propensity score matching (PSM) to pair each treated firm, defined as an SOE in 2014, with a non-SOE. We estimate propensity scores using a logit model that includes all baseline covariates. We implement 1 to 3 nearest-neighbor matching with replacement and impose a caliper of 0.25 to ensure high-quality matches. This table presents the univariate results between the treated and control groups. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * stand for statistical significance at the 1%, 5% and 10% level, respectively.

		Treatment firms	Control firms		
Variables		Mean	Mean	Mean-Diff	T value
Size	U	22.763	21.610	1.153***	22.47
	M	22.763	22.666	0.097	1.45
Leverage	U	0.526	0.380	0.146***	16.32
	M	0.526	0.539	-0.013	-1.26
Largest shareholder stake	U	0.407	0.335	0.072***	11.08
	M	0.407	0.407	0.000	0.01
Cash flow	U	0.038	0.040	-0.002	-0.71
	M	0.038	0.034	0.004	1.07
ROA	U	0.027	0.042	-0.015***	-6.46
	M	0.027	0.025	0.002	0.61
Board size	U	2.406	2.272	0.134***	12.04
	M	2.406	2.383	0.023*	1.93
Independent director ratio	U	0.390	0.405	-0.015***	-4.37
	M	0.390	0.385	0.005	1.3
Firm age	U	2.847	2.707	0.140***	9.69
	M	2.847	2.890	-0.043**	-2.85



Appendix IA.8. Dynamic effects using the PSM-matched sample

Note: This figure illustrates the time-varying effects of the executive pay limit regulation on five direct behavioral outcomes using the PSM 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.9. Placebo tests using the PSM-matched sample

Note: This figure displays the kernel density of 1,000 placebo estimates for the coefficient on *Treat_F* × *Post*, based on the PSM-matched sample. The placebo estimates are generated by randomly reassigning the treatment status across firms. Placebo tests are conducted for all five 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.10. PSM on the likelihood of CEO turnover

We estimate propensity scores on the likelihood of CEO turnover using a probit model for both measures in Nanda et al. (2024) and Lin et al. (2022), using the sample of firms from 2011 to 2014 to estimate the likelihood of CEO turnover in 2015. We implement 1 to 3 nearest-neighbor matching with replacement and impose a caliper of 0.25 to ensure high-quality matches. Panel A reports the results of the estimation, and Panel B presents the univariate results between the treated and control groups. All regressions include industry and year fixed effects. The t-statistics are based on standard errors adjusted for heteroscedasticity and clustered at the firm level. ***, **, and * stand for 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)
LnEmployment	-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

		Treatment firms	Control firms		
Variables		Mean	Mean	Mean-Diff	T value
$CEO\ turnover(\widehat{Nanda\ et\ al.,2024})_{2015}$	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