Labor Reallocation within Business Groups: Internal Labor Market Responses to Labor Cost Shocks

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Abstract

We examine how business groups reallocate labor across affiliates in response to labor cost shocks. Using data on Chinese listed firms and their subsidiaries, we find that labor cost reductions trigger significant recruitment expansions in affected affiliates and corresponding contractions in other group affiliates, reflecting internal labor market reallocation. This reallocation occurs predominantly among horizontally linked affiliates with substitutable relationships, while vertically linked affiliates show minimal adjustments. Transferred positions concentrate in operational and managerial roles requiring lower skills. Reallocation intensity is stronger in state-owned enterprises, labor-efficient groups, and regions with lower labor costs, faster growth, and weaker institutions. These adjustments also involve cross-regional reallocation, with employees hired by affiliates in different locations. Overall, internal labor markets enable business groups to absorb regional shocks and enhance organizational flexibility.

Keywords: Business groups, internal labor markets, labor reallocation, labor cost shocks, affiliate linkages

JEL Classification: J23, J63, L22, L23, M51

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1 Introduction

The rise of business groups offers a new perspective for understanding how firms respond to external economic shocks (Khanna and Palepu, 2000; Khanna and Yafeh, 2007). Traditionally, studies on labor cost shocks have primarily focused on individual firms, exploring how changes in labor costs influence recruitment, production, or performance(Draca et al., 2011; Hau et al., 2020; Geng et al., 2022). However, compared to standalone firms, business groups create opportunities for resource reallocation among member firms through internal markets, thereby alleviating frictions present in external markets. These frictions include high recruitment costs, information asymmetries, and other inefficiencies(Dai et al., 2024). The flexible allocation of production factors, credit resources, and innovation capabilities within business groups provides them with a significant advantage in mitigating external shocks (Bartram et al., 2022; Biermann and Huber, 2024; Giroud et al., 2024b). One explanation is that business groups construct internal factor markets among affiliated firms, effectively reducing frictions in external markets.

Building on this foundation, this study examines how business groups respond to declining labor costs by adjusting their internal labor markets (ILMs). ILMs are a critical mechanism within business groups that facilitate the reallocation of labor resources among member firms. By reducing external recruitment costs and accelerating labor reallocation, ILMs enable member firms to adapt more quickly to changes in the external environment(Tate and Yang, 2015; Giroud and Mueller, 2019; Cestone et al., 2024). This paper aims to uncover how business groups utilize ILMs to optimize labor resource allocation, analyze their response mechanisms to labor cost reductions, and quantify the impact of internal labor mobility on the overall performance of business groups.

We use China's nationwide reduction in employee social security contribution rates as a quasi-natural experiment to study how internal labor markets respond to shocks from declining labor costs. Specifically, most provinces reduced the social security contribution rate from 19% or 20% of the wage base to 16%, with Guangdong and Zhejiang provinces being the only exceptions. We prove that this reform reduce the

proportion of social security contributions that companies have to pay in relation to the overall labor costs in the treated province. We find that when the contribution rates in other provinces decreased, the number of recruitments by group-affiliated companies in these areas increased significantly compared to provinces where the rates remained unchanged.

At the same time, we observe that for business groups with subsidiaries in both treated and untreated provinces, the increase in recruitment is more pronounced in cost-reduced regions and accompanied by contractions in affiliates in controlled provinces. This suggests that when labor costs decrease in one region, the group reduces recruitment in other regions to increase recruitment in the cost-reduced region, indicating the existence of internal labor market allocation.

Furthermore, we distinguish between different collaboration modes of affiliates, namely horizontal and vertical linkages. We find that when the group has only vertically linked affiliates in other regions, the recruitment in the cost-reduced regions is not significantly affected. However, when the group has horizontally linked affiliates in other regions, recruitment in the cost-reduced regions increases significantly. This indicates that horizontal linkages, due to business homogeneity, play a more prominent role in internal labor market allocation.

Additionally, we find that the labor force capable of horizontal transfer is more concentrated in part-time positions, with job types primarily in production and management, and education levels mostly below undergraduate. At the group level, we find that state-owned enterprises (SOEs), those with higher labor intensity, and those without prior labor lawsuits exhibit stronger internal labor reallocation effects. This suggests that SOEs play a stabilizing role in internal labor market reallocation. At the city level, labor reallocation is more pronounced in regions with lower labor costs and faster economic growth, consistent with the trend of labor shifting toward locally more efficient but institutionally less developed markets.

Regarding cross-regional employment, we find that due to Guangdong and Zhejiang not reducing employee social security contribution rates, the number of recruitments by group-affiliated companies in other regions increased significantly. We further find

evidence of cross-regional hiring within business groups. When the parent firm is also located in a cost-reduced province, some affiliates hire workers whose actual workplaces are in Guangdong or Zhejiang. This indicates that groups may adjust their internal labor markets by recruiting employees from low-cost regions to work for subsidiaries in high-cost regions, demonstrating the existence of cross-regional employment. In contrast, when the parent firm is located in cost-unreduced provinces, cross-regional hiring is not observed. Finally, our study shows that companies more exposed to the policy exhibit faster growth, greater increases in enterprise value, and lower probabilities of incurring losses.

Our contributions are manifested in the following aspects:

First, we contribute to the literature on internal labor markets (ILMs), which studies how multi-unit firms and business groups reallocate workers across affiliates in response to shocks (Tate and Yang, 2015; Giroud and Mueller, 2015, 2019; Silva, 2021; Huneeus et al., 2021; Bianchi et al., 2023; Cestone et al., 2024). Early work by Tate and Yang (2015) and Giroud and Mueller (2015) demonstrates how local demand shocks, as captured by regional housing price declines, propagate within firms through internal networks when financial constraints restrict access to external capital. In their setting, financially constrained firms optimally spread cash-flow shocks across establishments, leading to employment spillovers across regions. At the business group level, Cestone et al. (2024) find that competitor exit enables rapid internal redeployment of labor, while Huneeus et al. (2021) document that affiliated firms adjust worker flows in response to international trade shocks. Complementary research has examined the consequences of such internal reallocations (Silva (2021)). For instance, Bianchi et al. (2023) exploit a pension reform shock in Italy to study how delayed retirements impede internal career progression, rather than prompting new labor reallocation. While existing research primarily focuses on external changes in investment opportunities—such as competitor exit or increased investment prospects—our paper extends this literature by examining how labor force changes themselves affect internal labor markets. Specifically, we focus on shocks that directly impact labor costs, providing new insights into the mechanisms of internal labor reallocation.

Second, we contribute to the growing literature on how firms respond to rising labor costs or enhanced labor protection. Prior studies have explored various aspects of firm behavior, including corporate growth (Bai et al., 2020), profitability (Draca et al., 2011), capital structure (Serfling, 2016), accelerated labor substitution (Hau et al., 2020), process innovation (Bena et al., 2022), and corporate investment (Geng et al., 2022; Wang and Yung, 2025). However, these studies primarily focus on firm-level responses, leaving the dynamics within business groups largely unexplored.

Third, our study makes a broader contribution to the literature on internal markets. Seminal studies provide foundational insights into the existence, efficiency, and governance of internal capital markets (Gertner et al., 1994; Stein, 1997; Lamont, 1997; Shin and Stulz, 1998) . Building on this framework, recent research has primarily focused on topics such as internal pollution transfer (Bisetti et al., 2022; Bartram et al., 2022; Chen et al., 2025), knowledge diffusion in internal innovation and production efficiency (Giroud et al., 2024b,a), and the allocation of internal credit resources (Bartram et al., 2022; Basco et al., 2024; Biermann and Huber, 2024). However, the existing literature on internal capital markets often assumes homogeneous relationships among subsidiaries. In contrast, we differentiate between horizontal and vertical relationships, enabling us to identify their heterogeneous effects.

The remainder of this paper is organized as follows: Section 2 describes the institutional background and 3data. Section 4 presents the empirical framework and results. Section 5 concludes.

2 Institutional Background

China's statutory social security contributions, especially the employer share of the basic pension scheme, have long been among the highest in the world and have imposed substantial burdens on enterprises. Before 2015, the combined employer–employee contribution rate for the five major insurances—pension, unemployment, medical, work injury, and maternity—averaged roughly 41 percent. The high rate reflected demographic aging, recognition of unfunded liabilities, and rigid benefit structures, which

together constrained labor demand, formal employment, and wage growth.

Evolution of the Reform. In 2015, as part of the central government's supply-side structural reform agenda of "reducing costs," China began a sequence of contribution-rate reductions (see Figure 3 for a timeline). The first round lowered unemployment, work-injury, and maternity insurance rates by 1.75 percentage points, and the 2016 round extended the reform to the basic pension, allowing provinces to cut employer rates from 20 to 19 percent under certain conditions. By 2017, some provinces—most notably Guangdong and Zhejiang—had already reduced their employer pension contribution rates to 14 percent, creating early regional variation in labor costs.

The most consequential adjustment occurred in 2019, when the State Council issued the *Comprehensive Plan for Reducing Social Security Contribution Rates*, effective May 1, 2019. The plan stipulated that provinces whose employer contribution rate to the basic pension exceeded 16 percent should lower it to 16 percent, while provinces with lower rates were instructed to design transitional arrangements. In practice, most provinces, which had previously charged 19 to 20 percent, reduced their rates to 16 percent—a reduction of 3 to 4 percentage points. In contrast, Guangdong and Zhejiang, having already established 14 percent rates in 2017, maintained these lower rates and did not make further cuts until 2022 and 2023. Figure 4 illustrates this divergence between treatment and control provinces. This heterogeneous implementation of a centrally announced policy created a quasi-natural experiment, characterized by a nationally mandated yet regionally asymmetric adjustment in employer labor costs.

Institutional Features and Identification. An important institutional feature of China's social security system is that contribution rates are determined by the firm's registered location rather than the actual work location of employees. This rule generates within-group variation in labor costs for business groups whose subsidiaries are registered in different provinces. It also distinguishes China from countries such as the United States, where the social security contribution rate is uniform nationwide. The 2019 reform therefore provides a plausibly exogenous, registration-based labor-cost shock that is orthogonal to local labor demand.

We compile province–year data on employer contribution rates to the basic pension

scheme from official policy documents and government announcements. These statutory rates, which vary across provinces and years, form the core treatment variable in our analysis. In practice, an enterprise's total pension contribution burden depends on the statutory rate times the contribution base. The contribution base is linked to its reported payroll and the province's average wage level, which is typically set as the firm's previous-year total wage bill, subject to upper and lower bounds defined as a percentage of the province's average wage, commonly between 60% and 300%. This implies that in provinces with higher average wages, such as Zhejiang and Guangdong, overall pension costs could remain relatively high even without a rate increase, whereas in lower-wage provinces, the 2019 rate reduction translated into a larger decrease in actual pension expenses. The reform's impact on effective labor costs thus depends on both statutory rates and local wage bases.

Advantages for Identification. The resulting institutional setting offers several advantages for identification. First, because contribution rates are determined by firms' registered locations rather than their actual work sites, labor-cost shocks differ across subsidiaries within the same business group. This allows us to identify within-group adjustments to exogenous variation in costs. Second, our job-posting data record the actual workplace location, enabling us to test whether firms reallocate employment across provinces in response to these differential costs. Third, since the reform directly altered employers' statutory contribution obligations without changing local demand conditions or employee wages, it creates a clear source of supply-side variation in labor costs. Finally, the timing of the reform—announced in March 2019 and implemented on May 1, 2019—was largely unexpected and applied uniformly across treated provinces, minimizing concerns about anticipation effects or selective compliance.

Validation of Treatment Effect. To confirm that the reform indeed lowered firms' effective labor costs, we later present a direct test using firm-level data on social-security expenditures. Specifically, we estimate how the share of pension payments in total labor expenses changes after the reform. The results, discussed in Section 4, show that this share declines significantly for firms in treated provinces, confirming that the 2019 reform effectively reduced labor costs and validating our classification of treated and control regions.

3 Data and Descriptive Statistics

Our empirical analysis combines multiple datasets to construct a comprehensive panel of business groups, their affiliates, and recruitment activities. This section describes the data sources, sample construction, key variables, and potential measurement concerns.

3.1 Business Group Structure and Ownership Data

We focus on Chinese business groups in which a publicly listed firm serves as the ultimate controlling parent of legally independent affiliates. These groups are defined by direct or indirect ownership stakes exceeding 50 percent, ensuring that the parent firm exercises effective control over affiliate decisions, including employment policies. Such groups often span multiple regions and industries, providing a natural setting to study internal resource reallocation in response to labor-cost shocks.

To delineate group boundaries, we first identify listed firms and their affiliates using the China Stock Market and Accounting Research Database (CSMAR) Listed Firms' Affiliate Database, which reports detailed ownership links between listed companies and their related entities. We then merge these records with the China Administrative Registration Database (CARD), maintained by the State Administration for Market Regulation. CARD contains comprehensive affiliate-level registration information—including firm name, registration number, establishment date, registered capital, address, industry classification, and shareholder structure—which allows us to extend ownership networks beyond listed entities and map the full organizational scope of each business group. Each firm's registered address is geocoded to the province, prefecture-city, and county levels, enabling us to link firms to regional shocks and local economic indicators with high geographic accuracy.

3.2 Financial and Firm-Level Characteristics

We supplement ownership data with financial statement information from CSMAR Financial Reports for all listed firms and their affiliates. These files provide business group-level variables including total assets, Tobin's Q, profitability (ROA), short- and long-term debt, cash holdings, cash flows, and dividend payouts. Integrating CSMAR and CARD allows us to characterize both the financial capacity and network structure of Chinese business groups over time.

3.3 Online Recruitment Data

Our primary measure of labor demand is derived from online recruitment data. We compile the universe of job advertisements published by listed firms and their affiliates between 2016 and 2023 across China's major recruitment platforms—51job, Zhaopin, Liepin, 58.com, Jobui, and multiple municipal talent websites. Duplicate postings across platforms are removed to ensure data consistency. Each posting includes information on job title, workplace location, salary range, education and experience requirements, gender preference, and intended number of hires. Using firm names obtained from the ownership databases, we link each posting to its corresponding firm through a standardized matching procedure. This yields a consistent firm–year panel of recruitment outcomes, including total openings, the spatial distribution of work locations, and average job characteristics such as required education, experience, and compensation levels.

Measurement considerations. Our primary outcome variable—job openings—is derived from online recruitment platforms, which may not capture the full spectrum of hiring activity. In particular, online postings are more common for white-collar and skilled positions, while blue-collar and manual labor positions are often filled through informal channels or offline recruitment. Moreover, any measurement error in job openings is unlikely to be systematically correlated with the pension reform, as the reform affected all types of employment uniformly. To the extent that online postings underrepresent certain sectors or occupations, our estimates should be interpreted as capturing the reform's impact on formal, advertised hiring rather than total employment changes.

3.4 Regional Economic Indicators

To account for variation in local economic environments, we merge city-level macroeconomic indicators from the China City Statistical Yearbooks and the National Bureau of Statistics. These variables include GDP growth, industrial composition, transaction prices of houses indices, and other indicators capturing differences in regional economic activity that may affect firms' hiring or reallocation behavior.

3.5 Sample Construction and Descriptive Statistics

Our merged dataset comprises 2,721 manufacturing business groups, covering 66,481 affiliate–year observations from 2016 to 2023. The sample is relatively balanced across years, with an average of 2,125 business groups-year observations per year. Among these, 49,128 affiliate–years are registered in the treated provinces—those directly affected by the pension rate reduction—while 17,353 affiliate–years are located in the control provinces of Zhejiang and Guangdong, where contribution rates remained unchanged during the reform period.

Table 1 reports descriptive statistics at both the affiliate and business group levels. Panel A summarizes affiliate-level characteristics, including job openings and registered capital. On average, an affiliate seeks to hire 260 employees per year, with substantial dispersion across firms. Panel B presents business-group-level characteristics. The average business group consists of 6 affiliates and employs approximately 4,800 workers. Partially treated groups—those with affiliates registered both inside and outside the control provinces—are substantially larger and more diversified, averaging 11 affiliates and over 7,350 employees, compared with approximately 4,000 employees among groups whose affiliates are entirely located in treated provinces. Together, these statistics highlight considerable cross-sectional variation in group structure, geographic exposure, and hiring behavior—variation that is central to our empirical analysis of within-group reallocation in response to province-specific labor-cost shocks.

3.6 Validity of Control Group

An important consideration is that our control provinces—Guangdong and Zhejiang—are among China's most economically developed regions, with higher average wages and more dynamic labor markets than most treated provinces. While their statutory pension contribution rates (14 percent) were lower than the national norm before 2019, their effective labor costs may have been comparable due to higher wage bases. This raises the concern that control-province affiliates may not provide a valid counterfactual for treated affiliates in unconditional comparisons.

We address this concern in several ways. First, our primary specifications include group×year fixed effects, which compare affiliates within the same business group, effectively controlling for group-level shocks and ensuring that identification comes from within-group variation in treatment exposure. Second, we conduct event-study analyzes that test for pre-trends and estimate year-specific treatment effects, allowing us to assess whether treated and control groups followed parallel trajectories before the reform. These approaches, detailed in Section 4, confirm that our findings are not driven by systematic differences between treated and control provinces.

4 Empirical Framework and Results

This section presents the empirical framework and corresponding results used to estimate the effects of the 2019 pension-rate reform on affiliates' hiring behavior and the transmission of cost shocks within business groups. The analysis proceeds in several steps. We begin by verifying that the reform indeed reduced labor costs. We then examine direct effects at both the group and affiliate levels, followed by an analysis of within-group spillover effects through internal labor markets. We further explore the mechanisms underlying these spillovers by examining horizontal versus vertical linkages, cross-regional labor reallocation, and heterogeneity across business groups and cities. Finally, we assess the reform's impact on overall business group performance.

4.1 Verification of Labor-Cost Reduction

Before examining business-group-level outcomes, we first verify that the 2019 pension contribution reform indeed reduced employers' effective labor costs. This analysis uses firm-level data from the China Tax Survey database, which provides detailed information on social security contributions and wage payments. Unlike the job-posting and ownership data used in our main analysis, this dataset is based on a random sampling scheme and thus contains a representative sample of Chinese firms. The objective here is not to study within-group reallocation but to confirm that the policy led to a genuine reduction in pension burdens at the firm level.

We estimate a difference-in-differences model of the following form:

$$\log(Y_{i,p,t}) = \beta\,(Treat_i \times Post_t) + \gamma' X_{i,t} + \alpha_i + \eta_p + \rho_{k,t} + \varepsilon_{i,p,t},$$

where $Y_{i,p,t}$ is the ratio of pension contributions to the sum of accrued wages and total social-security payments for firm i in province p and year t. The variable $Treat_i$ equals one if a firm is registered in a province directly affected by the 2019 rate reduction and zero otherwise. $Post_t$ equals one for years after 2019. The vector $X_{i,t}$ includes firm-level controls such as revenue, cash flow, paid-in capital, and debt. The specification includes firm fixed effects α_i, province fixed effects η_p, and industry-by-year fixed effects $\rho_{k,t}$. Standard errors are clustered at the county level.

Table 2 presents the estimation results. Across all specifications, the coefficient on $Treat_i \times Post_t$ is negative and highly significant. The estimates imply that the share of pension payments in total labor costs declined by roughly 1.6–1.7 percentage points in treated provinces after the reform. These magnitudes are robust to alternative control sets and indicate that the reform effectively lowered the pension burden for employers. This test confirms that the 2019 policy indeed produced a substantial decline in firms' effective labor costs. The result validates our definition of treated provinces and provides the empirical foundation for interpreting subsequent analyses of hiring and internal labor-market adjustments.

4.2 Direct Effects at the Group Level

We begin our analysis at the business-group level by testing whether groups with greater exposure to the 2019 pension-rate reform expanded hiring more after the reduction in employer contribution rates. Because the reform was centrally announced and implemented uniformly across provinces according to pre-determined statutory rates, it generates plausibly exogenous cross-sectional variation in the magnitude of cost reductions. To capture this variation, we construct a continuous measure of policy exposure at the group level.

For each business group g, exposure is defined as the share of affiliates registered in treated provinces (i.e., provinces where the employer contribution rate was reduced in 2019) weighted by 2018 hiring intensity or registered capital:

$$\text{PolicyExposure}_g = \sum_{i \in g} w_i \times \text{Treat}_i.$$

Because the reform is a one-time policy change, it is important to ensure that the estimated effects are not confounded by concurrent shocks. The outbreak of COVID in 2020 represents the most salient potential confounder, as it could have influenced firms' hiring decisions through two distinct channels.

COVID firstly affects a firm's hiring strategy by impacting local labor supply(Campello et al., 2020). We use *COVID Intensity* to measure the severity of the pandemic in each province, defined as the cumulative number of confirmed cases per 10,000 residents over 2020–2022. This index is aggregated to the group level using each affiliate's 2018 new openings as weights. Higher values of *COVID Intensity* imply tighter local labor supply through infection risks, mobility restrictions, and containment policies which may reduce new job postings.

COVID also affects companies' decisions on hiring by influencing the demand for their products, and in particular, the foreign demand needs to be given special attention. We use *COVID Exposure* to capture the extent to which each group is affected by foreign demand shocks during the pandemic. It is proxied by the 2018 provincial export-to-GDP ratio, weighted by each group's 2018 openings to form a time-invariant measure.

This variable controls for regional differences in export dependence that could influence hiring through changes in external demand. Including both COVID variables allows us to isolate the reform's causal impact from pandemic-related shocks. The specification is:

$$\log(Y_{g,t}) = \alpha + \beta_1(\text{PolicyExposure}_g \times Post_t) + \beta_2 \text{COVIDIntensity}_{g,t} + \beta_3(\text{COVIDExposure}_g \times Post_{2020,t}) + \mathbf{X}'_{g,t-1}\gamma + \mu_g + \lambda_t + \theta_p + \varepsilon_{g,t}, \quad (1)$$

where $Y_{g,t}$ is the logarithm of new job openings for group g in year t; $\mathbf{X}_{g,t-1}$ includes lagged group-level controls (*Size*, *Leverage*, *ROA*, *Asset Turnover*, and *Cash Flow*); and μ_g, λ_t, and θ_p denote group, year, and parent-province fixed effects, respectively. Standard errors are clustered at the business-group level. The coefficient β_1 captures the semi-elasticity of job openings with respect to exposure to the 2019 reform.

Table 3 reports the estimates and shows that business groups with greater exposure to the 2019 pension-rate reduction experienced significantly larger increases in new job openings after the reform. The estimated coefficients on the interaction term between policy exposure and the post-reform indicator are positive and economically meaningful across specifications. The magnitude of the coefficients suggests that a one-standard-deviation increase in exposure translates into roughly 2–4 percent higher growth in new openings relative to the pre-reform trend. This finding indicates that reductions in statutory labor costs had a substantial stimulative effect on firms' recruitment activity at the group level.

The inclusion of pandemic-related controls confirms that the observed expansion in hiring is driven by the reform rather than by almost contemporaneous COVID shocks in 2020. Higher local infection rates (*COVID Intensity*) are associated with a contraction in openings, consistent with temporary supply-side disruptions. Specifically, the coefficient on *COVID Intensity* ranges from -0.005 to -0.009 and is statistically significant in most specifications (columns (2), (4), (6) and (10)). A one-standard-deviation increase in COVID Intensity is associated with approximately a 0.6–0.9 percent decline

in new job openings, reflecting the adverse impact of pandemic-related lockdowns and labor supply constraints on recruitment activity.

In contrast, external demand exposure linked to export intensity (*COVID Exposure*) plays a more complex role. The coefficients on *COVID Exposure* × *Post2020* are positive and significant in several specifications (columns (5) to (6) and (9) to (10)), suggesting that business groups with higher export dependence experienced stronger hiring growth during the pandemic recovery period. This pattern likely reflects the resilience of foreign demand and the recovery of global trade after the initial COVID shock. Importantly, controlling for both COVID channels leaves the policy effect robust and, if anything, slightly stronger (compare columns (2) vs. (4) to (6), and columns (7) vs. (8) to (10)), suggesting that the reform's impact on hiring was largely independent of pandemic-related variation in labor supply and demand.

Economically, these results provide direct evidence that lowering employer contribution rates effectively reduced labor costs and encouraged recruitment among more exposed business groups. The estimated effect size is consistent with the notion that a marginal decline in non-wage labor costs improves firms' capacity to expand employment, particularly in multi-affiliate groups with broad geographic footprints. This direct evidence establishes the first stage of the analysis, motivating the subsequent examination of whether such cost advantages were further propagated within business groups through internal labor-market reallocation.

4.3 Direct Effects at the Affiliate Level

We next estimate affiliate-level difference-in-differences regressions to capture the local effects of the reform on labor demand. The specification is given by:

$$\log(Y_{i,g,p,t}) = \beta\,(Treat_i \times Post_t) + \gamma' X_{g,t} + \alpha_i + \eta_p + \rho_{k,t} + \varepsilon_{i,g,p,t}, \tag{2}$$

where $Y_{i,g,p,t}$ denotes the logarithm of job openings for affiliate i belonging to business group g in province p and year t. $Treat_i$ equals one if affiliate i is registered in a treated province directly affected by the contribution-rate cut and zero if registered

in the control provinces of Zhejiang or Guangdong. $Post_t$ equals one for years 2019 and thereafter. The vector $X_{g,t}$ includes group-level controls, namely *Size*, *Leverage*, *ROA*, *Asset Turnover*, and *Cashflow*. The specification includes affiliate fixed effects α_i, province fixed effects η_p, and industry-by-year fixed effects $\rho_{k,t}$. Standard errors are two-way clustered at the business-group and county levels.

The coefficient β measures the average change in job openings among affiliates registered in treated provinces relative to those in the control provinces after the reform. This design isolates local treatment effects while holding constant unobserved affiliate-specific heterogeneity and province-level shocks common to all firms. The identification assumption requires that treated and control affiliates exhibit parallel hiring trends prior to 2019, which is supported by event-study evidence presented in Figure 5.

Table 4 shows that affiliates registered in provinces directly affected by the reduction in employer contribution rates significantly expanded hiring after 2019. The estimated coefficients on *Treat* $\times$ *Post2019* range from 0.12 to 0.14 for openings, all statistically significant. These magnitudes imply that a 3–4 percentage point decline in statutory contribution rates led to roughly 12–14 percent higher new hiring relative to affiliates in the control provinces of Zhejiang and Guangdong. Including pandemic-related controls leaves the results virtually unchanged, and the parallel-trend evidence in Figure 5 supports the identifying assumption that pre-reform hiring trends were similar across groups.

4.4 Within-Group Spillover Effects Through Internal Labor Markets

The direct effects documented above capture local responses to reduced labor costs. However, business groups may also reallocate resources internally in response to differential cost shocks across regions. Internal labor markets allow firms to transfer workers, adjust hiring strategies, and coordinate employment decisions across affiliates within the same ownership network (Giroud and Mueller, 2015; Matvos and Seru, 2014).

When a subset of affiliates experiences a reduction in labor costs, the parent firm

may strategically shift new hiring toward these cost-advantaged locations while reducing recruitment in higher-cost regions (Tate and Yang, 2015). This reallocation is particularly feasible when affiliates operate in similar industries or share comparable production technologies and skills, as labor can be substituted more easily across similar task-specific situations than across vertically integrated stages (Gibbons and Waldman, 2004). Moreover, business groups with centralized human resource management and broader geographic footprints possess greater administrative capacity to coordinate hiring decisions and facilitate internal labor flows.

We test this hypothesis by examining whether affiliates in treated provinces expand hiring more strongly when their parent groups also include affiliates in control provinces that did not experience cost reductions. Specifically, we focus on whether groups operating across both treated and control provinces shifted hiring activity toward affiliates located in treated provinces, consistent with internal labor-market adjustments that transmit cost advantages across affiliates within the same ownership network.

To capture this mechanism, we estimate a difference-in-difference-in-differences (DDD) specification that exploits variation across affiliates within the same business group:

$$\log(Y_{i,g,p,t}) = \beta_1(Spillover_g \times Treat_i \times Post_t) + \beta_2(Treat_i \times Post_t) + \gamma' X_{g,t} + \alpha_i + \eta_p + \rho_{k,t} + \varepsilon_{i,g,p,t}, \tag{3}$$

where variables are defined as in Equation 2. $Spillover_g$ equals one if business group g operates affiliates both in treated and control provinces prior to the reform, and zero otherwise. The same group-level controls, fixed effects, and clustering procedures as in the baseline specification are applied.

The coefficient β_1 captures the extent to which affiliates in treated provinces expand hiring more strongly when their parent groups also include affiliates in control provinces that did not experience cost reductions. A positive estimate of β_1 indicates that business groups reallocate hiring internally toward treated affiliates, reflecting an additional margin of adjustment operating through within-group labor reallocation rather than

direct local cost changes.

To ensure that identification relies on appropriate comparison units, affiliates located in control provinces but belonging to partially treated groups are excluded from the estimation. For example, consider a business group with affiliates in both Zhejiang (control) and Jiangsu (treated). If the parent firm reallocates hiring from Zhejiang to Jiangsu in response to the reform, the Zhejiang affiliate's hiring will decline not because of local conditions but because of internal reallocation. Including such affiliates as controls would therefore bias the estimated spillover effect downward, as they are themselves affected by the reform through internal labor market adjustments. The valid control group instead consists of affiliates whose parent business groups are entirely registered in control provinces, where all affiliates faced unchanged contribution rates and were unaffected by within-group reallocation dynamics.

To assess the validity of the identifying assumption, we estimate an event-study version of the DDD model and plot the dynamic coefficients in Figure 6. The results indicate that the estimated coefficients in the pre-reform years are statistically indistinguishable from zero, suggesting parallel trends in hiring between the two sets of affiliates prior to 2019. Following the reform, the coefficients become positive and significant, implying that treated affiliates in mixed-exposure groups expanded hiring relative to affiliates in other groups after the reduction in employer contribution rates. The post-2019 pattern of coefficients thus supports the interpretation that the DDD estimates capture genuine within-group spillovers transmitted through internal labor-market adjustments rather than pre-existing differences in hiring trajectories.

To further illustrate the spillovers, we compare affiliates from partially treated groups and located in control provinces with affiliates from business groups only operated in Zhejiang and Guangdong. If the observed expansion of hiring among treated affiliates is indeed driven by internal labor market reallocation within business groups, one should also expect a corresponding reduction in hiring among affiliates located in control provinces that belong to the same partially treated groups. In other words, the internal reallocation of labor resources implies both spillovers to treated affiliates and contractions within control-side affiliates of the same group. We estimate:

$$\log(Y_{i,g,p,t}) = \beta_1(Spillover_g \times Post_t) + \gamma' X_{g,t} + \alpha_i + \eta_p + \rho_{k,t} + \varepsilon_{i,g,p,t}, \tag{4}$$

where the coefficient β_1 captures whether affiliates in partially treated business groups expand hiring after 2019 relative to affiliates in purely control groups.

Table 5 presents the estimates. The triple-difference results in columns (1) to (3) show that affiliates within partially affected groups—those operating both inside and outside treated provinces—experienced significantly larger post-reform increases in job openings. The coefficients on *Spillover* × *Treat* × *Post* range between 0.11 and 0.15, indicating economically meaningful within-group adjustments. In columns (1)–(3), the control group excludes affiliates registered in control provinces but belonging to partially treated business groups, thereby avoiding contamination from within-group spillovers; column (4) instead uses all affiliates registered in control provinces as the control group. The absence of pre-trend differences in Figure 6 confirms that these spillover effects emerged only after the policy shock.

The difference-in-difference estimates in column (5) indicate that the spillover effects are accompanied by corresponding contractions among indirectly affected affiliates, as reflected in the negative and significant coefficients on *Spillover* × *Post*. The magnitude of these coefficients is comparable to, or even larger than, the increases observed among directly treated affiliates.

Taken together, the results indicate that the 2019 pension-rate reform produced both direct and internal spillover effects on affiliate-level hiring. While the direct effect reflects local responses to reduced labor costs, the spillover effect reveals that business groups actively reallocated hiring capacity across affiliates, channeling new recruitment toward cost-reduced regions while simultaneously reducing hiring in affiliates located in higher-cost areas. This pattern provides direct evidence that internal labor markets function as an adjustment mechanism that propagates policy shocks within multi-affiliate organizations.

4.5 Horizontal versus Vertical Linkages

We further differentiate these within-group effects according to the type of linkages among affiliates. We distinguish between horizontal and vertical linkages within business groups, allowing us to identify potential heterogeneity in how internal connections shape affiliates' responses to cost shocks.

To operationalize these linkages, we construct measures of vertical and horizontal relationships based on China's 2017 Input–Output (IO) Table at producer prices. The IO table reports the matrix of intermediate flows between products and the vector of total output for each product. We aggregate products to two-digit macro-sectors according to their first two digits in the IO product code, ensuring consistency across firms classified by official industrial codes. This approach follows established methods in Atalay et al. (2014), Frésard et al. (2020) and Bartram et al. (2022), adapted to the Chinese IO structure.

Formally, let F_{pq} denote the intermediate use of product p by product q, and Y_q denote the total output of product q. The direct requirement coefficient is defined as $r_{pq} = F_{pq}/Y_q$, which measures the share of input p required to produce one unit of output q. We aggregate these flows to the sector level by computing

$$Flow_{IJ} = \sum_{p \in I} \sum_{q \in J} F_{pq}, \quad Y_J = \sum_{q \in J} Y_q,$$

and define $r_{IJ} = Flow_{IJ}/Y_J$, which captures the downstream reliance of sector J on inputs produced by sector I. Economically, r_{IJ} measures the strength of supply-chain dependence between upstream sector I and downstream sector J.

Using these input–output shares, we classify two sectors as vertically related if $\max\{r_{IJ}, r_{JI}\}$ exceeds a threshold of one percent (and five percent in robustness checks). A vertical link therefore indicates that firms in these sectors produce complementary goods or services connected through production chains. Sectors are labeled horizontal when they belong to the same two-digit macro-sector, implying that firms produce substitutable products that compete in similar markets.

Using this framework, we determine whether any pair of affiliates within the same business group are connected through horizontal or vertical relationships based on their industry classifications. We estimate the following regressions to analyze how these different intra-group linkages influence the transmission of labor-cost shocks:

$$\begin{aligned}\log(Y_{i,g,p,t}) = \beta_H \left(HorizontalSpillover_i \times Treat_i \times Post_t\right) \\ + \beta(Treat_i \times Post_t) + \gamma' X_{g,t} + \alpha_i + \eta_p + \rho_{k,t} + \varepsilon_{i,g,p,t},\end{aligned} \tag{5}$$

$$\begin{aligned}\log(Y_{i,g,p,t}) = \beta_V \left(VerticalSpillover_i \times Treat_i \times Post_t\right) \\ + \beta(Treat_i \times Post_t) + \gamma' X_{g,t} + \alpha_i + \eta_p + \rho_{k,t} + \varepsilon_{i,g,p,t},\end{aligned} \tag{6}$$

$$\log(Y_{i,g,p,t}) = \beta_V \left(HorizontalSpillover_i \times Post_t\right) + \gamma' X_{g,t} + \alpha_i + \eta_p + \rho_{k,t} + \varepsilon_{i,g,p,t}, \tag{7}$$

$$\log(Y_{i,g,p,t}) = \beta_V \left(VerticalSpillover_i \times Post_t\right) + \gamma' X_{g,t} + \alpha_i + \eta_p + \rho_{k,t} + \varepsilon_{i,g,p,t}, \tag{8}$$

where $Y_{i,g,p,t}$ denotes the logarithm of job openings for affiliate i belonging to business group g in province p and year t and variables are defined as in Equation 2. $HorizontalSpillover_i$ and $VerticalSpillover_i$ are affiliate-level indicators equal to one if affiliate i is horizontally or vertically linked, respectively, to at least one affiliate registered in a control province within the same business group.

Equation 5 estimates the heterogeneous within-group spillover effect for affiliates connected through horizontal linkages, capturing whether treated affiliates that are horizontally related to control affiliates exhibit different hiring responses after the reform. Equation 6 parallels this specification for vertically connected affiliates, testing whether

upstream or downstream relationships mediate the internal propagation of cost shocks among treated affiliates. Equations 7 and 8 provide complementary tests using affiliates located in the control provinces that are horizontally or vertically linked to treated affiliates, thereby examining whether spillover effects also extend in the opposite direction through substitution effect or along supply chains.

Table 6 presents the estimates and reveals that horizontal linkages are the dominant channel through which internal labor markets operate. Affiliates located in treated provinces that are horizontally related to affiliates in control provinces display significantly higher growth in job openings after the reform. The estimated coefficient on the horizontal spillover interaction term is around 0.19, indicating an economically meaningful reallocation of hiring within groups. The magnitude of these estimates is broadly comparable to the baseline spillover effects, reinforcing the robustness of the finding. This pattern suggests that when affiliates in low-cost provinces expand employment, business groups shift new hiring toward these affiliates at the expense of those in higher-cost regions, because similar production technologies and substitutable labor needs make reallocation across horizontally related units far easier.

By contrast, the estimates for vertical linkages are small and statistically insignificant in both directions. Affiliates vertically connected to control-province firms show no significant changes in openings, regardless of whether the linkage runs from treated to control provinces or vice versa. The absence of significant vertical spillovers implies that employment adjustments are not transmitted along supply chains, likely because supplier–customer relationships involve more rigid input commitments and less flexible labor substitution than horizontally related production units.

Economically, this pattern reflects fundamental differences in the nature of horizontal versus vertical linkages. Horizontal affiliates—those operating in the same industry—share similar production technologies, labor skill requirements, and product markets. This makes it easier for business groups to substitute labor across horizontally related units in response to cost shocks. For example, a manufacturing affiliate in Jiangsu can easily absorb workers originally intended for a similar affiliate in Zhejiang, as both require comparable skill sets and perform similar production tasks.

In contrast, vertical linkages involve complementary rather than substitutable production stages. A supplier affiliate producing intermediate inputs cannot easily absorb workers from a downstream customer affiliate, as the two operate in different industries with distinct labor requirements. Moreover, vertical relationships are governed by long-term contracts and input commitments that limit short-run adjustments in employment. As a result, labor reallocation along supply chains is constrained by technological and contractual rigidities, explaining why vertical spillovers are absent in our data.

Overall, these findings indicate that internal labor-market spillovers operate primarily through business segments that compete in similar markets rather than through interdependent production stages.

4.5.1 Job-Level Heterogeneity of Horizontal Spillovers

To further examine the composition of reallocated hiring, Table 7 investigates heterogeneity in the horizontal spillover effect across job types and required skill levels. The results reveal that the reallocation driven by horizontal linkages is concentrated in low- to mid-skill occupations. Affiliates in treated provinces increase openings for part-time and production positions, as well as those requiring less than a college's degree, while openings for positions requiring a bachelor's degree or above decline. These patterns suggest that the internal labor market adjustment primarily involves operational rather than high-skill positions, reflecting cost-sensitive expansions in routine and managerial roles.

Economically, the job-level evidence supports the view that horizontal internal reallocation reflects substitution in labor demand rather than technological complementarity. Business groups respond to labor-cost shocks by shifting hiring toward affiliates located in cost-advantaged provinces, particularly for positions characterized by flexible or scalable employment arrangements. In contrast, high-skill or specialized roles that depend on location-specific human capital display limited mobility across affiliates. These patterns are consistent with recent evidence that internal labor markets primarily facilitate adjustments through lower-level, operational job ladders rather than through

high-skill or professional tracks, where hiring and promotion are more rigid (Huitfeldt et al., 2023). Overall, these findings suggest that internal labor markets within Chinese business groups function as a short-run adjustment channel, reallocating routine employment opportunities across regions in response to spatial variations in labor costs.

4.6 Cross-Regional Labor Reallocation

A potential concern is that the observed spillover effects may not represent genuine hiring adjustments but rather cross-regional employment, whereby affiliates in treated provinces hire workers whose actual workplace is located in the control provinces of Zhejiang or Guangdong. To address this possibility, we re-estimate the spillover specification by focusing on job openings whose reported workplace is located in Zhejiang or Guangdong, and by separately analyzing groups whose parent firms are registered in treated versus control provinces.

Table 8 reports the results. The estimates show that cross-regional hiring occurs only when the parent firm is located in treated provinces. Affiliates in these groups modestly increase the number of openings for positions whose actual workplace is in Zhejiang or Guangdong, as reflected by the positive coefficient on *Spillover* $\times$ *Treat* $\times$ *Post* in column (2). However, when the parent firm is located in the control provinces, cross-regional hiring is not observed: the coefficient in column (3) is insignificant and negative. Turning to columns (4) and (5), where the dependent variable is the total number of openings, the results indicate that this cross-regional hiring pattern does not crowd out overall recruitment. Affiliates in groups with parent firms located in treated provinces experience a significant expansion in total openings, while those with parents in control provinces show no corresponding increase. This finding suggests that business groups headquartered in cost-reduced regions reallocate hiring toward affiliates in other provinces, amplifying the aggregate employment response to the reform.

This asymmetry suggests that the parent firm's location plays a critical role in shaping internal labor market adjustments. Parent firms located in treated provinces directly experience the cost reduction and may therefore have stronger incentives to coordinate hiring across affiliates to maximize the reform's benefits. In contrast, parent

firms in control provinces face unchanged labor costs and may lack the motivation or administrative capacity to reallocate hiring toward treated affiliates.

Moreover, when allocating scarce resources across competing projects within internal markets, headquarters play a central coordinating role (Williamson, 1975; Stein, 1997). In this process, parent firms typically retain control over key human resource decisions, such as hiring budgets, personnel transfers, and organizational restructuring, treating labor as an integral component of internal resource allocation. When the parent firm is located in a treated province, it is more likely to prioritize cost-advantaged regions in its hiring strategy and to facilitate cross-regional labor flows. This pattern is consistent with the view that internal labor markets are centrally coordinated by parent firms rather than autonomously managed by individual affiliates.

Taken together, the evidence indicates that spillovers arise mainly within business groups whose parent firms are located in treated provinces. We observe modest cross-regional hiring only in these groups—affiliates post more vacancies for jobs whose actual workplace is in Zhejiang or Guangdong—while there is no evidence of such hiring when the parent firm is located in control provinces. Moreover, when we consider total openings, affiliates connected to treated parents experience a significant expansion, whereas those linked to control parents do not. Overall, the reform led treated-parent groups to shift and scale up hiring toward cost-reduced locations, with cross-regional postings playing a secondary role and not offsetting the aggregate increase in vacancies. This pattern supports the view that internal labor markets amplified the policy's employment effects in provinces that directly benefited from the pension-rate reduction.

4.7 Heterogeneity Across Business Groups and Cities

We further examine how the strength of internal labor market spillovers varies with business group characteristics and local economic conditions. Specifically, we interact spillover exposure with group-level attributes—state ownership, labor intensity, and prior labor disputes—to capture heterogeneity in internal control, reliance on labor as a production input, and potential labor adjustment costs. In addition, we explore heterogeneity across city-level economic environments, including average wages, GDP

growth, and house prices, which reflect differences in local labor market tightness, economic opportunities, and housing-related mobility constraints that may shape the effectiveness of internal labor reallocation. Data on city-level GDP growth and average wages are obtained from the China City Statistical Yearbooks, while house price data are drawn from the China Real Estate Statistical Yearbook.

Table 9 presents the estimates. Panel A shows that spillover effects are significantly stronger among state-owned enterprise (SOE) groups and those with higher pre-reform labor intensity. The coefficient for SOEs is roughly three times larger than that for non-SOEs, indicating that state-owned groups have a greater capacity to reallocate employment internally in response to cost shocks. This pattern likely reflects their broader geographic footprint, more formalized personnel systems, and greater administrative flexibility in transferring workers across subsidiaries. Groups with higher labor intensity—those generating more revenue per worker—also exhibit larger spillover effects, suggesting that more productive or labor-intensive organizations can more effectively channel labor toward cost-advantaged affiliates. In contrast, the estimated coefficients are weaker among groups that experienced labor lawsuits before the reform, implying that prior legal frictions may limit internal flexibility and reduce the responsiveness of hiring decisions to external policy shocks. Together, these results indicate that both organizational efficiency and institutional stability strengthen a business group's internal adjustment capacity.

Panel B examines heterogeneity across local economic environments. Spillover effects are more pronounced in cities with lower average wages, faster GDP growth, and lower house prices, consistent with labor reallocation toward regions offering lower labor costs and higher growth potential. These results suggest that internal labor market adjustments amplify locational advantages by directing new hiring to areas where labor remains relatively inexpensive and economic activity more dynamic. In contrast, spillovers are weaker or insignificant in high-wage cities with tighter labor markets and higher housing costs, where further expansion is constrained.

Synthesizing the heterogeneity results across job types (Section 4.5.1), business group characteristics (Panel A), and local economic conditions (Panel B), a consistent

pattern emerges: internal labor market adjustments are most pronounced when (i) the reallocated positions involve routine, low- to mid-skill tasks that are easily substitutable across locations; (ii) the business group possesses strong organizational capacity, as reflected in state ownership, high labor intensity, and absence of prior labor disputes; and (iii) the destination regions offer lower labor costs and higher growth potential.

This pattern suggests that internal labor markets function as a short-run adjustment mechanism that reallocates routine employment opportunities across regions in response to spatial variations in labor costs. However, the capacity for such adjustments depends critically on both the organizational structure of business groups and the economic environment in which they operate. Groups with more centralized governance, greater administrative flexibility, and access to dynamic local labor markets are best positioned to exploit cost differentials and amplify the employment effects of labor-cost reductions.

Taken together, these findings show that internal labor markets amplify rather than offset regional disparities in labor demand. Groups with more efficient organizational structures and those operating in flexible, fast-growing local economies are best positioned to exploit labor-cost differentials across space. The heterogeneity results therefore highlight that the capacity for internal adjustment depends not only on the structure of business groups but also on the economic environment in which they operate.

4.8 Robustness Checks

To ensure that the estimated policy and spillover effects are not driven by spurious correlations or specification choices, we conduct a series of robustness checks summarized in Table 10.

First, we implement a placebo design in which Jiangsu and Fujian are redefined as control provinces, while other eastern and central provinces are treated. Jiangsu and Fujian provide suitable placebo benchmarks because they share similar levels of economic development, industrial composition, and export dependence with Zhejiang and Guangdong. Both provinces host a large number of manufacturing and service-sector

business groups with comparable firm-size distributions and labor-market conditions, making them appropriate counterfactuals for assessing regional hiring patterns. The placebo specification yields no significant effects for either the direct or spillover regressions (columns (1) and (2)), confirming that the baseline results are not generated by regional hiring trends unrelated to the reform. The absence of systematic differences under this falsification exercise supports the view that the estimated treatment effects capture the true impact of pension-rate reductions rather than general regional dynamics.

Second, we test the sensitivity of the results to alternative fixed-effect structures, including group-by-year, province-by-year, and parent-province-by-year fixed effects that absorb regional shocks correlated with the policy rollout. The coefficients on both the direct treatment and the spillover interaction terms remain positive and statistically significant across all specifications (columns (3) to (6)), indicating that the estimated effects are not driven by province-specific time shocks or by policy coordination at the provincial level.

Third, we replace the binary treatment indicator with the continuous statutory employer contribution rate to directly exploit variation in contribution intensity across provinces. The coefficient on this continuous measure is negative and significant (column (7)), implying that a one-percentage-point decrease in the statutory rate is associated with a 2 percent increase in new job openings. This continuous specification reinforces the interpretation of the reform as a genuine reduction in labor costs that stimulated employment expansion.

Taken together, these robustness exercises demonstrate that the observed policy and spillover effects are not sensitive to model specification, alternative definitions of the control group, or the inclusion of richer regional fixed effects. Across all exercises, the main results remain stable in both magnitude and statistical significance, reinforcing the causal interpretation of the baseline findings.

4.9 Business Group Performance Outcomes

Finally, we examine whether the pension-rate reform improved the overall performance of business groups by facilitating more efficient internal labor allocation. We estimate group-level regressions linking policy exposure (as defined in Equation 1) to subsequent changes in sales growth, Tobin's Q, and the likelihood of reporting losses.

Table 11 presents the results. The estimates indicate that business groups with higher exposure to the reform exhibit significantly faster growth and lower loss probabilities after 2019. The estimated coefficients on policy exposure are positive and statistically significant for both growth measures and negative for loss ratios. The magnitude of the effects suggests that a one-standard-deviation increase in exposure is associated with a 0.5–0.7 percentage point acceleration in sales growth and a 1–2 percentage point reduction in the probability of reporting losses. These improvements are economically meaningful and consistent across the two exposure measures constructed using baseline job openings and registered capital.

The positive association between exposure and Tobin's Q further suggests that the reform improved market valuations of more exposed business groups, consistent with investors anticipating productivity gains from lower labor costs and enhanced organizational flexibility. Although the coefficient on Tobin's Q is not always statistically significant across specifications, its direction aligns with the broader performance pattern.

Importantly, these performance gains are consistent with the internal labor market mechanism documented in earlier sections. Business groups that reallocated hiring toward cost-advantaged affiliates (as shown in Tables 5–6) were better positioned to exploit the reform's benefits, leading to faster sales growth and lower loss probabilities. The positive association between policy exposure and performance outcomes thus reflects not only the direct cost savings from lower pension contributions but also the efficiency gains from more flexible internal labor allocation.

To further isolate the role of internal labor markets, we re-estimate Equation 1 separately for groups with high versus low spillover intensity (defined as the share of affiliates exposed to within-group reallocation). Untabulated results show that perfor-

mance improvements are significantly larger among high-spillover groups, suggesting that the capacity to reallocate labor internally amplifies the reform's impact on group-level outcomes. This finding reinforces the interpretation that internal labor markets serve as an effective adjustment mechanism that converts labor-cost reductions into improved real and financial performance.

Taken together, these results indicate that the cost savings generated by the pension-rate reduction were not merely absorbed at the affiliate level but translated into tangible improvements in group-level performance. By reallocating labor internally and expanding hiring in cost-advantaged regions, business groups appear to have enhanced their overall operating efficiency and reduced downside risk. These performance gains reinforce the interpretation that internal labor markets serve as an effective adjustment mechanism—allowing multi-affiliate organizations to better absorb exogenous shocks, mitigate inefficiencies, and convert labor-cost reductions into improved real and financial outcomes.

5 Conclusion

This paper examines how business groups respond to exogenous labor-cost shocks by reallocating employment across affiliates through internal labor markets (ILMs). Exploiting China's 2019 pension contribution rate reform as a quasi-natural experiment, we find that reductions in statutory labor costs lead to significant expansions in hiring among affected affiliates, coupled with corresponding contractions within other affiliates of the same group. Specifically, treated affiliates expand hiring by approximately 12–14 percent, while affiliates in treated provinces belonging to partially exposed groups exhibit an additional 11–15 percent increase. Conversely, affiliates in control provinces within the same groups experience corresponding contractions of comparable magnitude. This symmetric pattern provides direct evidence that ILMs function as an active adjustment mechanism to regional cost shocks (Giroud and Mueller, 2019; Cestone et al., 2024). The reallocation predominantly occurs among horizontally linked affiliates sharing similar production functions, while vertically linked affiliates exhibit minimal adjustments. The transferred positions are mainly operational and managerial

roles requiring lower skill levels, and adjustments are more pronounced among state-owned enterprises, labor-efficient groups, and in regions with lower labor costs and faster economic growth (Tate and Yang, 2015; Huneeus et al., 2021; Bartram et al., 2022). Moreover, we find evidence of cross-regional hiring when parent firms are located in treated provinces, and document that groups with higher exposure to the reform exhibit significantly faster sales growth and lower loss probabilities, with performance gains concentrated among high-spillover groups (Giroud and Mueller, 2015; Biermann and Huber, 2024).

These findings have important implications for understanding how organizational structure shapes firms' responses to policy shocks. By reallocating labor internally, business groups can absorb regional shocks and sustain overall organizational efficiency, thereby enhancing adaptability to changing economic conditions (Khanna and Yafeh, 2007; Dai et al., 2024). Our evidence suggests that the employment effects of labor-cost reforms are amplified in regions where business groups concentrate their operations, implying that such policies may have heterogeneous distributional effects depending on the geographic footprint and organizational capacity of firms. From a policy perspective, policymakers should consider not only the direct effects of labor-cost reforms but also their indirect effects through internal labor markets when designing and evaluating labor market interventions (Draca et al., 2011; Geng et al., 2022). Moreover, the dominance of horizontal over vertical spillovers suggests that business groups' capacity for internal adjustment depends critically on the nature of inter-affiliate relationships, with horizontally integrated structures offering greater flexibility in responding to localized shocks. Future research could explore how firms endogenously design their organizational structures to maximize the benefits of internal labor markets, and whether similar adjustment mechanisms operate in response to other types of labor market shocks or in different institutional contexts.

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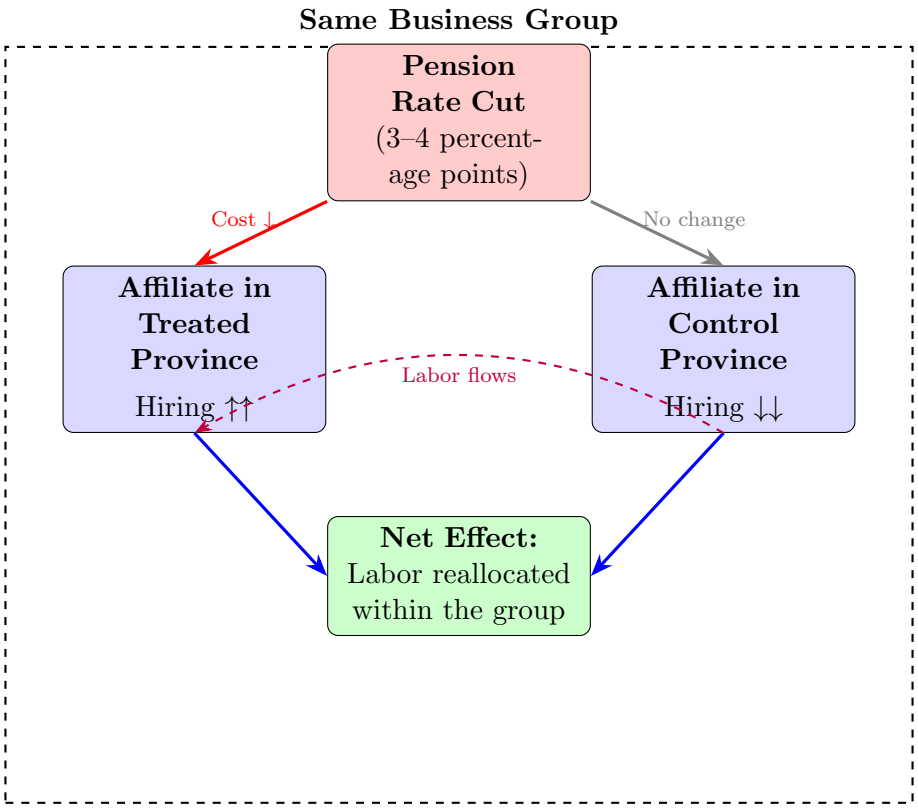
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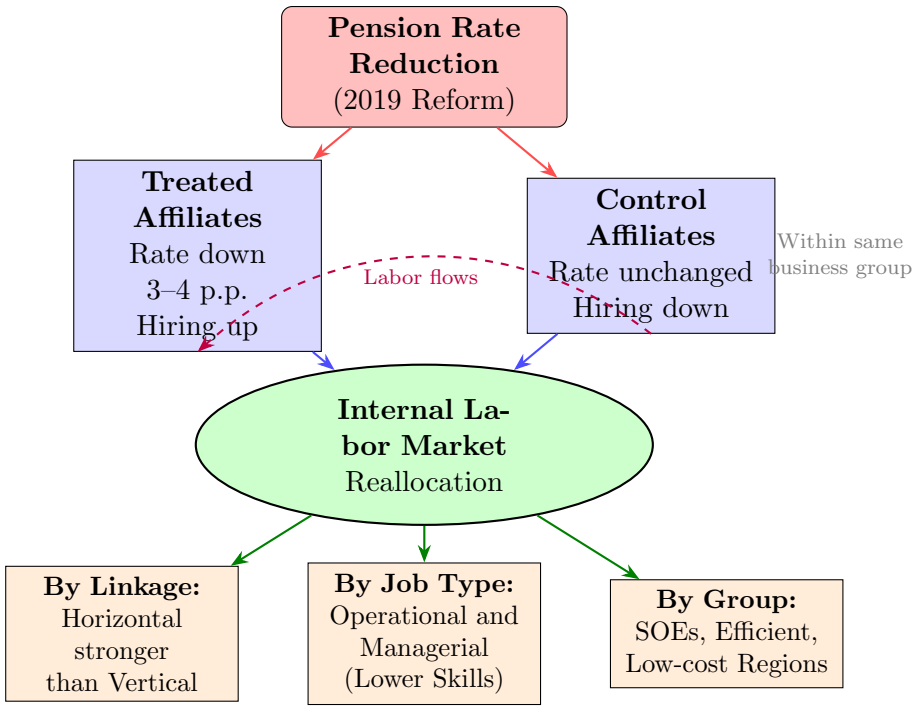
Figures

Figure 1: Internal Labor Reallocation: A Simple Illustration



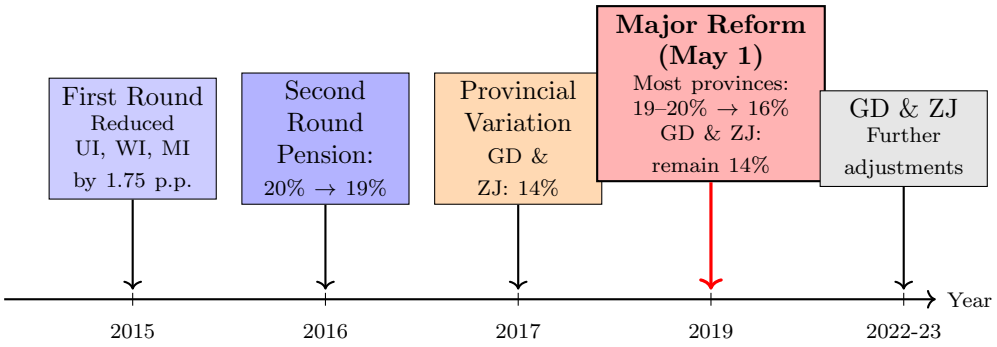
Notes: Within the same business group, a pension rate cut in treated provinces increases hiring in treated affiliates while decreasing hiring in control affiliates, resulting in internal labor reallocation.

Figure 2: Internal Labor Reallocation Mechanism in Business Groups



Notes: This figure illustrates the internal labor reallocation mechanism within business groups. A pension rate reduction increases hiring in treated affiliates while simultaneously decreasing hiring in control affiliates within the same business group. p.p. = percentage points.

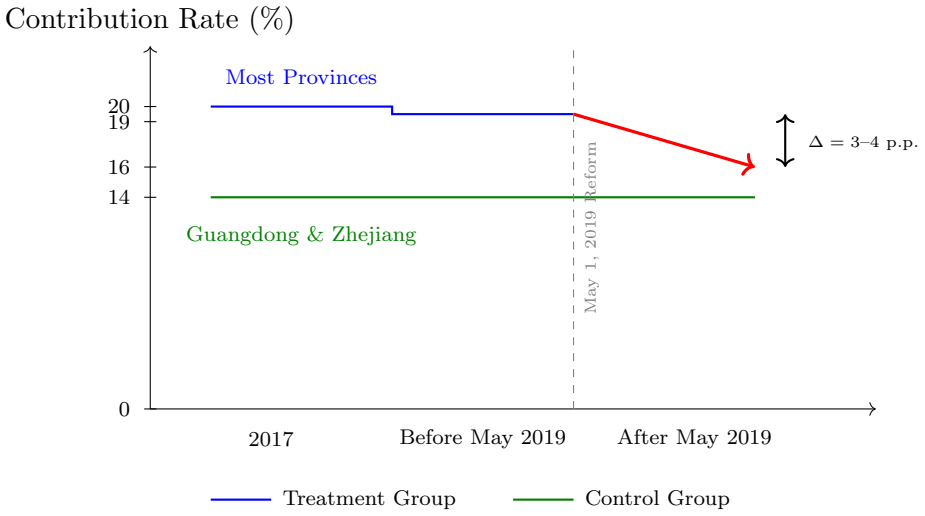
Figure 3: Timeline of China's Pension Contribution Rate Reform (2015–2023)



Treatment Group: 19–20% → 16% (reduction: 3–4 p.p.)
Control Group: Guangdong & Zhejiang remain at 14%

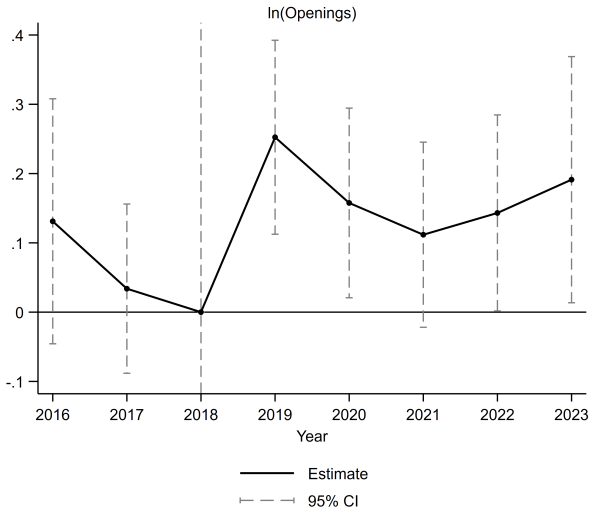
Notes: This figure illustrates the evolution of China's pension contribution reform. The 2019 reform created a quasi-natural experiment by reducing employer contribution rates to 16% in most provinces while Guangdong (GD) and Zhejiang (ZJ) maintained their 14% rates established in 2017. UI = Unemployment Insurance; WI = Work Injury Insurance; MI = Maternity Insurance; p.p. = percentage points.

Figure 4: Employer Pension Contribution Rates: Treatment vs. Control Provinces



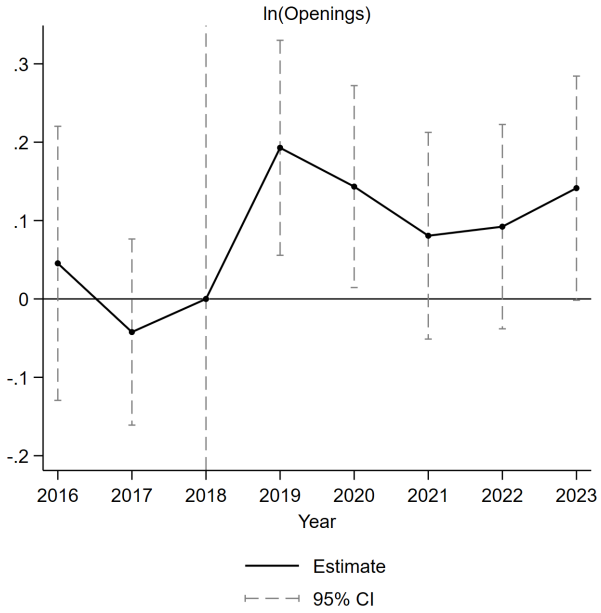
Notes: This figure shows the evolution of employer pension contribution rates for treatment and control provinces. The 2019 reform (vertical dashed line) reduced rates from 19–20% to 16% in most provinces, while Guangdong and Zhejiang maintained their 14% rates. p.p. = percentage points.

Figure 5: Parallel Trends for Policy Direct Effects



Notes: This figure illustrates the parallel trends assumption underlying the event study model and shows the event-time coefficients for job openings. The black solid line plots the point estimates, and the dashed lines represent the 95% confidence intervals. The year 2018 is normalized as the reference year (coefficient = 0). Estimates are at the firm-year level with standard errors clustered at business group and county level.

Figure 6: Parallel Trends for Spillover Effects



Notes: This figure plots the parallel trends for the spillover specification underlying the event study model and shows the event-time coefficients for job openings. The black solid line plots the point estimates, and the dashed lines represent the 95% confidence intervals. The year 2018 is normalized as the reference year (coefficient = 0). Coefficients and 95% confidence intervals are estimated at the firm-year level, with standard errors clustered at the business group and county level.

Tables

Table 1: Summary Statistics: Firms and Business Groups

The table reports sample summary statistics for affiliate- and business group-level characteristics. Panel A presents affiliate-level characteristics for the full sample of listed parent firms and their subsidiaries (66,481 affiliate-year observations), and separately for affiliates registered in Zhejiang or Guangdong (the control provinces, 17,353 observations) and those registered in other directly treated provinces (49,128 observations). Job openings denote the annual number of new job openings extracted from online vacancy data, and registered capital is reported in thousand RMB. All values are reported as mean, median, and standard deviation (SD) across the relevant unit of observation. Panel B summarizes characteristics at the business group level for 2,721 business groups. Control business groups (423 groups) are defined as those that operate affiliates only in Zhejiang and Guangdong provinces. Partially treated groups (855 groups) are defined as those that have affiliates registered both inside and outside the treated provinces, while all treated business groups (1,431 groups) have all affiliates registered in treated provinces. All values are reported as mean and standard deviation (SD) across the relevant unit of observation.

Panel A. Affiliate-Year Level Summary Statistics (Manufacturing Groups)

	All (66,481)			Treated (49,128)			Control (17,353)		
	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD
Job Openings	260	25	725	237	23	682	328	32	833
Registered Capital	48,410	10,000	219,835	53,601	10,000	233,387	33,049	7,000	172,750
Job openings (actual work in ZJ/GD)	185	0	11,860	33	0	4,094	616	26	22,164

Panel B. Business Group-Level Summary Statistics (Manufacturing Groups)

	All (2,721)		Control (423)		Partially Treated (855)		All Treated (1,431)	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Affiliates in Group	6.434	8.808	3.239	2.605	11.006	12.767	4.685	5.426
SOE	0.258	0.438	0.130	0.337	0.236	0.425	0.305	0.461
Labor Intensity	0.155	0.091	0.148	0.079	0.146	0.081	0.163	0.098
Size	22.295	1.258	21.701	0.938	22.684	1.242	22.231	1.268
Leverage	0.412	0.179	0.360	0.165	0.456	0.165	0.403	0.186
ROA	0.040	0.055	0.051	0.051	0.029	0.055	0.043	0.056
Asset Turnover	0.625	0.321	0.653	0.313	0.651	0.325	0.601	0.319
Cash Flow	0.051	0.049	0.056	0.051	0.051	0.044	0.050	0.050
Growth	0.146	0.183	0.144	0.171	0.142	0.183	0.150	0.185
Tobin's Q	1.972	1.022	2.046	0.866	1.866	0.888	2.018	1.131
Loss	0.147	0.230	0.116	0.208	0.168	0.229	0.145	0.236
Employment (avg)	4,814.971	8,921.239	2,347.428	3,297.974	7,351.331	11,867.797	3,996.462	7,451.835

Table 2: Verification of Labor-Cost Reduction: Firm-Level Evidence

This table presents results from firm-level difference-in-differences regressions that verify the impact of the 2019 pension contribution reform on effective labor costs. The dependent variable is the share of employer pension payments in total labor expenses, defined as pension contributions divided by the sum of accrued wages and total social-security payments. The key explanatory variable is $Treat_i \times Post_t$, where $Treat_i$ equals 1 if a firm is registered in a province that reduced its statutory employer contribution rate in 2019, and 0 otherwise. $Post_t$ equals 1 for years after 2019. Control variables include the inverse hyperbolic sine (IHS) of firm revenue, cash flow, paid-in capital, and debt. All regressions include firm fixed effects, province fixed effects, and contain year fixed effects in columns (1) and (3) or industry-by-year fixed effects in columns (2) and (4). The control variables include the inverse hyperbolic sine (IHS) of revenue, cash flow, paid-in capital, and debt. The sample period is 2016–2020. Robust standard errors are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

VARIABLES	Pension share in total labor cost			
	(1)	(2)	(3)	(4)
$Treat_i \times Post_t$	-0.017***	-0.016***	-0.017***	-0.016***
	(0.004)	(0.003)	(0.004)	(0.003)
Revenue			-0.001***	-0.001***
			(0.000)	(0.000)
Cash flow			-0.001***	-0.001***
			(0.000)	(0.000)
Paid-in capital			0.0001	0.0001
			(0.000)	(0.0001)
Debt			-0.0001**	-0.0001*
			(0.000)	(0.0001)
Firm FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y
Year FE	Y		Y	
Industry × Year FE		Y		Y
Observations	329,481	328,921	323,475	322,898
R-squared	0.610	0.621	0.612	0.623

Table 3: Policy Exposure, COVID Exposure, and Job Openings

The table reports estimates of the effects of policy exposure and COVID exposure on job openings. The dependent variable is the logarithm of the number of new openings at the group-year level. Policy exposure1 is constructed by weighting the exposure status of each group's parent firms and subsidiaries using their 2018 new openings. Subsidiaries registered in directly treated provinces are assigned an exposure value of one, while those registered in Zhejiang or Guangdong (the control provinces) are assigned zero. The weighted average across all affiliated firms yields the group-level policy exposure. Policy exposure 2 is defined analogously, except that the weights are based on the subsidiaries' registered capital instead of new openings. COVID Intensity measures the local supply-side shock from the pandemic. For each province, we calculate the number of confirmed COVID-19 cases per 10,000 residents in 2020, 2021, and 2022, and aggregate these using the group's 2018 distribution of new openings as weights to obtain a group-level weighted COVID intensity. COVID exposure captures the external-demand shock faced by the whole business groups. It is based on the 2018 provincial ratio of exports to GDP, which serves as a measure of exposure to foreign demand, again weighted by the group's 2018 new openings to obtain a time-invariant, group-level exposure measure. All control variables are measured with a one-year lag. All regressions include group, year, and parent-province fixed effects. Robust standard errors clustered at the business group level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively. The sample period is 2016–2023, except for columns that include COVID Intensity, which are restricted to 2016–2022 due to data availability.

VARIABLES	ln(Openings)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Policy exposure1 × Post2019	0.127**			0.160***	0.223***	0.261***				
	(0.054)			(0.055)	(0.063)	(0.064)				
Policy exposure2 × Post2019							0.116*	0.134**	0.171***	0.190***
							(0.060)	(0.061)	(0.065)	(0.067)
Covid Intensity		-0.006**		-0.007***		-0.009***		-0.005		-0.006*
		(0.003)		(0.003)		(0.003)		(0.003)		(0.003)
Covid exposure × Post2020			0.177		0.515**	0.564***			0.387*	0.413*
			(0.173)		(0.202)	(0.208)			(0.217)	(0.223)
Size	0.700***	0.631***	0.699***	0.632***	0.701***	0.632***	0.666***	0.590***	0.665***	0.590***
	(0.039)	(0.042)	(0.039)	(0.042)	(0.039)	(0.042)	(0.044)	(0.047)	(0.044)	(0.047)
Lev	-0.108	-0.151	-0.118	-0.142	-0.114	-0.148	-0.181	-0.175	-0.182	-0.177
	(0.127)	(0.140)	(0.127)	(0.140)	(0.127)	(0.140)	(0.145)	(0.158)	(0.145)	(0.158)
ROA	0.809***	0.807***	0.828***	0.790***	0.818***	0.800***	0.926***	0.910***	0.935***	0.919***
	(0.194)	(0.204)	(0.193)	(0.204)	(0.193)	(0.204)	(0.213)	(0.220)	(0.213)	(0.220)
ATO	0.101	0.091	0.106	0.088	0.105	0.093	0.105	0.061	0.109	0.066
	(0.066)	(0.075)	(0.066)	(0.075)	(0.066)	(0.075)	(0.074)	(0.084)	(0.074)	(0.084)
Cashflow	0.074	0.368**	0.080	0.361*	0.072	0.356*	0.075	0.290	0.080	0.292
	(0.174)	(0.187)	(0.174)	(0.187)	(0.174)	(0.187)	(0.207)	(0.217)	(0.207)	(0.217)
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Group FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Parent Province FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	22,448	18,910	22,448	18,910	22,448	18,910	16,467	14,224	16,459	14,224
R-squared	0.799	0.821	0.799	0.821	0.800	0.821	0.796	0.817	0.796	0.817

Table 4: Direct Effects of the Policy on Job Openings

This table shows presents results from affiliate-level DID regressions of Equation (2). The dependent variables in columns (1)–(4) are log job openings. The indicator variable Treat equals 0 if the affiliates registered in Zhejiang or Guangdong provinces, and 1 otherwise. Control variables include firm size, leverage ratio, return on assets (ROA), asset turnover (ATO), and cash flow, as well as affiliate, province and year or industry-by-year fixed effects in all columns. Column (2) also control the COVID effects. Robust standard errors clustered at the business group and county levels are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The sample period is 2016–2023.

Variables	ln(Openings)			
	(1)	(2)	(3)	(4)
Treat×Post	0.122**	0.132**	0.143**	0.138***
	(0.057)	(0.054)	(0.060)	(0.057)
Covid Intensity			0.049	
			(0.223)	
Covid exposure × Post2020			-0.008***	
			(0.003)	
Size		0.438***	0.391***	0.449***
		(0.040)	(0.046)	(0.042)
Lev		-0.097	-0.111	-0.044
		(0.127)	(0.140)	(0.138)
ROA		-0.276	-0.198	-0.179
		(0.182)	(0.196)	(0.189)
ATO		0.102	0.040	0.062
		(0.073)	(0.082)	(0.079)
Cashflow		0.029	-0.028	0.052
		(0.168)	(0.197)	(0.180)
Firm FE	Y	Y	Y	Y
Year FE	Y			Y
Industry FE				Y
Province FE	Y	Y	Y	Y
Industry × Year FE		Y	Y	
Observations	66,481	66,472	53,285	66,481
R-squared	0.762	0.772	0.793	0.764

Table 5: Internal Labor Market Spillovers: Reallocation Effects on Job Openings

This table presents results from affiliate-level difference-in-differences regressions of Equation (3) and Equation (4). The variable *Spillover* is a dummy equal to one for affiliates in groups with policy exposure between 0 and 1, which also means that those affiliates exposed to within-group spillovers through the internal labor market, and zero otherwise. The triple-difference term *Spillover* × *Treat* × *Post* captures the additional effects arising from within-group reallocation induced by the internal labor market. Columns (1) to (3) use affiliates excluding those registered in control provinces and those in groups with partial policy exposure ($0 <$ exposure < 1). Columns (4) use all affiliates in both control and treated provinces. Columns (5) use affiliates only in control provinces. Control variables include firm size, leverage ratio, return on assets (ROA), asset turnover (ATO), and cash flow. All regressions include firm, province, and year or industry-by-year fixed effects. Columns (2) additionally controls for COVID-related effects. Robust standard errors clustered at the business group and county levels are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The sample period is 2016–2023.

Variables	ln(Openings)				
	(1)	(2)	(3)	(4)	(5)
Spillover×Treat×Post	0.153***	0.106**	0.149***	0.295***	
	(0.056)	(0.052)	(0.057)	(0.103)	
Treat×Post	-0.073	-0.042	-0.058	-0.055	
	(0.095)	(0.099)	(0.095)	(0.095)	
Spillover×Post				-0.147	-0.147*
				(0.091)	(0.085)
Covid Intensity		-0.192			
		(0.245)			
Covid exposure×Post2020		-0.008**			
		(0.003)			
Size	0.474***	0.418***	0.479***	0.457***	0.468***
	(0.046)	(0.050)	(0.046)	(0.043)	(0.072)
Lev	-0.104	-0.162	-0.084	-0.044	-0.083
	(0.153)	(0.160)	(0.154)	(0.138)	(0.235)
ROA	-0.391*	-0.336	-0.391*	-0.187	0.115
	(0.216)	(0.228)	(0.217)	(0.189)	(0.301)
ATO	0.109	0.067	0.108	0.073	-0.005
	(0.086)	(0.089)	(0.085)	(0.079)	(0.115)
Cashflow	0.029	-0.063	0.056	0.057	0.114
	(0.199)	(0.211)	(0.199)	(0.180)	(0.277)
Firm FE	Y	Y	Y	Y	Y
Year FE	Y				
Industry FE			Y	Y	Y
Province FE	Y	Y	Y	Y	Y
Industry × Year FE		Y			
Observations	54,070	42,953	54,070	66,442	17,346
R-squared	0.763	0.794	0.763	0.764	0.777

Table 6: Spillover Effects within Business Groups: Horizontal Spillover vs. Vertical Spillover

This table reports results from affiliate-level regressions examining within-group spillover effects through horizontal and vertical linkages of Equation (5), Equation (6) and Equation (8). The dependent variable is the logarithm of new job openings. The indicator variable *Horizontal spillover* equals one for affiliate located in treated provinces that are horizontally related to another affiliate in control provinces, and zero otherwise. The indicator variable *Vertical spillover* equals one for affiliate located in treated provinces is vertically related—through supplier–customer relationships—to affiliate in control provinces, and zero otherwise. The coefficients on *Horizontal spillover*×*Treat*×*Post* in columns (1) and (2) capture purely horizontal spillover effects. The coefficients on *Vertical spillover*×*Treat*×*Post* in columns (4) and (5) capture purely vertical spillover effects from control provinces to treated provinces. The coefficients on *Horizontal spillover*×*Post* in column (3) capture horizontal spillover effects in control provinces. The coefficients on *Vertical spillover*×*Post* in column (6) capture vertical spillover effects in control provinces. Sample description for Columns (1) to (2) and (5) to (6): All affiliates excluding those registered in control provinces and in groups with $0 <$ policy exposure < 1; for Columns (3) and (6): All affiliates registered in control provinces. Control variables include firm size, leverage ratio, return on assets (ROA), asset turnover (ATO), and cash flow. All regressions include firm, province, and industry-by-year fixed effects. Robust standard errors clustered at the business group and county levels are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The sample period is 2016–2023. Sample description for

	Horizontal Spillover			**Vertical Spillover**		
Variables	ln(Openings)			ln(Openings)		
	(1)	(2)	(3)	(4)	(5)	(6)
Horizontal spillover × Treat × Post	0.190*	0.187*				
	(0.105)	(0.106)				
Treat × Post	0.014	0.060		0.020	0.067	
	(0.095)	(0.092)		(0.095)	(0.092)	
Horizontal spillover × Post			0.118			
			(0.088)			
Vertical spillover × Treat × Post				0.027	0.010	
				(0.071)	(0.071)	
Vertical spillover × Post						0.000
						(0.087)
Group-level controls		Y	Y		Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Province FE	Y	Y	Y	Y	Y	Y
Industry × Year FE	Y	Y	Y	Y	Y	Y
Observations	51,136	51,136	16,531	51,136	51,136	16,531
R-squared	0.769	0.771	0.788	0.769	0.771	0.788

Table 7: Job-level Heterogeneity: Job Characteristics of Horizontal spillover effects

This table investigates heterogeneity in the spillover effects of horizontal linkages regarding the Equation (5). All independent variables are the logarithm of new job openings, and Columns (1)–(3) split job openings by required education level; columns (4) and (5) examines part-time openings estimated using OLS model and the Poisson model respectively; and columns (6)–(10) classify job openings by functional category (management, technical, production, service, and administrative). All regressions use affiliates excluding those registered in control provinces and those in groups with partial policy exposure ($0 <$ exposure < 1). All regressions control for firm size, leverage ratio, return on assets (ROA), asset turnover (ATO), and cash flow and include firm, province, and industry–year fixed effects. Robust standard errors are clustered at the group and county levels. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The sample period is 2016–2023.

	Education Level			**Part-time**		**Job Type**				
Variables: ln(Openings)	Below College	College	Bachelor and Upper	ln(Part-time)	Part-time (Poisson)	Manage-ment	Technical	Production	Service	Adminis-trative
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Horizontal link × Treat × Post	0.199*	0.036	-0.701**	0.077**	0.908*	0.010	-0.062	0.276**	-0.052	0.111
	(0.107)	(0.152)	(0.318)	(0.031)	(0.479)	(0.220)	(0.146)	(0.127)	(0.147)	(0.171)
Treat × Post	-0.688***	-0.632***	0.189***	-0.593	0.117	-0.101	-0.091	-0.158	-0.124	-0.167*
	(0.120)	(0.126)	(0.059)	(0.519)	(0.314)	(0.121)	(0.103)	(0.109)	(0.094)	(0.094)
Group-level controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Province FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Industry × Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	53,423	53,423	7,370	53,401	9,509	24,013	37,933	33,700	28,960	27,227
R-squared	0.593	0.679	0.791	0.380		0.701	0.712	0.677	0.662	0.668

Table 8: Internal Labor Market Spillovers and cross-regional Labor Reallocation

This table examines whether cross-regional labor reallocation occurs within business groups regarding the Equation (3). All regressions use affiliates excluding those registered in control provinces and those in groups with partial policy exposure ($0 <$ exposure < 1). Columns (2) and (4) only use the subsample of the parent firms in the treated provinces, columns (3) and (5) represent the subsample of the parent firms in the control provinces, and column (1) use the full sample. Columns (1)–(3) use the logarithm of new job openings those actual work located in Zhejiang and Guangdong provinces as the dependent variable, while columns (4)–(5) use the logarithm of new job openings in this affiliate. The indicator variables *Spillover* is defined the same as previous tables. All regressions control for firm size, leverage ratio, return on assets (ROA), asset turnover (ATO), cash flow and include firm, province, and industry-by-year fixed effects. Robust standard errors are clustered at the group and county levels. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The sample period is 2016–2023.

Variables	ln(Openings actual work in Zhejiang or Guangdong)			ln(Openings)	
	(1)	(2)	(3)	(4)	(5)
Spillover × Treat × Post	0.033	0.119**	0.028	0.206***	0.072
	(0.043)	(0.050)	(0.204)	(0.065)	(0.350)
Treat × Post	-1.247***	-0.843**	-1.412***	0.193	-0.141
	(0.096)	(0.394)	(0.223)	(0.412)	(0.344)
Parent firm registered in	All prov.	Treated prov.	Control prov.	Treated prov.	Control prov.
Group-level controls	Y	Y	Y	Y	Y
Affiliate FE	Y	Y	Y	Y	Y
Province FE	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Observations	56,464	43,120	13,341	41,290	12,778
R-squared	0.775	0.667	0.853	0.761	0.770

Table 9: Business group- and City-level Heterogeneity in Internal Labor Market Spillover Effects

This table reports heterogeneity in internal labor market spillovers across different business groups-level and city-level characteristics. Panel A examines heterogeneity in spillover effects across business-group characteristics. Columns (1)–(2) compare state-owned enterprises (SOEs) and non-SOEs. Columns (3)–(4) divide groups by labor intensity, measured as the ratio of total employment to operating revenue at the group level. Columns (5)–(6) split groups according to whether labor lawsuit occurred before 2019 in this business group, and "low" represents never occurred before. Panel B explores heterogeneity across local economic conditions. Columns (1)–(2) use city-level average wages, measured by the annual average wage of employed workers in each prefecture-level city; columns (3)–(4) use the provincial GDP growth rate; and columns (5)–(6) use city-level house prices. All regressions use affiliates excluding those registered in control provinces and those in groups with partial policy exposure ($0 <$ exposure < 1). All regressions control for firm size, leverage ratio, return on assets (ROA), asset turnover (ATO), and cash flow and include firm, province, and industry–year fixed effects. Robust standard errors are clustered at the group and county levels. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The sample period is 2016–2023.

Panel A. Group-level Heterogeneity: Parent Firm Characteristics

	Labor Lawsuit		**Ownership Type**		**Labor Intensity**	
Variables: ln(Hires)	Never	Occurred	SOE	Non-SOE	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
Spillover×Treat×Post	0.111**	0.210	0.156*	0.049	0.123*	0.044
	(0.051)	(0.267)	(0.087)	(0.060)	(0.069)	(0.070)
Treat×Post	-0.048	0.156	-0.049	0.001	0.008	0.089
	(0.094)	(0.386)	(0.167)	(0.096)	(0.124)	(0.134)
Group-level controls	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Province FE	Y	Y	Y	Y	Y	Y
Industry × Year FE	Y	Y	Y	Y	Y	Y
Observations	50,158	3,869	18,121	35,922	25,161	25,577
R-squared	0.773	0.768	0.745	0.785	0.777	0.793

Panel B. City-level Heterogeneity: Local Economic Conditions

	City Wage		**GDP Growth Rate**		**House Price**	
Variables: ln(Hires)	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
Spillover×Treat×Post	0.122	0.135**	0.132**	0.276	0.105	0.100*
	(0.090)	(0.066)	(0.062)	(0.272)	(0.081)	(0.060)
Treat×Post	-0.028	0.033	0.033	-0.619*	-0.127	0.038
	(0.142)	(0.157)	(0.099)	(0.367)	(0.131)	(0.155)
Group-level controls	Y	Y	Y	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
Province FE	Y	Y	Y	Y	Y	Y
Industry × Year FE	Y	Y	Y	Y	Y	Y
Observations	27,741	20,423	23,516	20,857	26,350	23,683
R-squared	0.806	0.793	0.795	0.804	0.794	0.777

Table 10: Robustness Checks for Policy Direct Effects and Spillover Effects

This table presents robustness checks of the policy direct effects and spillover effects on affiliate-level DID regressions. The dependent variable in all columns is the logarithm of new job openings. Columns (1)–(2) report placebo tests in which Jiangsu and Fujian are controlled, while other eastern and central provinces are considered treated. Columns (3)–(6) test the sensitivity of the policy direct effects regression results to alternative fixed-effect structures, including the addition of province-by-year and parent-province-by-year fixed effects. Column (7) replaces the treatment dummy with the continuous statutory employer insurance contribution rate to directly capture variation in contribution intensity across provinces. Control variables include firm size, leverage ratio, return on assets (ROA), asset turnover (ATO), and cash flow. All specifications control for group-level characteristics and include affiliate and province, and industry-by-year fixed effects, unless otherwise indicated. Robust standard errors clustered at the business group level are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The sample period is 2016–2023.

	Placebo Test: Jiangsu and Fujian as Control		**Alternative Fixed Effects**				**Insurance Contribution Rate**
Variables	ln(Hires)		ln(Hires)				ln(Hires)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Placebo treat×Post	-0.105*	-0.119					
	(0.063)	(0.104)					
Placebo spillover×treat×Post		-0.050					
		(0.058)					
Treat×Post			0.189***		0.182***	-0.235*	
			(0.059)		(0.068)	(0.127)	
Spillover×treat×Post				0.116**		0.209***	
				(0.047)		(0.060)	
Insurance contribution rate							-0.020***
							(0.010)
Sample	1	2	All affiliates		3	3	All affiliates
Group-level controls	Y	Y		Y	Y	Y	Y
Affiliate FE	Y	Y	Y	Y	Y	Y	Y
Province FE	Y	Y	Y	Y		Y	Y
Industry FE	Y	Y					
Industry × Year FE					Y		Y
Group × Year FE			Y				
Province × Year FE				Y			
Parent Province × Year FE					Y	Y	
Observations	59,930	53,187	61,070	54,061	66,480	54,069	66,472
R-squared	0.768	0.768	0.827	0.777	0.767	0.766	0.772

Notes: Sample 1: All affiliates registered in eastern and central provinces; Sample 2: All affiliates registered in eastern and central provinces, excluding those registered in placebo control provinces in groups with $0 <$ policy exposure < 1; Sample 3: All affiliates excluding those registered in control provinces and in groups with $0 <$ policy exposure < 1.

Table 11: Policy Exposure and Business Group Performance Outcomes

This table reports business group-level regressions linking policy exposure to group performance outcomes (sales growth, Tobin's Q, and loss indicator). Policy exposure 1 and 2 are weighted by 2018 new openings and registered capital, respectively. All regressions control for firm size, leverage ratio and asset turnover (ATO) are measured with a one-year lag, and also include group, year, and parent-province fixed effects. Robust standard errors clustered at the business group level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively. The sample period is 2016–2023.

	Growth		**Tobin's Q**		**Loss**	
Variables: ln(Openings)	(1)	(2)	(3)	(4)	(5)	(6)
Policy exposure1 × Post	0.031*		0.073		-0.019*	
	(0.018)		(0.052)		(0.011)	
Policy exposure2 × Post		0.067***		0.113*		-0.019
		(0.023)		(0.059)		(0.013)
Size	0.219***	0.219***	-0.475***	-0.507***	-0.193***	-0.189***
	(0.015)	(0.017)	(0.057)	(0.064)	(0.009)	(0.010)
Lev	0.067	-0.002	0.174	0.101	0.968***	1.027***
	(0.050)	(0.059)	(0.143)	(0.154)	(0.031)	(0.035)
ATO	0.689***	0.633***	0.243***	0.221***	-0.307***	-0.299***
	(0.036)	(0.042)	(0.067)	(0.079)	(0.020)	(0.024)
Year FE	Y	Y	Y	Y	Y	Y
Group FE	Y	Y	Y	Y	Y	Y
Parent Province FE	Y	Y	Y	Y	Y	Y
Observations	22,584	16,523	22,589	16,527	22,589	16,527
R-squared	0.327	0.302	0.670	0.667	0.429	0.423

Appendix

Missing values in job openings may arise either because a firm does not actively hire in a given year or because no online postings are observed, rather than indicating firm exit. Therefore, we do not mechanically treat all surviving affiliates without observed online job postings as having zero hiring. To mitigate potential concerns arising from this ambiguity, we first rely on firm registration records to identify affiliates that remain active. We then impute zero job openings only for affiliates that had posted jobs online at least once prior to 2019 but exhibit missing postings in subsequent years. Finally, we assess the robustness of our results by applying alternative log transformations to the imputed job openings.

Table A1 shows that the baseline treatment and internal labor market spillover effects are robust to alternative log transformations of job openings. Relative to the baseline specifications, the estimated effects are slightly larger in magnitude, which is consistent with the fact that alternative log transformations give more weight to hiring adjustments among affiliates with low baseline posting intensity. Importantly, the qualitative patterns remain unchanged. The key coefficients remain similar in statistical significance when applying ln(Openings+0.1) and ln(Openings+1), alleviating concerns that the results are driven by the imputation of zero job openings or the treatment of firms with sparse online postings.

Table A1: Robustness Check: Alternative Log Transformations of Job Openings

This table examines the sensitivity of the baseline results to alternative log transformations of the dependent variable. Columns (1)–(3) use ln(Openings+0.1), while Columns (4)–(6) use ln(Openings+1). All regressions include the same control variables and fixed effects as in the baseline specification. Robust standard errors clustered at the business group and county levels are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The sample period is 2016–2023.

Variables	ln(Openings + 0.1)			ln(Openings + 1)		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	0.279***	0.180		0.097*	-0.036	
	(0.073)	(0.110)		(0.052)	(0.083)	
Spillover×Treat×Post		0.217***			0.118**	
		(0.068)			(0.052)	
Spillover×Post			-0.248**			-0.212**
			(0.106)			(0.091)
Groupplevel Controls	Y	Y	Y	Y	Y	Y
Affiliate FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Group FE	Y	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y	Y
Province FE	Y	Y	Y	Y	Y	Y
Observations	81,811	62,803	17,941	81,811	62,803	17,941
R-squared	0.680	0.663	0.681	0.722	0.704	0.727