When Protection Hurts: Labor Market Consequences of Mandatory Heat Allowances in China*

January 29, 2026

Sumit Agarwal[1], Kam C. Chan[2], Nianhang Xu[3], Ruoming Yang[3], Rongjie Zhang[4,5]

1. NUS Business School and Faculty of Arts and Social Sciences, National University of Singapore, Singapore
2. Research Center of Finance, Shanghai Business School, Shanghai, China
3. School of Business, Renmin University of China, Beijing, China
4. Tsinghua University, Department of Construction Management, China
5. Institute of Real Estate and Urban Studies, National University of Singapore, Singapore

Abstract

This paper examines the unintended labor-market consequences of mandatory heat-protection policies in China. Exploiting a mid-2010 policy that doubled the statutory heat allowance in a major Chinese city, we combine a continuous differences-in-differences design with employer-employee-matched panel data covering over 2 million workers from 2008 to 2013. We find that firms offset the explicit transfer by implicitly slowing wage growth for workers: in a representative outdoor-exposed firm (with 34% outdoor exposure), post-policy nominal wage growth decreases by about 1.8 percentage points relative to unaffected firms, implying an annual wage loss of roughly 1,360 yuan—7.6 times the extra mandated transfer. At the same time, dismissal probability rises by approximately 2.5 percentage points. Both adverse effects are concentrated among private firms, while state-owned firms remain unaffected. Across firms, wage and employment margins serve as substitutes: smaller firms rely more on the wage margin, whereas larger firms adjust on the employment margin. These baseline results remain robust across a series of tests and can be replicated using a multi-city sample of listed firms.

We also provide evidence for two distinct mechanisms. The major mechanism is selective wage incidence: firms offset labor costs by selectively slowing wage growth for workers subject to the mandated benefits. The other complementary mechanism is capital-labor substitution: capable firms expand fixed-asset investment, leading to non-selective dismissals. As a consequence, the mandate ends up taxing the very workers it is designed to subsidize, while indoor workers may also bear welfare losses through universal layoffs. These findings underscore a tension between climate-driven labor protection mandates and firm financial fragility, and highlight the importance of complementary policy instruments to prevent adverse worker outcomes.

Keywords: extreme heat; labor protection policies; labor costs; employee compensation; dismissal probability

* We thank Jing Wu, Li-an Zhou, Yiwei Li, Gaoliang Tian, Yi Jiang, Yuxuan Xiu, and conference and seminar participants at Asian Economic Development Conference (2025), Dishui Lake International Conference in Finance (2025), Renmin University of China, Shanghai University of Finance and Economics, Shenzhen University, Southern University of Science and Technology, and The 1st China Young Scholars Forum of Finance and Accounting for helpful comments and suggestions. Nianhang Xu acknowledges the financial support from the National Natural Science Foundation of China (Grant no. 72225005 and 72495154). Ruoming Yang acknowledges the financial support from the National Natural Science Foundation of China (Grant no. 72402224). Rongjie Zhang acknowledges the financial support from the National Natural Science Foundation of China (Grant no. 72504156). All remaining errors are our own.

1. Introduction

Global warming stands as one of the most conspicuous manifestations of climate change. To safeguard workers' welfare, numerous governments, including those of the United States, Germany, Spain, and China, have enacted legislation to protect workers from extremely hot working environments. However, given that most of these policies increase firms' labor costs, it is natural to ask how these heat-protection mandates affect employees' labor market outcomes and, especially, whether the consequences are consistent with the policies' intentions.

Our paper aims to investigate the unintended consequences of heat-protection mandates in the labor market. We hypothesize that, despite the intended benefits to employees of these policies, employees do not fully receive them, as is the case with other labor protection measures in different contexts, such as minimum wage laws and measures protecting female workers.[1] We expect that firms required to provide mandated heat allowances would reduce workers' wage growth to offset rising labor costs. Therefore, although the workers may receive higher allowances under the policy and their annual income continues to increase over the years, which indeed looks encouraging, their welfare is implicitly harmed.

To test our hypothesis, we need to capture counterfactual wage growth in the absence of the heat protection mandate. We take advantage of a full-sample, micro-level employer–employee matched database that allows calculation of the annual income of each employee in a large Chinese city between 2008 and 2013. The database covers over two million workers from about 200,000 firms. We focus on a 2010 city-level heat protection policy change that raised the heat allowance employers must pay from 60 yuan to 120 yuan per summer month. Although no database can capture allowance details, because small allowances are often given in cash and not recorded as wages in practice, we can still estimate the likelihood that an employee receives allowances. With the help of ChatGPT and firm-name textual analysis, we define a continuous-form treatment intensity indicator, or the outdoor-work exposure intensity, at the firm level. Using a differences-in-differences (DID) design with a series of fixed effects (i.e., employee-employer matched effect, year fixed effect, industry-year fixed effect), our analysis brings consistent evidence that the income growth rate of employees who work for

[1] Stigler (1946) finds that minimum wage policies incentivize firms to discharge less productive workers, thereby diminishing aggregate economic output while simultaneously impeding wealth accumulation for households that rely on continued employment.

firms with higher outdoor work exposure intensity significantly decreased after the heat protection policy became more stringent compared with employees who work for unaffected indoor firms, which falls on the opposite of the policy's intention. For a representative outdoor firm with an outdoor exposure rate of about 34%, this 1.8 percentage-point reduction in wage growth can translate into about 1,360 yuan in income loss per person per year. If we assume that 34% of the employees at the representative outdoor firm can receive an extra 180 yuan in heat allowance, then the wage loss can exceed 22.2 times the allowance gain for an outdoor worker. Even under the looser assumption that all workers at the representative outdoor firm are heat-allowance recipients, the wage loss can still exceed 7.6 times the allowance gains.

Meanwhile, the probability that an employee of an outdoor firm will be dismissed is much higher after the policy shock than among their counterparts in unaffected indoor firms, especially in larger firms, whose wage-reduction responses are less common than in small and medium-sized firms. Consequently, we observe that the number of employees at affected outdoor firms declines relative to those at indoor firms. Not surprisingly, the effects of the policy shock are dominated by private firms rather than state-owned firms, with the latter arguably being less sensitive to labor cost changes. Interestingly, the heterogeneous pattern across firm sizes illustrates substitution between wages and employment. We find that large firms are less likely to cut wage growth rates but more likely to increase dismissal probabilities. In contrast, medium and small firms take the opposite approach: they rely more on the wage margin than on the employment margin.

Our main findings on wage effects hold across a range of robustness checks. First, to enhance confidence in the classification of indoor and outdoor firms, we construct alternative treatment variables, including dummy variables and a keyword-extraction method, and always reach qualitatively similar results. Second, to address the potential omitted-variable bias arising from differentiated growth rates across outdoor and indoor industries, we not only control for industry-by-year fixed effects in the main regressions but also directly exploit within-industry variation in outdoor-working-related probabilities as the source of the treatment effect, yielding consistent findings. Third, we test whether firms exploit loopholes in the policy by adopting labor dispatching strategies and find no evidence that affected outdoor firms transfer their

formal employees to labor-dispatching firms to avoid paying heat allowances while still enabling those employees to work for them.

Lastly, to enhance the external validity of our main findings, we extend our single-city analysis to a multi-city, firm-level analysis by using provincial-level heat protection policy shocks. We collect data on listed firms from 26 provinces and apply a staggered DID model. Our findings are generally consistent with the baseline results: increasing the heat allowance at the provincial level reduces employees' incomes in outdoor firms. We also present a placebo test by focusing on an alternative listed firm sample located in cooler cities, and as expected, the employees' incomes are not affected by the heat protection policy.

We then investigate two mechanisms that possibly underlie the labor market outcomes associated with the heat protection policy. The primary mechanism stems from wage cost incidence through selective and implicit wage growth cut. We find that outdoor firms selectively cut wage growth rates for policy-targeted employees, such as low-income workers and male workers, to offset rising heat allowance expenses for these workers. That is to say, the workers who are meant to be protected by the policy bear the adverse effects more. Besides, the listed firm database enables us to identify which firms are more vulnerable to wage losses. The findings show that firms with less effective governance and more intense financing pressures are more likely to experience wage losses. Both pieces of evidence support the idea that firms would transfer their financial pressures to vulnerable employees, thereby offsetting the benefits those workers intentionally earn.

The complementary mechanism is capital-labor substitution, followed by unselective layoffs. Using a firm-year-level industrial firm database, we find that affected outdoor firms invest more in fixed assets to substitute relatively expensive human resources with relatively cheap machines. Consistent with the capital-human shift, we also find that the total factor productivity (TFP) of affected outdoor firms increased relatively after the policy shock. Although our full-sample database does not allow us to replicate the above findings, we still find that layoffs are universal within each affected firm, which is unlike selective wage reductions. These findings jointly support the mechanism by which firms, at least some large firms, take action to adjust/upgrade their overall operational strategy and reduce reliance on humans in response to rising labor costs. Although such upgrades may improve firms'

efficiency, the employees still suffer from higher turnover. In other words, the negative effects spill over from outdoor workers to other workers within the affected firms due to employment adjustments.

This paper contributes to a burgeoning strand of empirical literature on the unintended effects of labor protection measures, such as minimum wage hikes, mandatory insurance, and dismissal laws. Although some studies claim employee protections provide the benefits to employees that lawmakers intend (Jardim et al., 2022; van der Wiel, 2010), other studies point out that such policies may increase firms' labor costs and consequently drive them to change the hiring and firing decisions, which may harm aggregate employee benefits (Banker et al., 2013, Akee et al., 2019). Melchiorre and Rocca (2013) report that strict employment protection laws in Italy lead to a higher unemployment rate. Research based on multi-country data reaches similar results (Holt and Hendrickson, 2017). Despite the rich literature on labor protections in the context of minimum wage laws, research on employee protections related to global warming remains scarce (Su et al., 2020). It is unclear whether labor policies to protect workers from extreme heat lead to unintended consequences in firms' employment practices. Our research estimates the effects of Chinese policies intended to protect workers from heat, complementing the literature.

Our study also contributes to the existing scholarship on the impacts of extreme heat. Extremely high temperatures have long been recognized as hazardous working conditions for employees. In recent years, temperature-related deaths have surged globally (Vicedo-Cabrera et al., 2021). Park et al. (2021) find extreme heat causes about 20,000 injuries annually in the United States. Other studies show that heat exposure reduces employee working hours, lowers productivity, and raises absenteeism (Zivin and Neidell, 2014; Somanatha et al., 2021). A Chinese study finds that the overall income reduction of manufacturing workers is closely correlated with high temperatures (Yu et al., 2023). In addition to finding that extreme heat decreases employee performance, previous studies find a negative relationship between extreme heat and hiring behaviors (Jin et al., 2024; Acharya et al., 2024; Xiao, 2022) and extreme heat and firm performance, including sales and productivity (Addoum et al., 2020), stock returns (Choi et al., 2020), performance of supply chain partners (Pankratz and Schiller, 2024), and the output elasticity of labor (Somanathan et al., 2021). However, all these studies

focus on firm-level analysis and do not address salary changes or employee-level employment shifts. To the best of our knowledge, our paper is among the first employee-level empirical studies to investigate the impacts of government-imposed extreme heat protections on workers' employment outcomes from the perspective of employers' wage-paying and hiring practices. In addition to the employee-level social security database, we incorporate several other firm-level databases, including industrial and listed firm data, to provide a broader picture of the labor-market consequences of extreme heat.

Our paper also represents a novel extension of the broader literature on strategic responses to labor-market policy shocks. Strategic responses are well studied in the analysis of the relationship between local governments and the central government (Holmstrom and Milgrom, 1991; Fang and Zhang, 2025) and, more commonly, among firms. Leonardi and Pica (2013) analyze how Italian firms strategically respond to strict employment protection laws and find that, to offset increased labor costs, firms reduce the wages of newly hired workers. One recent study in China finds that after the 2008 Labor Contract Law strengthened labor protections for workers who had worked at the same firm for more than 10 years, some employers opted to dismiss workers before they reached the critical 10-year mark to circumvent the policy. Hence, the 10-year policy has resulted in substantial welfare losses for these dismissed workers (Akee et al., 2019). A few studies also reveal the active shift of investment preferences under strict minimum wage requirements or dismissal laws (Geng et al., 2022; Bena et al., 2022; Dai and Qiu, 2024). However, the literature underexplores policies that aim to raise employees' allowances in certain circumstances. A few studies, such as Friesen (2001), focus on overtime pay, but policies regarding pay for workers in extreme heat have not yet been studied. In this study, we document that affected firms take several actions to respond to policy shocks to reduce their economic burdens following the mandated increase in the heat allowance. All these actions, including not only selective wage reductions among the most vulnerable workers but also shifts from staffing to machines, harm employees, leading to even greater deviations from the original policy intentions.

The remainder of the paper is structured as follows. In Section 2, we provide the institutional background of the heat protection policy in China, both at the national and the city level. In Section 3, we describe the main data we use to conduct single-city analysis and the

corresponding empirical strategy. In Section 4, we present our primary empirical finding that increasing the allowance for workers who work in extreme heat leads to reduced employee income and increased dismissal probability. We also show a series of robustness checks. In Section 5, we extend our single-city analysis to a multi-city analysis using listed firm data to validate the external validity of our baseline findings. In Section 6, we explore the mechanisms underlying the baseline findings. Section 7 concludes.

2. Institutional Background

2.1 Heat Protection Policies in China

In the face of the harm caused by global warming, governments around the world have introduced regulations requiring employers to protect their employees during extreme heat. In the United States, California, Oregon, and Washington states have enacted such protections. California took the lead in 2005 by establishing regulations that require employers to provide water, shade, and rest breaks for outdoor workers when the ambient temperature exceeds 95°F (35°C). This law has been shown to have reduced workers' heat-related injuries by approximately 30% (Park et al., 2021). Similarly, in Europe, most countries have laws that protect employees from extreme heat, such as requiring access to water and breaks, and granting employees the right to interrupt their work without losing income. Moreover, Germany, Spain, and Portugal have set specific temperature limits that, when reached, require employers to take protective measures or to stop work.

China provides a valid institutional setting to study the effects of such labor protections on labor market outcomes, as it was one of the first countries to mandate certain employee protections during extreme heat. China has two central government-level heat protection policies. In July 1960, the Chinese central government issued the first national heat-protection laws, including cooling guidelines for factories, boilers, and workshops. These measures focused on technical operations to protect outdoor workers but did not mandate heat protection allowances.

In July 2012, the State Administration for Safety Regulation, the Ministry of Health, the Ministry of Human Resources and Social Security, and the All-China Federation of Trade Unions jointly issued the Administrative Measures for Heat Prevention and Cooling (AMHPC),

which amended the 1960 measures to further protect workers from extremely hot weather. The costly requirement, which is the focus of our research, is that employers must pay heat allowances to outdoor employees who work on extremely hot days (i.e., when the daily highest temperature exceeds 35°C). These allowances must be paid in wages and cannot be replaced by drinks or other related in-kind supports. The AMHPC also imposes some less quantifiable requirements, including providing adequate cooling drinks, providing medicines, giving advanced training, preparing emergency plans, and shortening working hours by reducing shifts, adjusting working hours, and prohibiting overtime work on extremely hot days for outdoor employees.

Notably, the AMHPC states that specific heat protection measures are to be imposed by provincial- and city-level governments, based on local circumstances that vary widely across provinces and cities. In other words, the restrictions imposed by the central government are the lower bound of local governments' heat protection policies. Local governments can set stricter rules, for example, by requiring firms to provide heat allowances on all summer days regardless of the actual temperature.

2.2 Heat Protection Policy in the Anonymous Sample City

Unlike the infrequent updating of national-level heat protection policies, the local government of the big city in our analysis has announced several heat protection policies since the start of the new century. Appendix Table 1 summarizes how the four policy measures, announced between 2007 and 2014, changed over time in our sample city, and how we construct our policy shock based on the strengthening of heat allowance standards.

The first important point was July 2007, which was also the year before the start of our original sample period and, thus, should serve as the policy's base year. Although the central government had no mandate about heat allowances then, the local government explicitly required employers to pay their outdoor-working employees a heat allowance of 60 yuan per month per person for all three summer months, that is, June, July, and August, regardless of whether the actual temperature exceeded a certain threshold.

In July 2010, the amended heat protection policy in the sample city raised the heat allowance from 60 yuan per month to 120 yuan per month. Therefore, we adopt 2010 as the time of our policy shock. Since our database is an employee-year panel, we cannot capture

changes within 2010 before or after July. Therefore, we regard 2010 as a grace period and exclude it from our empirical analysis.

The government announced the third policy in July 2012, right after the central-level announcement regarding the AMHPC, and the fourth policy in May 2013. However, as shown in Appendix Table 1, these new policies were nearly identical to the 2012 policy. Therefore, we do not regard them as additional policy shocks.

2.3 Discussions on Magnitude of Heat Protection Allowances

Before conducting empirical analysis, we should better understand what the seemingly trivial increase in the heat allowance from 180 yuan per person per year to 360 yuan per person per year means for affected firms. It is noteworthy that our main databases, the employer–employee–matched data based on the social security contribution panel, fail to provide any information about firm performance. Therefore, we turn to another database, the National Taxation Survey,[2] to explore the policy impact on firms' operations and performance.

The National Taxation Survey is a full-sample representative database that covers 38,315 firms[3] (including 34,477 indoor firms and 3,838 outdoor firms) in the same anonymous sample city in 2009 (i.e., the year right before the policy shock). Similar to our main employer–employee–matched database, the taxation database also covers firms of different sizes and across different industries. We classify all respondent firms into outdoor and indoor firms using the same method described in Section 3.2.

As shown in the first line of Appendix Table 2, the average net profit (*Profit*) of outdoor firms is 1,082,920 yuan, which is only 61.72% of that for an average indoor firm. However, the average number of employees (*EmployeeNum*) is significantly larger in outdoor firms (130 persons) than in indoor firms (100 persons), indicating a relatively high labor intensity among outdoor firms. Therefore, if we consider how much profit each employee can generate for the firm (measured as *P_Profit* in the third line of Appendix Table 3), we observe significant differences between outdoor and indoor firms. For indoor firms, each employee, on average, produces a net profit of 12,550 thousand yuan for the employer in 2009; for outdoor firms, the

[2] The data are downloaded from https://cndeepdata.pro/. This database, encompassing both large corporations and small and medium-sized enterprises, offers a high degree of representativeness for empirical analysis.
[3] We have excluded those firms with negative net assets.

same statistic yields only 9,270 yuan, suggesting lower profitability per unit of labor. Thus, increasing the heat protection allowance to 180 yuan per employee can take away over 1.94% of the profit generated by outdoor firm employees, which is a non-negligible share.

Additionally, we can measure the relative importance of the heat allowance increase using the firm-specific profit level as a reference. Specifically, for an average outdoor firm with 130 employees, we assume they need to pay an additional 180 yuan to all these workers due to the increase in the heat allowance. Thus, we can calculate the proportion of such an increased labor cost among the total profits of each firm as 2.16% (=180*130/1,082,924). Given the return-on-equity (*ROE*) for each respondent firm shown in the last line, the average *ROE* for outdoor firms of 3.73%, which is already 1.79 percentage points lower than that for the indoor firms, will drop to an even lower level of about 3.51% due to the economic burden imposed by the policy shock. These statistics indicate that outdoor firms are less profitable and more sensitive to increases in operational costs.

Therefore, although the absolute value of the heat allowance increase seems tiny, it would take a non-negligible bite out of the profits of affected firms, consistent with the common knowledge that outdoor firms are typically among the least profitable. For these firms, the exogenous increase in labor costs is not a small piece. Thus, it is not surprising if outdoor firms exhibit strong reactions to the policy shock.

3. Data and Specifications

3.1 Single-city employer-employee-matched data

The main database used in this paper is the administrative records of social security contributions from a large Chinese city between 2007 and 2014. Employees contribute to their social security accounts monthly through an automatic, pre-tax deduction, and employers match the contribution. The data give an employer–employee-matched unbalanced contribution panel at the year level. These employers represent all firms (not just listed firms) in the city, enabling us to capture the effects on the large majority of small firms that house most of the outdoor workers. Although the data cover only those employed in the formal sectors, coverage remains high. Compared to aggregate official statistics, we estimate our data covers over half of the city's total employment by 2014.

The city remains anonymous per the data use agreement. It is among China's largest and most prosperous cities. In total, our original database has some 330 million employee-month observations from 116,000 employers and 6.4 million workers. We can identify the names of 33% of the employers, who collectively account for over 85% of the employer–employee observations.

We refine our sample using the following three steps. First, our database includes only firms; we exclude government agencies and non-profit organizations. Thus, we can link business registration data to the firm's identity to classify ownership type as state-owned (16%) or privately owned (84%), both of which are included in our sample. Studying the heterogeneity in employers' payment and hiring practices between the state-owned and private sectors is one focus of this paper. Second, we exclude two types of special firms from our main sample:[4] 1) all 623 labor-dispatching firms whose employees are physically working for other firms that we cannot identify,[5] and 2) very small firms with fewer than 10 employees, because these firms typically are short-term in nature, and their employees generally exhibit unstable incomes. Lastly, we restrict our sample to adult workers aged 18-55 (females) and 18-60 (males), corresponding to their retirement ages. The final sample includes approximately 2.0 million employees, 19,320 employers, and 80 million employee-month observations.

For each worker in the database, we can back out their annual earnings because the total contribution in year t is a fixed percentage of the worker's yearly earnings with the same employer in year t–1.[6] The yearly earnings include wages and other incomes, such as allowances and bonuses.[7] The 80 million employee-month observations can thus be aggregated to 6.2 million employee-year observations. Two things are noteworthy about the income indicator. First, regarding the time period, the back-out method based on previous employment status means that we can only obtain income information from 2008 and 2013, which start one year later and end one year earlier than the end year of the original sample. Second, although the heat protection allowances should have been included in our annual

[4] Our main findings remain unchanged if we incorporate these observations back into our analysis. The results are available upon request.
[5] We separately adopt the subsample of these 623 labor dispatching firms in a subsection of our paper.
[6] See Guo et al. (2025) for more details about the backing-out methodology.
[7] The total wage indicator exclude year-end bonuses. However, such bonuses are only common in a few industries, typical outdoor industries not included.

earnings indicators by law, in practice, they can sometimes differ. Heat allowances can be given in small cash or even in-kind, and are not (fully) recorded in any administrative database, which is also one of the reasons why studies on heat allowances are scarce. We acknowledge this as a limitation of our data and therefore identify our estimates as the lower bound. Nevertheless, we can estimate the likelihood that an employee will receive heat allowances, which will be discussed in Section 3.2.

Another key element is dismissal probability, which considers whether an employee is still working for a firm at the end of the year. Specifically, for each employee matched with the employer, we can define whether they leave the employer by the end of the year based on whether they are no longer observed in the social security records and do not find a new job within the subsequent three months (and includes being unemployed by the end of the sample period). We denote this by a dummy variable, *If_leave*. Employees who leave their current employers but find new employment within three months[8] are part of reasonable and expected turnover rather than having been fired by the current employer; thus, they are not considered to have been dismissed. The sample period for this outcome variable is 2007–2014, which is two years longer than the income database, because employment status does not require back-out calculations as income does. We exclude employees whose last contribution record within the firm was in December 2014 (i.e., the last month of our sample period) because we cannot determine whether they left in December 2014.

3.2 ChatGPT-based treatment intensity indicator

A key challenge in investigating how the treatment group is affected by heat protection laws is that people cannot directly identify which firms engage in outdoor work. Since heat allowances are sometimes given away in cash and are not recorded in any database, we need a proxy for how likely the employee is to receive them. One way is that if the employee works in a firm highly engaged in outdoor activities, they are more likely to receive the heat allowance.

In our baseline analysis, we use ChatGPT's textual analysis to generate a continuous indicator, *pOutdoor*, to measure treatment intensity across firms. We use Python software to

[8] We acknowledge that we cannot determine why an employee leaves the employer. In our main analysis, we arbitrarily take three months as the cutoff between turnover and dismissal. Additionally, in the first two columns of Appendix Table 7, we change the cutoff to six months and one year, reaching estimates consistent with our main results.

automatically invoke the API of ChatGPT 4.0 to infer the probability of a given firm being involved in outdoor work or, equivalently, the proportion of workers among the given firm's employees who work outdoors. To enhance ChatGPT's capacity, we construct our prompt using typical prompt engineering practices, following three main steps. First, we ask ChatGPT about which industry the firm is more likely to belong to. In our raw data, we have detailed industry information for about half of the firms, which we also provide to ChatGPT to make the analysis more concise. We also record the industry information inferred by ChatGPT across 18 major industries to supplement our working database. Second, we ask ChatGPT to assess the typical working environment in the given industry and determine the proportion of workers who work outdoors. Specifically, we use the outdoor working proportions estimated by Licker et al. (2022) for some industries to guide ChatGPT's inference. Last, we ask ChatGPT to comprehensively evaluate and determine the proportion of outdoor workers at a given firm, and to provide a textual explanation. At this stage, we also aim to incorporate textual descriptions of the main business scope for some firms to strengthen inference. We manually checked 1,000 of the output results and are convinced by ChatGPT's inference process and outcomes. Therefore, for each sample firm, we can obtain the proportion of outdoor workers within the firm, denoted by *pOutdoor*. *pOutdoor* is a continuous variable ranging from 0 (all employees work indoors) to 1 (all employees work outdoors). We can also interpret *pOutdoor* as a probability: for a given firm, what is the likelihood that its employees' work involves outdoor operations? As shown in Table 1, for the employer–employee–matched data, the mean *pOutdoor* is 0.188 (SD = 0.273), indicating that 18.8% of employees are believed to work outdoors.

Figure 1 depicts the distribution of firms with different treatment intensity by industry based on *pOutdoor*. Figure 1 conveys at least two pieces of information. First, different industries have highly divergent treatment intensity. Specifically, the order of the bars in Figure 1 follows the average *pOutdoor* value of each industry[9]. As is common knowledge, firms in the construction industry experience the highest heat exposure: an estimated 37% of all employees work outdoors. Following the construction industry are the agriculture, mining,

[9] The average pOutdoor value of each industry is depicted in Appendix Figure 1.

public management[10], transportation, and energy industries. On the other hand, the industries with the lowest heat exposure include finance, IT, education, and cultural industries, whose treatment intensity is below 10%, which also aligns with our expectations. Second, and more importantly, Figure 1 also depicts the divergent distribution of firms across industries by treatment intensity using stacked bars. The five stacks from darkest blue to lightest blue indicates firms with strongest heat exposure probability (i.e., over 80% of the employees are believed to be responsible for outdoor duties) and least heat exposure probability (i.e., although the firm is related with outdoor operations, only no more than 20% of the employees are believed to be responsible for outdoor duties). The grey area denotes the proportion of firms whose employees are all believed to work indoors, i.e., are not covered by the heat protection policy.

*** Insert Figure 1 about here ***

Two things are noteworthy here. First, our treatment indicator is not a dummy variable, but a continuous variable. That means we do not assign all outdoor firms to the outdoor industry. Instead, we exploit the information embedded in firms' names to determine each firm's heat-exposure intensity. For instance, considering two firms from the construction industry, one working on architectural design and the other on on-site construction, the latter is believed to have higher heat exposure than the former, which would reflect in a larger *pOutdoor* value. Figure 1 also explicitly shows that, even in the construction industry, more than 30% of firms are not engaged in outdoor work. At the same time, the exposure intensity of the other firms also ranges widely. Similarly, even in the least-exposed industry, finance, a small proportion of firms is still partially related to outdoor operations.

Second, although the within-firm labor division exists, we cannot directly identify a worker's specific work scope within a firm. We acknowledge that an architectural design firm may hire outdoor workers, while an on-site construction firm may also hire indoor workers. However, our specification assumes that, on average, an employee working for an architectural design firm is less likely to be exposed to outdoor work than an employee working for an on-

[10] Public management industry typically includes the repairment and management of water conservancy, environment facilities and other public facilities, which are all outdoor facilities.

site construction firm, and thus is less likely to be affected by the policy shock we are interested in, given a smaller treatment intensity.

In addition to the ChatGPT-based textual analysis, in the robustness check section, we directly use a simpler keyword extraction method to generate an alternative treatment indicator. We use the firm name to identify *Outdoor* by extracting outdoor-related keywords from the name text.

We present the summary statistics of the main database in Table 1. The mean and standard deviation of *Income* are 68,182 and 50,431, respectively, suggesting the composition of employees is diverse. Further, on average, 30.0% of the employees leave the current firm within one year. The increased heat allowance of 120 yuan per summer month is equivalent to about 2.8% of the monthly average income of a representative outdoor worker in our sample before 2010. Although the increase in the heat allowance seems trivial from the perspective of individual income, we can use National Taxation Survey data to further measure what the increase in the heat allowance means to firms in terms of profit, which we shall discuss in more detail in Section 3.3.

*** Insert Table 1 about here ***

3.3 Empirical Specification

The major challenge in investigating employees' income and employment is the endogeneity of unobserved individual-level variables. To mitigate this problem, we construct the following continuous DID model with two-way fixed effects, as shown in Eq. (1):

$$Outcome_{ijkt} = \beta_1 * pOutdoor_j * Post_j + \delta_t + \mu_{kt} + \gamma_{ij} + \varepsilon_{ijkt}, \quad (1)$$

where i refers to an employee, j refers to an employer, k refers to an industry, and t refers to a year. $Outcome_{ijkt}$ refers to two main outcome variables, $Ln(Income_{ijkt})$ and If_leave_{ijkt}. $Ln(Income_{ijkt})$ is the natural logarithm of the annual actual income (including allowances) of employee i working for firm j by the end of year t. If_leave_{ijkt} is a dummy variable with a value of 1 if employee i stops working for firm j by the end of year t, and 0 otherwise.[11] *pOutdoor* is the continuous treatment intensity variable with the value ranging from 0 for firms whose all employees work indoors to 1 for firms whose all employees are involved in outdoor operations.

[11] In our main regressions, we adopt ordinary least square (OLS) regressions with both outcome variables. However, when using *If_leave* as the outcome variable, we also adopt logistic regression as a robustness check, which is reported in the last column of Appendix Table 7.

Methodologically, the continuous DID allows identification to rely on smooth variation in treatment intensity and more local parallel trends conditional on exposure levels. *Post* equals 1 after 2010 (i.e., the announcement year of the policy that raised the heat allowance), and 0 for the years 2008 and 2009. Notably, considering the policy shock that took place in mid-2010, we exclude the year 2010 from our regressions as the grace period. δ_t refers to year-fixed effects, which absorb the single term *Post*. μ_{kt} refers to industry-by-year fixed effects, which aim to control for confounding effects from industry-specific policies. Noted that our treatment intensity variable is not fully reliant on industry classification, although it does correlate with industry as discussed in the previous subsection, industry-by-year fixed effects would not absorb all the variations. Last, γ_{ij} refers to the employer–employee fixed effects, which is equivalent to employee-fixed effects for employees who never change jobs during our sample period. γ_{ij} absorbs the single term of *Outdoor*. Since we have controlled for the employee-fixed effects, the explanatory power of Eq. (1) should have been high without controlling for other time-variant individual attributes (e.g., education level), which we do not have in our database. ε_{ijt} is the error term clustered by employer.

After controlling for a series of fixed effects, β_1 captures the net effect of the policy on outdoor employees' income or dismissal probability. Therefore, taking income as an example, when we estimate the differences in income growth between treated and control employees, our income growth comes from the income before and after the policy shock for the same given employee working for the same firm. We winsorize all the continuous variables at the 1% and 99% levels.

3.4 Two Additional Databases

Our main database covers the full sample of firms, including both tiny firms and mega firms. The advantage of such a full-sample database is obvious: we can capture the policy effects on ordinary firms and workers, who constitute the majority of the economy and would yield generalized rather than marginal implications. Also, the wide variety of firms enables us to investigate the heterogeneity across firm sizes. However, the main database also has a few limitations. First, it contains only a few outcome variables. Second, the main database only covers one major city. Therefore, as a complement, we adopt two additional databases in our empirical analysis.

The first additional database is multi-city firm data on non-financial listed firms located in 26 provinces and cities that experienced at least one extreme heat day (i.e., a daily temperature exceeding 35 °C, the threshold set by the national-level heat protection policy) from 2009 to 2016. Our sample period ends in 2016 to exclude the unprecedented nationwide stimulus policies in the real estate sector since that time, which may have a large effect on the construction industry (i.e., the industry with the most outdoor firms defined in our paper, according to Appendix Table 2) and consequently confound the validity of our identification of using outdoor and indoor industries as treatment and control groups, respectively. We also drop the sample before 2008 to exclude the effects of the financial crisis on listed firms. The final sample comprises 2,376 listed firms across 241 prefectural cities in 26 provinces. The summary statistics of the sample are presented in Appendix Table 4. The city-level variables are collected from the Resource and Environmental Science Data Platform (RESDP), while the firm-level information is downloaded from the China Stock Market and Accounting Research. Appendix Table 4 shows that the average employment size of the sample listed firms is 4,022, which is 30 times the average firm employment size of the single-city database (133). Additionally, the average annual income of employees working for these listed firms (99,800 yuan) is 1.4 times that of employees in the single-city database (69,500 yuan). We use this multi-city database to test the external validity of the single-city findings in Section 5.

Second, in the mechanism section, we use another database, China's Industrial Enterprise Database[12] for the anonymous city, to analyze how firm performance responds to the policy shock. This database covers only large industrial firms whose annual primary business revenues exceed a certain threshold (i.e., 5 million yuan before 2011 and 20 million yuan thereafter). However, this database provides rich information on each year's firm-level financial conditions, including assets, debts, and revenues, and can be linked to our employer–employee-matched data using firm name, enabling us to directly investigate the effects of the heat protection policy on firm performance. The sample includes 1,522 industrial firms, involving 276,039 employees, matched with employee–employee-matched data. The summary

[12] The database is downloaded from CCER (https://www.ccerdata.cn/). Although the taxation data also provide a few financial indicators, we do not use it here because the sample period only starts from 2009, which makes it impossible to conduct a parallel trends test.

statistics are summarized in Appendix Table 5. The proportion of outdoor firms (6.9%) is slightly lower than that of the micro-level employer–employee-matched database (8.3%).

Notably, given that both the listed firm database and China's Industrial Enterprise Database only cover those very large, highly-paying firms, they should be less sensitive to the increase in heat allowance of 180 yuan per person per year. Therefore, their responses towards the heat protection policy should be the lower bounds relative to the large majority of tiny firms.

4. Empirical Results

4.1 Effects on Employees' Income

The first research question we test is whether the increase in the heat allowance affects employees' income. Before conducting regressions, we first depict the time trend of average income levels for both the treatment and control groups in Figure 2, Panel A. Here, we define the treatment group as those with a positive value of the continuous treatment intensity variable, *pOutdoor*. The blue solid line and light shade denote the coefficients and 95% confidence level of year fixed effects using the treatment group as the regression sample, while the black solid line and dark shade denote the coefficients and 95% confidence level of year fixed effects using the control group as the regression sample. The coincident confidence levels for 2009 indicate that employees from both outdoor and indoor firms exhibit statistically insignificant differences in wage growth from 2008 to 2009. However, from 2010 to the end of our sample period, employees' wage growth in indoor firms significantly outpaced that of outdoor firms, indicating that the increase in the heat allowance may lead to lower income growth for workers in outdoor firms. Some summary statistics can also support the raw pattern. Before 2010, the average annual wage growth rates for indoor firms (*pOutdoor* = 0) and outdoor firms (*pOutdoor* > 0) is 13.9% and 15.3%, respectively. However, the statistics reversed after 2010: annual wage growth rates increased to 17.2% among indoor firms, exceeding the 16.5% rate for outdoor firms. Although these patterns and the statistics are rough estimates and fail to incorporate sector-specific shocks and firm-specific attributes, they still imply a "relatively worsening" situation among outdoor firms, even though all sectors are growing thanks to the upward trajectory of the Chinese economy throughout the sample years.

*** Insert Figure 2 about here ***

Our formal regressions follow Eq. (1), where we introduce the interaction term between the policy and the treatment intensity variable, *Post* × *pOutdoor*, into the same regression and estimate the DID model in Table 2, Panel A. In column (1), we control for year fixed effects to capture the time trend and employer-employee fixed effects to capture unobservable variables related to worker-firm matching. In column (2), we cluster the error at the firm level to account for within-firm serial correlation in the error term. In column (3) (our main specification), we further control for industry-by-year fixed effects to account for sector-specific demand conditions, productivity trends, and other time-varying shocks that may differentially affect wages across industries. From columns (4) to (6), we further try clustering the error in different ways: at the employee level in column (4)[13], at the industry level in column (5), and two-way clustered at the employer and year level in column (6). All the specifications yield highly consistent results: after the policy raises the heat allowance, the incomes of employees working for affected outdoor firms with full policy exposure during the following calendar year decrease by approximately 5.6% relative to those working for unaffected indoor firms (column (3)).

*** Insert Table 2 about here ***

To assess the economic magnitude, we focus on firms with average outdoor exposure (*pOutdoor*=0.34) among all treatment firms. Using the estimated coefficient on *Post* × *pOutdoor*, we can infer that employees working for these representative firms experienced a post-policy reduction in cumulative log wage growth of approximately 1.84 percentage points (=exp(0.0546*0.34)-1) between 2011–2013 and 2008–2009 relative to firms with zero exposure (*pOutdoor*=0). Given an average post-policy annual wage of 73,481 yuan across all exposed firms, this corresponds to 1,360 yuan per year. In other words, although the wages of affected workers keep rising after the policy shock, they actually lose 1,360 yuan per year when their decelerated wage growth rates relative to those of their unaffected counterparts in indoor firms are taken into account.

Notably, even though the income indicator recorded in our main database should include all allowances and bonuses (including heat allowances) according to the law, in practice, heat

[13] Since the treatment is defined at the firm level and varies only across firms, clustering at the firm level is the conservative choice relative to clustering at the worker level.

allowances are typically given in cash or even in kind and cannot be fully recorded in formal income panel data. Therefore, the 1,360 yuan in wage losses from slower wage growth could be partially offset by the higher cash allowances they received. Nevertheless, the wage offset can still be roughly 7.6 times the statutory transfer of 180 yuan (i.e., an extra 60 yuan allowance per month in June, July, and August), implying that the mandated subsidy was more than fully offset by wage adjustments. Such an estimate can even be an underestimation, because the extra 60 yuan allowance could first be the maximum value the employer provided and, second, already (partially) included in our wage indicator. From the perspective of labor market incidence, the pattern is consistent with a compensating differential: firms facing higher outdoor exposure reduced wage growth after the mandate, more than offsetting the intended redistribution toward workers employed in more heat-exposed firms.

To verify the validity of the DID specification, we present the dynamic effects of the policy shock in Figure 3. Compared with binary DID, our continuous DID setting allows the identifying assumption to be weaker and more local, with parallel trends conditional on exposure levels. Using 2008 as the baseline, we find no pre-trend in 2009 (i.e., the year before the 2010 policy shock) in the annual income differences across firms with different treatment intensities, indicating that the income growth rates of different firms are statistically unchanged during the pre-period relative to the baseline period. Figure 2 also shows that the effect of the 2010 policy on income decreases immediately after the policy and then remains relatively stable for the remaining years, consistent with Table 2, Panel A, and Figure 2, Panel A.

*** Insert Figure 3 about here ***

Last, to make our point more explicitly, we substitute the outcome variable from wage level to wage growth rate and also decompose the employer-employee fixed effects into weaker components (i.e., employer fixed effects, and employee's personal attributes, including age, age in square term, gender, and whether the employee is born in the same city) and then replicate Panel A of Table 2. As shown in Appendix Table 6, the negative coefficients on the interaction terms suggest that wage growth rates decreased more for workers from firms with higher policy exposure intensity after the policy shock. For a representative outdoor firm, the wage growth rate is 1.08 percentage points lower than that of indoor firms, comparable to the

magnitude translated from baseline regression results (0.53 percentage points[14]). Such a finding again supports the conclusion that firms with stronger policy exposure intensities would reduce wage growth to offset the extra allowance expenditures. This could also partially explain the rationality of the large magnitude of the negative policy effect: although cutting down the absolute wage level of outdoor workers due to increased heat allowances would surely irritate affected employees, the effects could be easily hidden behind the shrinking wage growth rate. From the workers' perspective, the higher nominal wage growth and the explicit heat allowance may have signaled an earnings gain, while limited information about wage dynamics in unaffected firms prevented them from recognizing the relative wage compression induced by the mandate.

4.2 Effects on Dismissal Probability

The analysis on the effects of the heat protection policy on employees' dismissal probability also starts with the raw trend. The pattern shown in Figure 2, Panel B, is highly consistent with that in Figure 2, Panel A. The coincidence confidence levels in 2009 indicate that employees from both outdoor and indoor firms do not differ significantly in the probability of dismissal before the policy shock. However, from the year 2010 to the end of our sample period, the dismissal probability for employees of outdoor firms continuously surpasses that of employees of indoor firms.

Table 2, Panel B, summarizes the regression results of Eq. (1) using dismissal probability as the outcome variable. As shown by the positive coefficient of interaction term in column (3) with the most complete specifications, after the increase in the heat allowance, employees working for an outdoor firm with full exposure (*pOutdoor*=1) at the start of the year are approximately 7.5%[15] more likely to leave the firm without finding a new job within three months or by the end of the year compared to those working for an indoor firm. If we consider the representative outdoor firm with 34% of the employees working outdoors (*pOutdoor*=0.34), then the treatment effect would be 2.5%. Given that the average dismissal probability among outdoor-firm workers before 2010 was about 19.7%, the magnitude of this treatment effect is

[14] Taking 2008-2009 as the pre period and 2011-2013 as the post period, the estimated log wage gap of about −0.0186 for a representative outdoor firm (*pOutdoor*=0.34) over roughly 3.5 years implies an annualized growth differential of approximately -0.53 percentage points per year relative to unaffected firms.

[15] exp(0.0723)-1=0.0750.

equivalent to a remarkable 12.9% (2.5%/19.7%). In other words, raising the heat allowance leads to substantially higher dismissal probability and less stable employment. These effects provide further evidence of the unintended negative impacts of China's heat protection policy.

Additionally, as shown in Figure 4, the dismissal probability shows highly consistent parallel patterns with Figure 3. We find insignificant pre-trends in the differences in dismissal probability among firms with different treatment intensity before 2010 and lasting positive effects since 2011. The effects grow larger in 2014, which might be explained by the fact that employment changes are usually harder and take a longer time than wage adjustments due to employment contracts.

*** Insert Figure 4 about here ***

Last, as a natural consequence of the rising dismissal rate, we also expect firm size to shrink. Therefore, we use firm-level data to analyze how firm size responds to the policy shock, using a DID design.[16] As shown in column (1) of Appendix Table 8, Panel B, the interaction term between *Outdoor* and *Post* is significantly negative at the 1% level. With the magnitude of the coefficient of *Post*Outdoor* at –0.0685, the policy that increased the heat allowance leads to an approximately 2.4%[17] reduction in the number of employees at average outdoor firms (*pOutdoor*=0.34) relative to the unaffected firms. The last two columns of Appendix Table 6, Panel B, suggest that the effects are concentrated among private employers rather than state-owned employers.

4.3 Employer-level Heterogeneous Responses

In this subsection, we investigate the two sets of employer-level heterogeneous effects.

We first consider firms' ownership type. In China, compared with managers of state-owned firms, owners of private firms can benefit much more from reductions in human costs and increases in profits (Kato and Long, 2006; Le et al., 2023). Therefore, private firms are more motivated than state-owned firms to reduce employees' income in response to the new

[16] The firm-year database contains 31,572 employers from 2007 to 2014. The summary statistics of the firm-level database, presented in Appendix Table 8. Panel A, show that an average firm has approximately 133 employees. We adopt the following regression model to conduct our analysis: $EmployeeNum_{jkt} = \beta_1 * Outdoor_j * Post_j + \alpha_j + \mu_{kt} + \delta_t + \varepsilon_{jkt}$, where j refers to an employer, k refers to a industry, and t refers to a year. *EmployeeNum* refers to the number of employees at the employer level in natural logarithmic terms. *Outdoor* is the dummy variable for employers who are more likely to be involved in outdoor operations based on keyword extraction of firm names. α_j refers to employer fixed effects, μ_{kt} refers to industry by year fixed effects, and δ_t refers to year fixed effects, respectively. ε_{jkt} is the error term clustered by the employer. β_1 captures the net effect of the policy on outdoor-working-related firms' size. All the continuous variables are winsorized at the 1% and 99% levels.

[17] $0.34\times(\exp(0.0723)-1)=0.0241$

heat protection policy. As shown in the first two columns of Table 3, Panel A, the results confirm our expectations.[18] Similar to the first two columns of Table 3, Panel A, the first two columns of Table 3, Panel B, present results for state-owned and private firms: employees of private outdoor firms are significantly more likely to leave their firms than employees of indoor firms after the policy shock. At the same time, there is no significant difference between employees in outdoor and indoor state-owned firms, and the coefficient even turns negative. The between-group differences are insignificant.

*** Insert Table 3 about here ***

We then examine how firm size affects firms' sensitivity to changes in labor costs. We divide our sample into three groups: small firms with no more than 100 employees (column (3)), medium firms with 100-1000 employees (column (4)), and large firms with more than 1000 employees (column (5)). As shown in the last three columns of Table 3, panel A, small- and medium-sized firms significantly reduce their employees' wage growth rates at the 1% and 5% levels, respectively, while the coefficient for large firms is insignificant, though still negative. In other words, when firm size grows larger, the reverse effects on wages turn weaker. However, it does not mean that working for large firms would ensure benefits from the new mandates. The last three columns of panel B suggest that the negative effects on dismissal probability are concentrated among large firms rather than among medium or small firms. Combining both sets of estimates, the heterogeneity across firm size suggests that large firms adjust along the employment margin, raising dismissal probability without compressing wage growth, whereas small and medium firms adjust primarily along the wage margin without additional layoffs. A natural interpretation is that larger firms possess slack or internal reallocation capacity that enables personnel adjustment, while smaller firms rely more heavily on the wage margin to absorb the mandated cost shock.

Our findings about differential responses among firms of different sizes are highly consistent with the literature. A large empirical literature documents that mandated benefits and labor market regulations induce adjustments along either the wage or employment margins,

[18] To test the significance of the between-group coefficient difference, we conduct Fisher's permutation tests following Lian (2016)'s bootstrap inter-group coefficient difference test method. The subgroup regressions in the remaining part of the paper follow the same methods.

depending on relative adjustment costs and the regulation's incidence (Gruber, 1994; Gruber, 1995; Baicker and Chandra, 2006). Related literature shows that labor adjustment costs and reallocation frictions generate highly heterogeneous adjustment margins across firm sizes, with large firms smoothing shocks through internal buffers while small firms adjust through discrete employment changes (Davis and Haltiwanger, 1992). Taken together, these findings suggest that mandated labor costs may be absorbed along whichever margin is least costly, which is employment in larger firms and wages in smaller firms in our setting.

4.4 Robustness Checks

4.4.1 Alternative outdoor employer identification

In this subsection, we check whether our findings are sensitive to different treatment variables. First, based on ChatGPT's predictions of the proportion of outdoor workers, which is a continuous variable, we can construct dummy variables indicating whether specific firms are more likely to be involved in outdoor work than others, and then exploit the standard DID model.

The first two columns of Table 4, panel A, report the regression results with employees' income as the outcome variable using alternative definitions of dummy treatment variables to replicate column (3) of Table 2, panel A. In column (1), we can simply define the treatment group as those with *pOutdoor*> 0. In column (2), we can construct a more conservative treatment dummy variable by using the threshold of 0.2. Whether using the less strict definition or the more strict definition, the main results remain consistent. The results shown in Table 4, Panel B, present similar patterns, with the dismissal rate as the dependent variable.

*** Insert Table 4 about here ***

Another way to construct a treatment indicator is to abandon ChatGPT-based predictions and turn to a keyword-extraction method. Specifically, we first generate a keyword list based on the following outdoor-related industries: 1) agriculture, forestry, animal husbandry, and fishing; 2) mining; 3) production and supply of electricity, heat, gas, and water; 4) construction; 5) water, environmental, and public facilities management; and 6) transportation. Then we define 55 keywords[19] related to these outdoor industries in total. If a firm name contains at

[19] All the keywords are the Chinese synonymous expressions of the industry name. We list these 55 keywords in Appendix Table 3.

least one of these keywords, we identify it as an outdoor firm whose employees are all outdoor workers. Thus, we can construct another dummy treatment indicator, *Outdoor_keyword*, which directly denotes whether the firm name contains outdoor-related keywords. Using this keyword-extraction method, we identify 5.8% of the firms as outdoor firms, whose employees account for 7.9% of all employees in the sample. Considering that ChatGPT predicts more than half of the firms will be related to outdoor work, although most are only treated at a very low level, the keyword extraction method could yield a more conservative estimate. In column (3) of Table 4, the results are consistent with our baseline estimation for income (panel A) and dismissal probability (panel B), although the latter one turns insignificant.

Therefore, we can safely conclude that our baseline findings regarding outdoor firm employees' decreased total income and increased dismissal rate after the policy shock are not sensitive to the definition of the treatment and control groups.

4.4.2 Potential effects of between-industry market condition differences

Our baseline identification of outdoor firms is conducted at the firm level by assigning some firms to the outdoor group and others to the indoor group. Although such a definition is not entirely based on industry information, the classification results are still intrinsically related to it. A related concern is that our policy shock in 2010 could be confounded by other economic events following the Global Financial Crisis in 2008, and our treatment and control groups may experience divergent development conditions. For instance, due to unobserved policy changes around 2010 (i.e., the same time as the heat protection changes took effect), some indoor industries (e.g., IT, finance) might have grown extremely fast, while outdoor industries (e.g., construction) might have grown relatively slower. If that were true, the wage growth reduction might be driven by the between-industry market condition dispersion rather than the policy shock that we are interested in. However, since we already control for industry-year fixed effects, such an alternative explanation should be systematically excluded. In other words, since our definition of outdoor firms is not reliant on industry classifications, the valid variation embedded in our specification is the within-industry variation. Nevertheless, to mitigate the potential effects of any omitted-variable bias, we conduct two additional robustness checks in both panels of Table 4.

The first robustness check aims to reduce the reliance of our identification strategy on industry-level variations. In column (4) of Table 4, we construct another treatment variable, *high_pOutdoor*, which equals 1 for firms whose firm-level ChatGPT-predicted *pOutdoor* is higher than the median value of *pOutdoor* of its industry, and 0 otherwise. Take two firms both from the construction industry (i.e., the industry with the most outdoor firms in our sample) as an example: a firm working on designing buildings is mainly related to indoor operations (*high_pOutdoor*=0), while the other firm working on constructing buildings should be heavily related to outdoor activities (*high_pOutdoor*=1), although both firms belong to a so-defined outdoor industry. In other words, we are comparing how closely the two firms are related to outdoor operations within the same industry. Thus, the treatment effects arise from within-industry variation in *pOutdoor*, which effectively alleviates the endogeneity problem rooted in between-industry variation. As expected, column (4) of both panels of Table 3 shows that the interaction term between *Post* and *high_pOutdoor* remains significant at the 1% level. Therefore, we conclude that our main findings are not severely affected by the between-industry endogenous issues.

Second, we exclude from our sample those industries with higher average wage growth and adopt the new subsample to replicate the main regression in column (3) of Table 2. To identify which industries have higher wage growth rates, we first run regressions to estimate coefficients for each of the 18 industries in our sample, using the micro-level income panel of the sample city with wage growth rate as the dependent variable and a series of industry dummies as core explanatory variables. The regressions also include a series of personal attributes (i.e., *female*, *native*, ln(*age*), and its square), firm-level attributes (i.e., *State* and firm size, ln(*EmployeeNum*)), and year fixed effects. Appendix Figure 2 presents the industry rankings by wage growth rate. In column (5) of Table 4, we exclude the top four industries with the highest wage growth rates (i.e., mining, real estate, finance, and construction). The interaction terms remain significant at the 1% level in both panels, suggesting that the divergent growth trends among those booming industries do not drive the baseline effects.

4.4.3 Alternative strategy for labor dispatching

Another feasible and effective response of employers toward unfavorable policy shocks is to fine-tune their personnel administration by transferring some of their employees,

especially those affected by the policy, to so-called "labor dispatching firms." Labor dispatching firms hire a large number of workers and dispatch them to other firms to provide actual services. These firms are technically the direct employers of dispatched employees, although the employees' tasks are performed for other companies. Therefore, compared with workers who are directly hired by the firm they work for, those who are technically employed by a labor dispatching firm but still physically work for the original employer may be less protected under heat allowance policies. Many Chinese firms, especially large ones, have been known to use labor dispatching to avoid their obligation to pay certain social security-related benefits to their workers (Lo, 2023). In such an institutional context, if an outdoor firm transfers the employment relationship of its outdoor-working employees to a labor dispatching firm, the firm may avoid paying heat allowances to them, or at least be more likely to provide lower heat allowances. Thus, the firms can keep labor costs at the original level without suppressing employees' wage growth. If such an alternative strategy holds, the increasing dismissal probability observed in our baseline results may be an overestimation.

We test whether the firms do take such alternative measures in the following steps. First, as mentioned in the data description section, our original employer–employee-matched database includes 623 labor dispatching firms (which we do not include in our main analysis). After we re-include these labor dispatching firms in our sample, we can identify whether an employee who leaves their current job continues to work for a labor dispatching firm afterwards. If so, we can naturally infer that the employee does not leave their original employer but has simply transferred to a labor dispatching firm and may still be working for the original employer. We construct the following DID model with two-way fixed effects, as shown in Eq. (2):

$$Ln(Godispatch_{ijkt}) = \beta_1 * pOutdoor_j * Post_j + X_i + \delta_t + \mu_{kt} + \gamma_j + \varepsilon_{ijkt}, \quad (2)$$

where i refers to an employee, j refers to an employer, j refers to an industry, and t refers to a year-month. Our sample includes all employee-year-month observations for employees who leave their current job. Each observation is an employee i who leaves the original employer j in year-month t. The dependent variable, $GoDispatch1_{ijt}$, equals 1 if the employee i continues to work for a labor dispatching firm in year-month t+1 after leaving firm j in year-month t, and equals 0 if the next employer is a non-labor dispatching firm. Considering that the personnel

transfer may take some time, we also provide a less strict definition by allowing a gap of three months (*GoDispatch2*), six months (*GoDispatch3*), and one year (*GoDispatch4*) between the time the employee leaves the current job and the time when the employee starts to work for a labor dispatching firm. X_i refers to a series of employee-level attributes, including *Native*, *Female*, ln(*Age*), and the square term of ln(*Age*). δ_t refers to year-fixed effects, which absorb the single term *Post*. μ_{kt} is the industry-by-year fixed effects. γ_j is the employer fixed effects. ε_{ijkt} is the error term clustered by the employer. β_1 captures the net effect of the policy on whether the employees' next job is with a labor dispatch firm. All the continuous variables are winsorized at the 1% and 99% levels.

The summary statistics of the departed employees' sample are summarized in Appendix Table 9. About 2.9% of departing employees start their next job at a labor dispatching firm within the month after leaving their current job, according to *GoDispatch1*. The proportions would increase to 3.9% and 4.4% if we further allow for a three-month gap and a one-year gap, respectively, between the departed job and the new job at the labor dispatching firm.

The regression results following Eq. (2) are presented in Appendix Table 10. As shown by column (1) of Appendix Table 10, employees who leave their jobs in outdoor firms do not have a higher probability of continuing their career in a labor dispatching firm compared to those in the unaffected firms. By contrast, the coefficient is negatively significant. The results remain robust if we further relax the definition of the outcome variable from columns (2)–(4). In other words, outdoor firms do not systematically transfer their personnel to informal arrangements following the policy shock.[20] Therefore, we can conclude that employers do not take advantage of labor dispatching to exploit policy loopholes and bypass the negative impacts of heat protection laws. In other words, worsening employment stability could not be offset by firm-level strategic adjustments to personnel structure. This finding also suggests that the firm size reduction shown in Appendix Table 8 is unlikely to be driven by the transfer of employees from their original employers to labor dispatching firms.

[20] Another possibility is that affected firms might hire more new employees from labor dispatching firms. Due to data constraint, we cannot directly test such a hypothesis. However, we do not find significant increase of the total number of employees working for labor dispatching firms after policy shock relative to the pre-period. In other word, the policy shock does not boost the expanding of labor dispatching firms, which can partially argue that outdoor firms may not hire more workers through labor dispatching firms to substitute the loss of formal employees.

5. External Validity with Multi-City Evidence

To investigate the external validity of our findings, we extend our single-city analysis to a multi-city analysis in this section. We confine this analysis to the listed firms. It is noteworthy that results based on listed firms are very likely to provide a lower bound on the effect, because listed firms are more economically resilient and more heavily regulated than non-listed firms.

5.1 Provincial-Level Policies

Similar to the single-city analysis, we classify the provincial-level policy shock as raising the lowest permitted heat protection allowance. We manually look up policy details for each Chinese province and define whether each policy is a valid policy shock according to the following criteria. First, if a provincial-level policy is the first in the province to explicitly state the amount of the heat protection allowance, we define it as a rise in the allowance. Second, for provinces without provincial-level policies stating the exact heat protection allowance both before and after the national-level policy shock, we assume that the policy date is directly following the national-level mandatory heat protection allowance, which the central government announced in July 2012 (see the detailed introduction in Section 2.1). Notably, three provinces (Tibet, Qinghai, and Heilongjiang) are excluded because none of their cities experienced any extreme heat days during our sample period. Another two provinces released two rounds of heat protection policies before and after June 2012, but the detailed heat allowance standards remain unchanged; thus, they are excluded from our analysis.

Finally, we find that 26 out of the 31 provincial-level governments in China implemented heat protection policies or followed the national-level policy between 2009 and 2016. We summarize the details of provincial-level policies used in this section in Appendix Table 11. The policy announcement dates range from July 2010 to July 2015.

5.2 Empirical Specifications

The empirical identification strategy is a staggered DID model, as specified in Eq. (3):

$$Outcome_{jkt} = \beta_1 * Outdoor_j * Post_j + X_{jt} + Z_{kt} + \alpha_j + \delta_t + \varepsilon_{jt}, \quad (3)$$

where j refers to a listed firm, k refers to a city, and t refers to a year. $Outcome_{jkt}$ includes the average annual compensation of the employees (denoted by *EmployeeWage*) and the number of employees (denoted by *EmployeeNum*) of firm j of city k at the end of year t, which

approximately correspond to the employee-level indicators of income and dismissal probability, respectively. Unlike our single-city analysis, the names of listed firms typically contain little information because of their complex business scope, making it inappropriate to apply the ChatGPT prediction method. Therefore, we construct the treatment dummy variable *Outdoor* for all listed firms in outdoor industries, based on the industry codes in the data and following the industry classifications shown in Appendix Table 3. *Post* equals 1 after the announcement year of the provincial-level policy that increases the heat allowance for the province (similar to the previous regressions, the announcement year itself is omitted as the grace period), and 0 otherwise.

X and *Z* are two vectors of firm-level and city-level time-variant control variables. In line with several studies on listed firms (Gao et al., 2015; Focke et al., 2017; Dai et al., 2020), we include a set of control variables at the firm level, including the natural logarithm of total assets (*Size*); firm age (*Age*); the ratio of total debt to total assets (*Leverage*); market-to-book ratio (*M/B*); return on assets (*ROA*); the ratio of property, plants, and equipment to total assets (*PPER*); the average of monthly stock return for the past 12 months (*Return*); the standard deviation of monthly stock returns in the past 12 months (*Volatility*); the ratio of cash to total assets (*Cash*); and the growth rate of revenue (*Rev_Growth*), as well as some governance variables, including the total shareholding ratio of the top five shareholders (*Top5*); the ratio of independent directors to total directors (*Indept*); and whether the CEO and chairperson are the same (*Dual*). We also control the number of employees when we use the employee's wage as the outcome variable. City-level variables (*Z*) include GDP growth rate (*GDP_growth*), the natural logarithm of population (*Population*), and the growth rate of foreign direct investment (*FDI_growth*).

We control for year-fixed effects (δ_t) and firm-fixed effects (γ_{ij}). Thus, β_1 can be interpreted as the net effect of the policies on the income and firm size of the employees of interest. Unlike in the single-city database, we cannot control for industry-by-year fixed effects here because our treatment variable, *Outdoor*, is defined by industry classification and exhibits no within-industry variation. Again, we winsorize all the continuous variables at 1% and 99% levels and cluster standard errors at the firm level.

5.3 Empirical Results

5.3.1 Effects of provincial-level policies

The estimation results of staggered DID models are summarized in the first two columns of Table 5, with the effects on employees' wages in column (1) and the effects on firm size in column (2). Column (1) shows a significantly negative coefficient for the interaction term at the 5% significance level, indicating that the increase in the heat allowance can lead to a reduction in employees' total income relative to those working for unaffected firms. The magnitude of the effect is highly consistent with our single-city analysis. Nevertheless, according to the estimated coefficient shown in column (2) of Table 5, the heat protection policies do not lead to significant reductions in the number of employees in a firm, which is different from what we find in the single-city sample. A possible reason is that employees of listed firms are better protected by their labor contracts because they are always under public scrutiny.

*** Insert Table 5 about here ***

We also present the dynamic effects of provincial-level heat protection mandates on employees' wages in Figure 5. We adopt the year before the policy shock as the default group. As expected, no pre-trend exists two or three years before the policy shock. However, from the second year onward, following the more stringent policies imposed by provincial governments, negative effects emerge and persist until the end of the sample period. These findings further enhance the external validity of our findings from the single city.

*** Insert Figure 5 about here ***

5.3.2 Placebo tests for the cooler cities

To take full advantage of the multi-city database, we further extend the above analysis to cities without extreme heat days during our sample period of 2009–2016 to provide a placebo test. The intuition is simple: firms located in cool cities should not have been affected by any heat-protection policy. However, we cannot conduct a placebo test under the staggered DID setting specified by Eq. (3). Therefore, we treat the 2012 central-level policy as a single exogenous shock, assuming that all local governments would comply with it once it takes effect. *Post_National* equals 1 for the year after 2012 (the year 2012 is excluded as a grace period), and 0 otherwise. The remaining identifications are all the same as specified in Eq. (3). The regression result of such a placebo test is shown in column (3) of Table 4. Not surprisingly,

relative to indoor listed firms, outdoor listed firms located in cities without heat days do not decrease the compensation of their ordinary employees after the national-level heat protection policy shock in 2012.

6. Mechanism Analysis

Our previous sections suggest that outdoor-operation-related employers reduced their workers' income growth rates and dismissed some of them after the government required employers to provide higher heat allowances, and the magnitude of these negative effects on employee welfare is substantial. The next question is the mechanism underlying the baseline findings. In this section, we test two possible mechanisms. The major mechanism is from the wage margin: employers selectively transfer the extra labor cost back to the same employees. The other mechanism is from the employment margin: employers systematically adjust their operational structure, using machines to substitute for the more expensive human resources.

6.1 Selective offset on the wage margin

6.1.1 Employee-specific response

The intuition of the major mechanism is straightforward. Since the heat allowance applies only to outdoor workers in outdoor firms, the firm should deliberately suppress these workers' wage growth to shift the allowance back to the recipients. We investigate this response using a within-firm heterogeneity analysis.

Although job assignments are unobserved due to the lack of information about specific positions in our database, we can still infer that some types of employees within a single firm are more likely to be involved with outdoor operations than other employees based on gender and hierarchical position, which serve as credible within-firm proxies for outdoor task exposure. A most natural assumption is that male workers are more likely to be outdoor workers than female workers within the same firm-year. Additionally, lower-income workers should be more likely to perform outdoor duties directly than higher-income workers, who are more likely to be managers within the same firm in the same year. Furthermore, if we restrict the sample to lower-income workers within the firm, male workers should also be more likely to be responsible for outdoor operations compared to their female counterparts. Therefore, we could investigate whether the reverse effects concentrate more among these specific types of

workers within the same employer. If that is the case, we can infer that firms' wage-reduction responses are highly selective: they tend to pass on the increased costs back to employees who should have benefited from the mandate.

Accordingly, in the first two columns of Table 6 Panel A, we separately run the same regression following Eq.(1) on male workers and female workers. The results show that male workers are affected more by the heat protection policy, while the coefficient for females is insignificant. In the next two columns, we use low-income employees (i.e., those whose annual incomes are below the median of all workers in the firm during the year) as additional proxies for outdoor workers. The results again suggest that only the low-income employees are affected by the policy shock, while the high-income employees from the same outdoor firms are hardly affected. In the last two columns, we further focus on the gender differences among lower-income workers. All these lower-income workers should be more likely to take on labor-intensive tasks within the firm than higher-income executive-level employees. Still, male workers are more likely to work outdoors (e.g., on-site construction), while female workers are more likely to work indoors (e.g., basic paperwork). Consistent with our expectations, low-income male workers also suffered significantly more than the female workers from a similar hierarchy within the firm.

*** Insert Table 6 about here ***

These findings jointly indicate that, in response to the strengthened mandate, the affected firms offset it by rebalancing the internal compensation structure along the task margin, effectively passing the compliance costs back to the intended beneficiaries. In other words, despite the original policy intention to benefit more vulnerable workers (i.e., outdoor workers with high heat exposure), the firms' responses instead amplify their disadvantage in wage growth.

6.1.2 Firm-specific response

Apart from which employees take on the cost of the policy, a related question is which firms choose to shift the costs to employees. We hypothesize that firm-level financial conditions and government ability would lead to wage reduction. If an affected firm has a sound corporate governance structure or stronger financial capacity, it should be able to absorb the

rising costs on its own, for example by improving productivity and revenue, rather than passing the costs directly back to employees.

Table 7 explores the firm-level heterogeneity underlying the negative effects of the heat protection policy on employees' wages using the listed firm database. For corporate governance measures, we focus on CEO independence. Specifically, for each firm in our working sample, we use a dummy variable indicating whether the CEO and the board chairperson are the same person as a proxy for firm governance. As widely used in the corporate finance literature, CEO duality is a governance measure for listed firms (Daily and Dalton, 1997; Banerjee et al., 2020; Gao and Mohamed, 2018). CEO-chairperson duality firms have greater agency problems and tend to have less supervision. Consequently, this enables CEOs to exert greater bargaining power in their dealings with employees, thereby increasing the likelihood of mitigating escalating costs at the expense of employee welfare. To test this conjecture, we divide our sample firms into two groups based on CEO-chairperson duality. Columns (1) and (2) in Table 7, which summarize the regression results, only show that firms with independent CEOs barely cut employees' wage growth (column (1)), and the CEO-chairperson duality subsample drives our unintended consequences of wage growth reductions (column (2)). These results concur with the literature, which finds that firms with non-duality perform better in monitoring (Goergen, 2020). These findings are also consistent with Mandato and Devine (2020), who state that firms with CEO-chairperson duality have ineffective board oversight and may ignore the damage to employees. In conclusion, the dispersion in wages across firms with different levels of corporate governance independence indicates that more advanced corporate governance helps alleviate wage losses for employees due to the heat protection mandate.

*** Insert Table 7 about here ***

In columns (3) and (4) of Table 7, we further explore the heterogeneous effects of the heat protection policy on firms with different financial constraints. We measure firm-level financial constraints using the SA index, following Hadlock and Pierce (2010). Specifically, we divide the listed firms into two groups based on whether their SA index is above the industrial median. A higher SA index indicates greater financial pressure and worse financial status. Given that financial capacity limits firms' ability to absorb additional costs within the operational margin,

listed firms with a higher SA index should be more likely to cut employees' income than those with stronger financial capacity. The results are generally consistent with our expectations: only employees of the high-SA listed firms are negatively affected by the heat protection policy, while wage reductions among the low-SA group are insignificant.

Taken together, these findings support a selective wage incidence channel: firms facing tighter financial constraints and weaker governance capacities shift the compliance burden back to the likely allowance recipients of the mandate by disproportionately slowing their wage growth, effectively offsetting the intended transfer.

6.2 Capital-labor substitution on the quantity margin

6.2.1 Increasing fixed-asset investment

Existing studies about minimum wage policies and dismissal laws in the United States and China find that firms invest more in capital and technology instead of labor when the minimum wage is raised (Harasztos and Lindner, 2019; Geng et al., 2022; Bena et al., 2022; Dai and Qiu, 2024). If firms shift from a human workforce to machines, we expect them to increase fixed-asset investments to substitute for labor.

In this subsection, we provide suggestive evidence using a subsample of China's Industrial Enterprise Database, which covers 1,522 large industrial firms and can be further matched with our original database at the firm-year level by firm name. Similar to the listed firm data we use in Section 5, this data can only reflect how large firms respond to the mandate. As discussed in Section 4.3, large firms are more likely to behave on the quantity margin rather than adjust their workers' wage growth rates. Therefore, we can use this database to investigate how large firms leverage their scale advantages, particularly their greater organizational slack, to respond to the mandate.

We then construct a continuous DID model with fixed effects following Eq. (4):

$$rFixAsset_{jkt} = \beta_1 * pOutdoor_j * Post_j + X_j + \delta_t + \mu_{kt} + \varepsilon_{jkt}, \quad (4)$$

where j refers to an industrial firm, k refers to its industry, and t refers to a year. Each observation is a firm j from industry k at the end of year t. The dependent variable, $rFixAsset_{kjt}$, is the fixed asset amount normalized by total assets for firm j in industry k at the end of year t, and is used to measure the intensity of non-labor capital ownership among industrial firms. X_j refers to a set of firm-level attributes, including the debt-to-asset ratio (DA), the rate of return

on common stockholders' equity (*ROE*), and the current ratio (*CR*). δ_t refers to year-fixed effects, which absorb the single term *Post*. γ_j is the firm fixed effects. μ_{kt} is the industry-by-year fixed effects. ε_{ijt} is the error term clustered by firm. Thus, β_1 captures the net effect of the policy on industrial firms' fixed-asset investments. All the continuous variables are winsorized at the 1% and 99% levels. Firms with negative net assets are excluded.

Following Eq. (4), we first examine whether capital-labor substitution occurs among industrial firms after the mandate. As presented in column (1) of Table 8, the fixed-asset investments of affected outdoor-working industrial firms increase significantly at 10% level. For an outdoor industrial firm with an average policy exposure (*pOutdoor*=0.34), its fixed assets proportion among total assets would go up by 1.1 percentage points. Considering that the mean value of *rFixasset* is 23.6% for an average outdoor firm with positive policy exposure (*pOutdoor*>0) before 2010, such an increase can be translated into a 4.8% increase relative to the average fixed-asset investment level.

*** Insert Table 8 about here ***

To make the machine-labor substitution channel more complete, we turn to replicate the baseline results in column (3) of two panels of Table 2 using the industrial firm sample, which covers 276,039 employees working in 1,445 industrial firms. As shown in columns (2) and (3) of Table 8, the coefficient of the interaction term is insignificant for income (column (2)), but significantly positive for dismissal probability (column (3)). Taking the average outdoor firm again as an example, the dismissal probability rises by 3.9 percentage points after the policy shock compared to unaffected firms, accounting for approximately 20% of the average dismissal probability among outdoor firms during the pre-period (19.9%), which is an even larger effect than the baseline estimations based on full-sample analysis. These patterns are highly consistent with what we have found in Section 4.3: as large firms whose average personnel size exceeds 1,000 people, these industrial firms also emphasize more on employment margin (i.e., increasing employees' dismissal probability) rather than wage margin (i.e., slowing employees' wage growth) in response to increasing labor costs.

A natural follow-up question to firms' decreasing investment behavior is how total factor productivity (TFP) is affected. The intuition is that, since the capital-labor substitution occurs, the productivity of affected firms should improve. We again use the industrial firm sample

from 2008 to 2013 to calculate each firm's TFP each year. Specifically, we employ four standard measures to strengthen the robustness. The OLS TFP (denoted by *TFP_ols*) and FE TFP (denoted by *TFP_fe*) are computed as the residuals from a Cobb–Douglas value-added production function estimated respectively without and with firm fixed effects, following Brandt et al. (2012) for Chinese industrial firm data. We additionally use proxy-based semiparametric estimators—LP-ACF (Levinsohn and Petrin, 2003; Ackerberg et al., 2015), denoted by *TFP_lpacf*, and OP-ACF (Olley and Pakes, 1996; Ackerberg et al., 2015), denoted by *TFP_lpac*. In these two estimators, intermediate inputs or investment serve as proxies for unobserved productivity shocks, respectively. For both proxy estimators, we construct the proxy term following Bournakis and Mallick (2018) to mitigate endogeneity and improve robustness in value-added specifications. In all cases, TFP is defined as the Hicks-neutral residual net of factor inputs. Appendix Table 5 reports the summary statistics of each TFP indicator.

From columns (4) to (7) of Table 8, we adopt continuous DID approaches to test the effect of the heat protection policy on outdoor firms' TFP. The regressions also follow Eq. (4). Consistent with our expectation, we find robust positive effects at the 10% level for all four TFP indicators. For a representative outdoor firm (*pOutdoor*=0.34), after the policy shock, its TFP would be 11.8% higher than that of indoor firms, indicating substantial efficiency gains from the labor-capital shift.

We also present the dynamic effects in Appendix Figure 3, which shows no pre-trend in 2009 but exhibits long-lasting negative impacts after the policy shock from 2011 to 2013. These findings consistently indicate better performance among outdoor firms after the shift from labor to capital, relative to indoor firms, which are less affected.

6.2.2 Unselective layoffs

Although we cannot directly investigate capital-labor substitution using the full-sample database due to the lack of outcome variables, we can still provide some consistent evidence. As in Section 6.1.1, we also investigate whether the increasing dismissal probability is selective across employees within the same firm. As shown in Panel B of Table 6, the subgroup regression results differ from the patterns observed in wage reductions.

The first two columns of Table 6 suggest that both male and female employees at outdoor firms are significantly affected by the policy shocks, though the coefficient for male workers is larger. The next two columns provide consistent evidence: within the same firm-year, not only lower-income workers but also higher-income workers experience the same increase in dismissal probability, and the magnitude and significance level are very similar across the two subgroups. The last two columns also show no gender differences among low-income workers, with the magnitude among female workers slightly larger than that among males.

These findings have two implications. First, the unselective layoffs mean that the reverse policy effects are spilled over from outdoor-working employees to indoor-working employees. As long as the firm is under greater policy exposure, all employees, whether or not they are actual benefit recipients, would face higher dismissal rates. Second, the lack of within-firm heterogeneity in dismissal probability suggests potential structural adjustments among outdoor firms following the mandate, which is highly consistent with the systematic shift from labor to capital as we found using industrial firm data.

The above findings can support our mechanism: investing more in fixed assets would allow outdoor firms to take greater advantage of machines, thereby becoming less reliant on humans and leading to (non-selective) layoffs across all their employees. Nevertheless, it is noteworthy that the findings based on large industrial firms cannot be overinterpreted to imply that all outdoor firms can achieve productivity improvements amid intensified labor costs. The channel about "intensified fixed-asset investment — capital-labor substitution — ultimate productivity upgrade" may not work for a firm without large scale, strong financial capacity, and advanced governance skills. In other words, the capital-labor substitution mechanism may exist only among large firms.

7. Concluding Remarks

Although the doubling of the heat protection allowance in mid-2010 increased labor costs by only 180 yuan per person per year, the relative significance is non-negligible given the weak profitability of outdoor firms in China. Using a micro-level employer–employee matched panel database with annual total income information covering over two million workers in a large Chinese city between 2008 and 2013, we apply a continuous difference-in-differences

approach with employee fixed effects, industry trends, and time trends to examine the effect of this policy on firms and employees. We find that firms offset the explicit transfer of heat allowances implicitly: by slowing wage growth for workers. Specifically, workers in a representative outdoor-related firm with a 34% heat exposure probability see their wage growth rate decrease by approximately 1.8 percentage points relative to those in unaffected firms, leading to a 1,360 yuan wage loss per year, 7.6 times the allowance gains. Further, the dismissal probability at the employee-level also increases by approximately 2.5%. Private firms dominate both findings; state-owned firms have no significant effects. Large firms respond more to the employment margin; smaller firms rely more on the wage margin. The baseline results remain robust after a series of robustness checks and can be replicated using a multi-city, listed-firm database.

Furthermore, we test two distinct adjustment mechanisms. The major mechanism is selective wage incidence: firms (especially those with less advanced governance and worse financial capacity) transfer the additional costs by slowing wage growth for workers with higher expected take-up of the mandated benefit, reallocating the cost of compliance along the task margin. The complementary mechanism operates through capital–labor substitution on the quantity margin: higher labor costs induce (large and capable) firms to invest more in fixed assets, thereby reducing their reliance on labor, leading to non-selective dismissals across the workforce. The welfare loss among workers is obvious. On the one hand, the mandate ends up taxing the very workers it is designed to subsidize by reducing their wage growth. On the other hand, indoor workers may also suffer from capital-labor substitution and subsequent universal layoffs.

Our work sheds light on the unintended consequences of mandatory employee heat allowances. Once employers bear the economic cost of labor protection and corporate monitoring is ineffective, employers would more than offset the transfer through implicit wage cuts, making it difficult for employees to enjoy the policy's intended benefits. Such effects are more salient when employers are financially vulnerable and have weak corporate governance. Besides, the negative effects can even unexpectedly spill over to indoor workers when the capital-labor substitution process is accelerated. Therefore, the economic welfare of employees under the heat protection law is not straightforward. We call for government policymakers to

carefully evaluate protection laws before adoption. It may be helpful for a government to offer financial support to mitigate employers' labor cost pressure.

References

Acharya, V.V., A. Bhardwaj, T. Tomunen. 2023. Do firms mitigate climate impact on employment? Evidence from US heat shocks. NBER Working Paper: 31967.

Ackerberg, D.A., K. Caves, G. Frazer. 2015. Identification properties of recent production function estimators. *Econometrica*, 83(6): 2411-2451.

Addoum, J.M., D.T. Ng, A. Ortiz-Bobea. 2020. Temperature shocks and establishment sales. *Review of Financial Studies*, 33(3): 1331-1366.

Akee, R., L. Zhao, Z. Zhao. 2019. Unintended consequences of China's new labor contract law on unemployment and welfare loss of the workers. *China Economic Review*, 53: 87-105.

Baicker, K., Chandra, A. 2006. The labor market effects of rising health insurance premiums. *Journal of Labor Economics*, 24(3): 609-634.

Banerjee, A., M. Nordqvist, K. Hellerstedt. 2020. The role of the board chair—A literature review and suggestions for future research. *Corporate Governance: An International Review*, 28(6): 372-405.

Banker, R. D., D. Byzalov, L. T. Chen. 2013. Employment protection legislation, adjustment costs and cross-country differences in cost behavior. *Journal of Accounting and Economics*, 55(1): 111-127.

Bena, J., H. Ortiz-Molina, E. Simintzi. 2022. Shielding firm value: Employment protection and process innovation. *Journal of Financial Economics*, 146(2): 637-664.

Bournakis, I., S. Mallick. 2018. Investment, productivity and employment: evidence from UK firm-level data. *Journal of Productive Analysis*, 49(2–3): 247-273.

Brandt, L., J. V. Biesebroeck, Y. Zhang. 2012. Creative accounting or creative destruction? firm-level productivity growth in Chinese manufacturing. *Journal of Development Economics*, 97(2): 339-351.

Cooper R, Gong G, Yan P. 2018. Costly labour adjustment: General equilibrium effects of China's employment regulations and financial reforms. *Economic Journal*, 128(613): 1879-1922.

Choi, D., Z. Gao, W. Jiang. 2020. Attention to global warming. *Review of Financial Studies*, 33(3): 1112-1145.

Dai, Y., P.R. Rau, A. Stouraitis, W. Tan. 2020. An ill wind? Terrorist attacks and CEO compensation. *Journal of Financial Economics*, 135: 379-98.

Dai, X., Y. Qiu. 2024. Minimum wage hikes and technology adoption: Evidence from U.S. establishments. *Journal of Financial and Quantitative Analysis*, 59(4): 1620-1658.

Daily, C.M., D.R. Dalton. 1997. CEO and board chair roles held jointly or separately: Much ado about nothing? *Academy of Management Perspectives*, 11(3): 11-20.

Davis, S. J., Haltiwanger, J. 1992. Gross job creation, gross job destruction, and employment reallocation. *The Quarterly Journal of Economics*, 107(3): 819-863.

Fang, H., R. Zhang, L. Zhou. 2025. Corruption stereotype and the unintended consequences of an anti-corruption campaign: Evidence from the real estate sector in China. *Journal of Public Economics*, 249, 105474.

Focke, F., E. Maug, A. Niessen-Ruenzi. 2017. The impact of firm prestige on executive compensation. *Journal of Financial Economics*, 123: 313-36.

Friesen, J. 2001. Overtime pay regulation and weekly hours of work in Canada. *Labour Economics*, 8(6): 691-720.

Gao, H., J. Luo, T. Tang. 2015. Effects of managerial labor market on executive compensation: Evidence from job-hopping. *Journal of Accounting and Economics*, 59: 203-20.

Gao, N., A. Mohamed. 2018. Cash-rich acquirers do not always make bad acquisitions: New evidence. *Journal of Corporate Finance*, 50: 243-264.

Geng, H. G., Y. Huang, C. Lin, S. Liu. 2022. Minimum wage and corporate investment: Evidence from manufacturing firms in China. *Journal of Financial and Quantitative Analysis*, 57(1): 94-126.

Goergen, M., P. Limbach, M. Scholz-Daneshgari. 2020. Firms' rationales for CEO duality: Evidence from a mandatory disclosure regulation. *Journal of Corporate Finance*, 65: 101770.

Gruber, J. 1994. The incidence of mandated maternity benefits. *The American Economic Review*: 622-641.

Gruber, J. 1997. The incidence of payroll taxation: evidence from Chile. *Journal of Labor Economics*, 15(S3): S72-S101.

Guo, N., L. Zhang, R. Zhang, B. Zou. 2025. Migration restrictions and the migrant-native wage gap: The roles of wage setting and sorting. Working paper.

Hadlock, C. J., Pierce, J. R. 2010. New evidence on measuring financial constraints: Moving beyond the KZ index. *Review of Financial Studies*, 23(5): 1909-1940.

Harasztosi, P., Lindner, A. 2019. Who pays for the minimum wage?. *American Economic Review*, 109(8): 2693-2727.

Holmstrom, B., P. Milgrom. 1991. Multitask principal-agent analyses: Incentive contracts, asset ownership, and job design. *Journal of Law, Economics, and Organization*, 7: 24-52.

Holt, H, J.R. Hendrickson. 2017. Turning pink slips into red tape: The unintended effects of employment protection legislation. *Contemporary Economic Policy*, 35: 421-38.

Jardim, E., M. C. Long, R. Plotnick, E. Van Inwegen, J. Vigdor, H. Wething. 2022. Minimum-wage increases and low-wage employment: Evidence from Seattle. *American Economic Journal: Economic Policy*, 14(2): 263-314.

Jin, Z., F. W. Li, Y. Lin, Z. Zhang. 2024. Do firms adapt to rising temperatures? Evidence from establishment-level data. Working Paper.

Kato, T., C. Long. 2006. Executive compensation, firm performance, and corporate governance in China: Evidence from firms listed in the Shanghai and Shenzhen Stock Exchanges. *Economic Development and Cultural Change*, 54(4): 945-983.

Le, T. H., D. Park, C. Castillejos-Petalcorin. 2023. Performance comparison of state-owned enterprises versus private firms in selected emerging Asian countries. *Journal of Asian Business and Economic Studies*, 30(1): 26-48.

Levinsohn, J, A. Petrin. 2003. Estimating production functions using inputs to control for unobservables. *Review of Economic Studies*, 70(2): 317-341.

Leonardi, M., G. Pica. 2013. Who pays for it? The heterogeneous wage effects of employment protection legislation. *Economic Journal*, 123: 1236-78.

Li G, Fujiyama K, Wu C, et al. 2024. Employment protection, corporate governance, and labor productivity around the world. *Journal of International Financial Markets, Institutions and Money*, 92: 101978.

Lian, Y., 2016. BDIFF: Stata module to compute bootstrap and permutation tests for difference in coefficients between two groups. *Statal Software Components*.

Licker, R., K. Dahl, J.T. Abatzoglou. 2022. Quantifying the impact of future extreme heat on the outdoor work sector in the United States. *Elementa: Science of Anthropocene,* 10(1).

Lo, V. I. 2023. Labour dispatch in China: Flexibility and security in employment. *Peking University Law Journal*, *11*(2): 161-183. (in Chinese)

Mandato, J., W. Devine. 2020. Why the CEO shouldn't also be the board chair. *Harvard Business Review*, 3(4).

Melchiorre, M., E. Rocca. 2013. The unintended consequences of Italy's labour laws: How extensive labour regulation distorts the Italian economy. *Economic Affairs*, 33: 156-73.

Olley, G.S., Pakes, A. 1996. The dynamics of productivity in the telecommunications equipment industry. *Econometrica*, 64(6): 1263-1297.

Pankratz, N.M., C.M. Schiller. 2024. Climate change and adaptation in global supply-chain networks. *Review of Financial Studies*, 37(6): 1729-1777.

Park, J., N. Pankratz, A. Behrer. 2021. Temperature, workplace safety, and labor market inequality. Working paper, IZA Institute of Economics.

Somanathan, E., R. Somanathan, A. Sudarshan, M. Tewari. 2021. The impact of temperature on productivity and labor supply: Evidence from Indian manufacturing. *Journal of Political Economy*, 129(6): 1797-1827.

Stigler G J. The economics of minimum wage legislation. *American Economic Review*, 1946, 36(3): 358-365.

Su, Y., L. Cheng, W. Cai, J. Lee, S. Zhong, S. Chen, T. Li, X. Huang, C. Huang. 2020. Evaluating the effectiveness of labor protection policy on occupational injuries caused by extreme heat in a large subtropical city of China. *Environmental Research*, 186: 109532.

Van der Wiel, K. 2010. Better protected, better paid: Evidence on how employment protection affects wages. *Labour Economics*, 17: 16-26.

Vicedo-Cabrera A M, Scovronick N, Sera F, et al. 2021. The burden of heat-related mortality attributable to recent human-induced climate change. *Nature Climate Change*, 11(6): 492-500.

Xiao, Z. 2022. Labor exposure to climate change and capital deepening. Working Paper.

Yu, Y. , J. Huang, T. Zhou. 2023. The impact of extreme temperature on labor wage: Evidence from Chinese manufacturing firms. *Global Environmental Change*, 83: 102768.

Zivin, J. G., M. Neidell. 2014. Temperature and time allocation: Implications for climate change. *Journal of Labor Economics*, 32(1): 1-26.

Figures and Tables

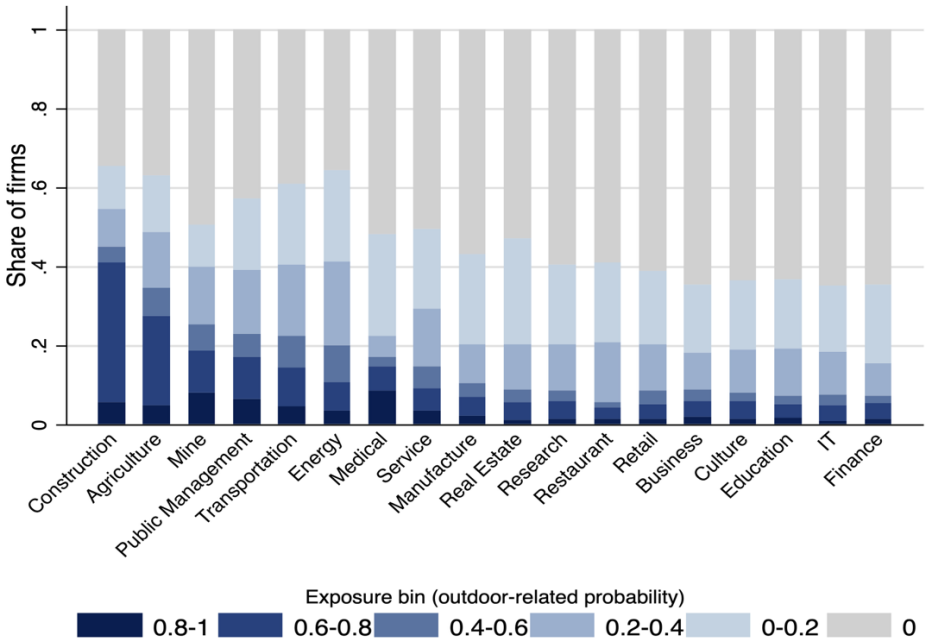
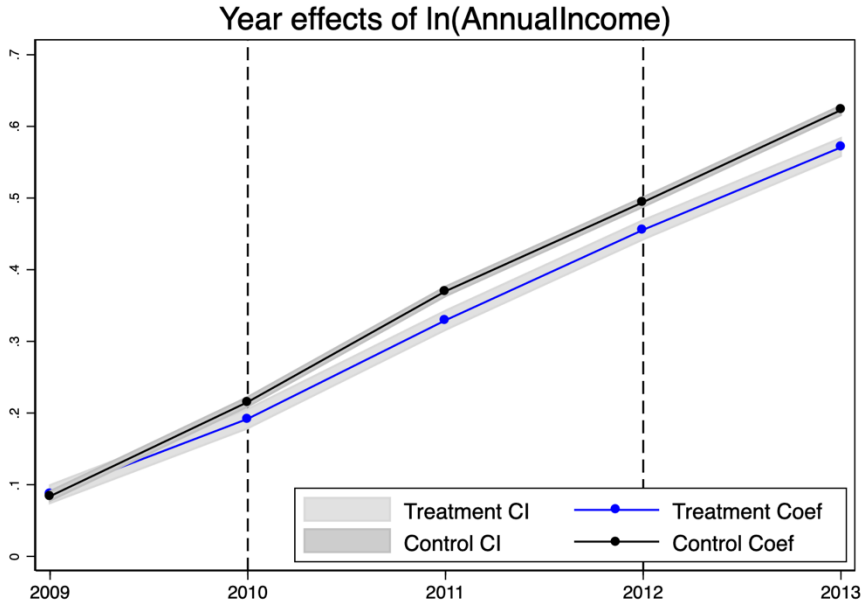
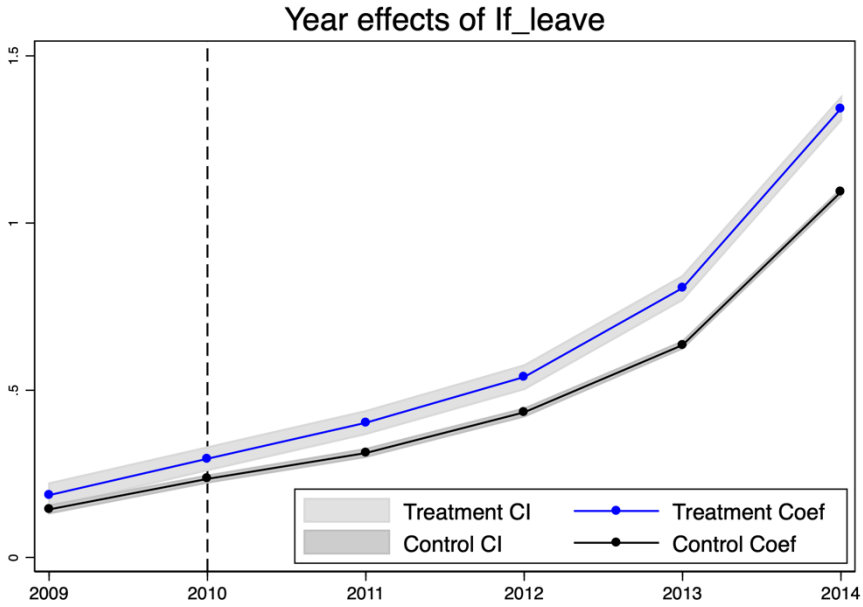


Figure 1. Distribution of Treatment Intensity by Industry

Notes: This figure depicts the distribution of firms across different heat-exposure intensities (i.e., *pOutdoor*) within industries, ranging from 0 (hardly related to outdoor work) to 1 (entirely outdoor work). Different stacks refer to different treatment intensities, and different bars denote different industries. The order of the bars is based on the average value of *pOutdoor*, as shown in Appendix Figure 1.



Panel A. Employees' Wages



Panel B. Employees' Dismissal Rate

Figure 2. Year Trends of Effects of Heat Protection Policy on Employment Outcomes

Notes: This figure reports the coefficients and 95% confidence intervals for year-fixed effects on employees' income (Panel A) and the dismissal rate (Panel B) for the treatment group (blue line) and the control group (black line). The year before 2008 is used as the default group. The vertical line denotes the time when the local government of the anonymous city increased the heat allowance to twice the prior rate.

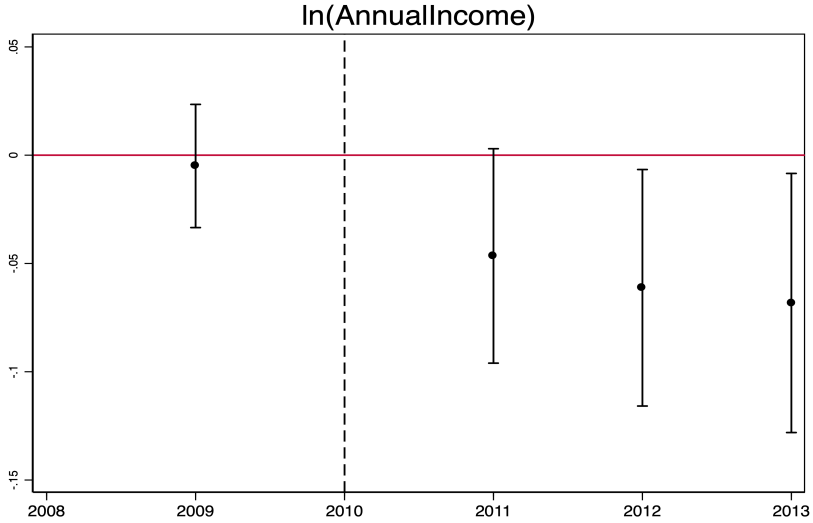


Figure 3. Dynamic Effects of Heat Protection Policy on Annual Income of Employees

Notes: This figure reports the dynamic effect of the heat protection policy on employees' annual income using a single-city micro-level database from 2008 to 2013. The year 2008 is used as the default group. The vertical line denotes the time when the local government of the anonymous city increased the heat allowance to twice the prior rate.

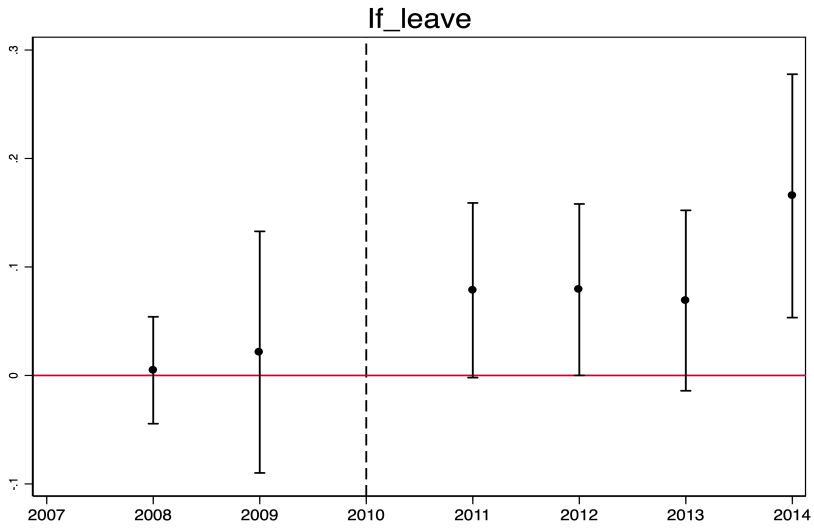


Figure 4. Dynamic Effects of Heat Protection Policy on Dismissal Probability

Notes: This figure reports the dynamic effect of the heat protection policy on the dismissal probability of employees at outdoor firms, using a single-city micro-level database from 2007 to 2014. The year 2007 is used as the default group. The vertical line indicates the time when the local government announced the heat protection policy (July 2010). Given that the policy shock occurred in mid-2010, we exclude 2010 and treat it as a grace period.

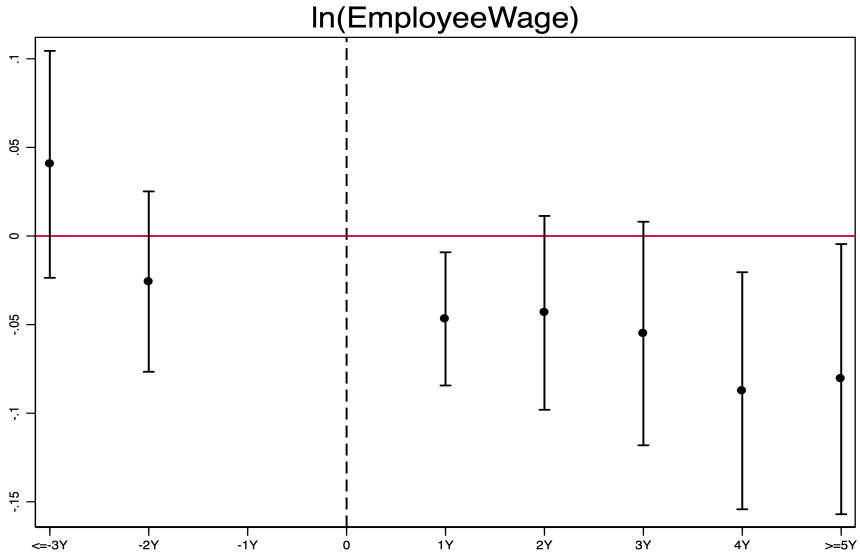


Figure 5. Dynamic Effects of Provincial-Level Heat Protection Policy on Employees' Wage: Multi-City Database

Notes: This figure reports the dynamic effect of the announcement of the provincial-level heat protection policy on the average wage of outdoor listed firms using the multi-city listed firm database from 2009 to 2016 under the staggered DID framework. The year before the policy shock (2009) is used as the default group. The vertical line denotes the time when the corresponding local government announced the provincial-level heat protection policy. Given that the policy shock occurred in 2010, we exclude 2010 and consider the period as a grace period.

Table 1. Summary Statistics of Single-City Micro-Level Database

Variables	Explanation	Obs.	Mean	Std. Dev.	Min	Max
Income	Annual salary for each employee each year, in yuan	6,627,842	68,182	50,431	6,500	208,500
If_leave	1 if the employee leaves the firm in the year, 0 otherwise	6,733,509	0.300	0.458	0	1
Post	1 after the heat protection policy was announced in 2010, 0 otherwise	6,627,842	0.675	0.468	0	1
pOutdoor	ChatGPT prediction about outdoor probability, ranging from 0 to 1	6,627,842	0.188	0.273	0	1
Outdoor_keyword	If the employer's name contains outdoor-related keywords, the value is 1,0 otherwise	6,627,842	0.0818	0.274	0	1
State	1 if the employer is state-owned, 0 otherwise	6,627,842	0.160	0.367	0	1
Female	1 if the employee is female, 0 otherwise	6,627,842	0.385	0.487	0	1
Age	Age of the employee	6,627,842	34.63	9.312	18	60
Native	1 if the employee is born in the same city as the employment city, 0 otherwise	6,627,842	0.547	0.498	0	1

Notes: This table reports the presentstistics of the singlforcity single-city database from 2008 to 2013. The sample covers 2,391,896 adult workers below retirement age who work for 21,821 employers from the formal labor sector of the anonymous city. Employees working for financial firms and labor dispatching firms are excluded from the working sample. Employers with no more than 10 employees and all labor dispatching firms are excluded from the working sample. For *If_leave*, the sample period is extended from 2007 to 2014, while we exclude employees who last appeared in the firm in December 2014.

Table 2. Effects of Heat Protection Policy on Employment

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Effects on Employees' Wages						
*Post * pOutdoor*	-0.0514***	-0.0514***	-0.0546**	-0.0546***	-0.0546**	-0.0546*
	(0.0008)	(0.0178)	(0.0219)	(0.0016)	(0.0227)	(0.0208)
Observations	5,633,201	5,633,201	4,477,469	4,477,469	4,477,469	4,477,469
R-squared	0.953	0.953	0.953	0.953	0.953	0.953
Employer–employee FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Industry-year FE	NO	NO	YES	YES	YES	YES
Cluster Level	NO	Employer	Employer	Employee	Industry	Employer and year
Panel B. Effects on Employees' Dismissal Probability						
*Post * pOutdoor*	0.0847***	0.0836***	0.0723***	0.0723***	0.0723***	0.0723***
	(0.0019)	(0.0018)	(0.0029)	(0.0035)	(0.0247)	(0.0167)
Observations	6,137,664	6,720,894	4,226,775	4,226,775	4,226,775	4,226,775
R-squared	0.382	0.143	0.449	0.449	0.449	0.449
Employer–employee FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Industry-year FE	NO	NO	YES	YES	YES	YES
Cluster Level	NO	Employer	Employer	Employee	Industry	Employer and year

Notes: Panel A reports the baseline effect of the heat protection policy in the anonymous city on employees' income with different specifications. Each observation of the sample is an employee-year. Panel B reports the baseline effect of the heat protection policy in the anonymous city on employees' dismissal probability with different specifications. Each observation of the sample is also an employee-year. In the most complete specification shown in column (3) of Panels A and B, we control for employee-employer fixed effects, year-fixed effects, and industry-by-year fixed effects. Robust errors are clustered at the firm level, individual level, and industry level in columns (3), (4), and (5), and two-way clustered at the firm level in column (6). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 3. Between-Firm Heterogeneity Effects of Heat Protection Policy on Employment

	(1)	(2)	(3)	(4)	(5)
Panel A. Effects on Employees' Wages					
	Private	State-owned	Small	Medium	Large
*Post * pOutdoor*	-0.0712***	0.0198	-0.0512***	-0.0520**	-0.0115
	(0.0252)	(0.0323)	(0.0183)	(0.0257)	(0.0580)
Observations	3,627,635	849,834	732,126	2,066,262	1,531,109
R-squared	0.955	0.943	0.955	0.955	0.960
Employer–employee FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Industry-year FE	YES	YES	YES	YES	YES
Panel B. Effects on Employees' Dismissal Probability					
	Private	State-owned	Small	Medium	Large
*Post * pOutdoor*	0.0956***	-0.1061	0.0178	0.0370	0.1353*
	(0.0245)	(0.0687)	(0.0200)	(0.0253)	(0.0715)
Observations	3,827,748	385,228	759,051	1,988,442	1,182,924
R-squared	0.451	0.527	0.514	0.483	0.506
Employer–employee FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Industry-year FE	YES	YES	YES	YES	YES

Notes: This table reports the firm-level heterogeneity by using subsamples of different ownership type (state-owned employers in column (1) and private firms column (2)), and using subsamples of different firm sizes (small firms with no more than 100 employees per year in column (3), medium firms with 100-1000 employees in column (4), and large firms with more than 1000 employees in column (5)). In all columns, we control for employee-fixed effects, year-fixed effects, and industry-by-year fixed effects. Robust errors are clustered at the firm level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 4. Robustness Checks of the Effects of Heat Protection Policy on Employment

	(1)	(2)	(3)	(4)	(5)
Panel A. Effects on Employees' Wages					
					Exclude industries with higher growth
Post * (*pOutdoor* >0)	-0.0267***				
	(0.0094)				
Post * (*pOutdoor* >0.2)		-0.0224**			
		(0.0110)			
Post * *Outdoor_keyword*			-0.0299*		
			(0.0176)		
Post * *High_pOutdoor*				-0.0183*	
				(0.0105)	
Post * *pOutdoor*					-0.0644***
					(0.0216)
Observations	4,477,469	4,477,469	4,477,469	4,470,304	3,724,025
R–squared	0.953	0.953	0.953	0.953	0.955
Employer–employee FE	YES	YES	YES	YES	YES
Industry-year FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Panel B. Effects on Employees' Dismissal Probability					
					Exclude industries with higher growth
Post * (*pOutdoor* >0)	0.0205***				
	(0.0070)				
Post * (*pOutdoor* >0.2)		0.0281***			
		(0.0105)			
Post * *Outdoor_keyword*			0.0119		
			(0.0190)		
Post * *High_pOutdoor*				0.0312***	
				(0.0098)	
Post * *pOutdoor*					0.0834***
					(0.0253)
Observations	4,226,775	4,226,775	4,226,775	4,226,775	3,546,394
R–squared	0.449	0.449	0.449	0.449	0.455
Employer-employee FE	YES	YES	YES	YES	YES
Industry-year FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Notes: This table reports the effects of the heat protection policy on employees' income (Panel A) and dismissal probability (Panel B) across different specifications of the treatment variables. The two variables from the first two columns are dummy treatment variables generated from the ChatGPT-predicted probability of outdoor working. Column (3) adopts the dummy variable based on a name-text-based keyword-extraction method. Column (4) turns to another treatment variable, *high_pOutdoor*, which denotes whether *pOutdoor* for the firm is higher than the industrial-mean-value. Column (5) excludes four industries with the highest wage growth rates from the control group; across all columns, we control for employer-employee fixed effects, industry-by-year fixed effects, and year fixed effects. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 5. Multi-City Evidence for Effects of Heat Protection Policies on Employment

Variables	(1) ln(*EmployeeWage*)	(2) ln(*EmployeeNum*)	(3) ln(*EmployeeWage*)
	Cities with heat days		Cities without heat days
*Outdoor * Post*	-0.0613**	0.0210	
	(0.0242)	(0.0464)	
*Outdoor * Post_National*			0.0289
			(0.0422)
Size	0.3768***	0.5693***	0.4475***
	(0.0190)	(0.0281)	(0.0449)
Age	0.0225	0.0031	0.0201
	(0.0182)	(0.0322)	(0.0223)
Leverage	0.0182	0.1002	-0.1888
	(0.0471)	(0.0905)	(0.1687)
M/B	0.0034	-0.0045	0.0012
	(0.0022)	(0.0040)	(0.0057)
ROA	0.1765	-0.1969	0.5345**
	(0.1128)	(0.1818)	(0.2217)
PPER	0.2484***	0.7563***	0.0534
	(0.0561)	(0.1233)	(0.1354)
Return	-0.3229***	-0.1653	-0.0618
	(0.1032)	(0.1693)	(0.3411)
Volatility	0.0484	0.0703	0.0909
	(0.0563)	(0.1050)	(0.2266)
Cash	-0.0689*	-0.1808***	-0.2296*
	(0.0380)	(0.0698)	(0.1200)
Rev_Growth	0.0221***	0.0168	-0.0012
	(0.0079)	(0.0122)	(0.0156)
Top5	-0.1107*	-0.1602	0.0609
	(0.0649)	(0.1211)	(0.1553)
Indept	-0.0984	0.0145	-0.6488**
	(0.0871)	(0.1663)	(0.2691)
Dual	-0.0261**	0.0100	0.0172
	(0.0121)	(0.0222)	(0.0373)
ln(*EmployeeNum*)	-0.5292***		-0.5457***
	(0.0189)		(0.0400)
GDP_growth	0.0015	0.0051*	0.0051
	(0.0018)	(0.0031)	(0.0051)
Population	0.0205	-0.0416	0.0684
	(0.0351)	(0.1028)	(0.0908)
FDI_growth	-0.0061	0.0046	-0.0087
	(0.0060)	(0.0101)	(0.0128)
Observations	11,841	11,868	1,128
R-squared	0.880	0.924	0.865
Employer FE	YES	YES	YES
Year FE	YES	YES	YES

Notes: This table reports the effect of the heat protection policy on the multi-city sample covering 26 provinces in China, where provincial governments announced heat protection policies at different times in 2009 and 2016. The first two columns report the effects on employees' average wage and the number of employees. The regressions follow a staggered DID specification in which the treatment group is only outdoor firms after the policy shock. The last column includes only cool cities that never experienced any heat days during the time window, covering 478 listed firms across 108 cities in 24 provinces. *Post_National* equals 1 after the central government announced the national-level heat production policy in June 2012, which was the first nationwide policy concerning heat protection since the 1960s. In all columns, we control for firm-fixed effects, year-fixed effects, and firm-level and city-level time-variant attributes. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 6. Within-Firm Heterogeneity of Effects of Heat Protection Policy on Employment

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Effects on Employees' Wages						
	Male	Female	Higher Income	Lower Income	Low Income Male	Low Income Female
*Post * pOutdoor*	-0.0573**	-0.0398	-0.0090	-0.0504**	-0.0620***	-0.0231
	(0.0224)	(0.0247)	(0.0203)	(0.0237)	(0.0234)	(0.0302)
Observations	2,785,084	1,692,385	1,895,253	2,298,389	1,308,112	990,277
R-squared	0.954	0.952	0.954	0.966	0.968	0.964
Employer–employee FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Industry-year FE	YES	YES	YES	YES	YES	YES
Panel B. Effects on Employees' Dismissal Probability						
	Male	Female	Higher Income	Lower Income	Low Income Male	Low Income Female
*Post * Outdoor*	0.0862***	0.0489**	0.0627**	0.0663***	0.0577***	0.0733**
	(0.0254)	(0.0205)	(0.0283)	(0.0204)	(0.0198)	(0.0292)
Observations	2,455,286	1,771,489	1,736,725	2,442,074	1,969,357	1,131,532
R-squared	0.441	0.461	0.456	0.473	0.474	0.472
Employer–employee FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Industry-year FE	YES	YES	YES	YES	YES	YES

Note: This table reports the within-employer heterogeneous analysis. Columns (1) and (2) use the subsample of male workers and female workers, respectively. The next two columns use a subsample of workers whose income is higher than the within-firm median level (column (3)) and lower than the within-firm median level (column (4)). Columns (5) and (6) exclude all the higher-income employees and use the subsample of workers who are lower-income male employees (column (5)) and lower-income female employees (column (6)). In all columns, we control for employer–employee fixed effects, industry-by-year fixed effects, and year-fixed effects. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 7. Multi-City Heterogeneity of Effects of Heat Protection Policy on Employees' Wages

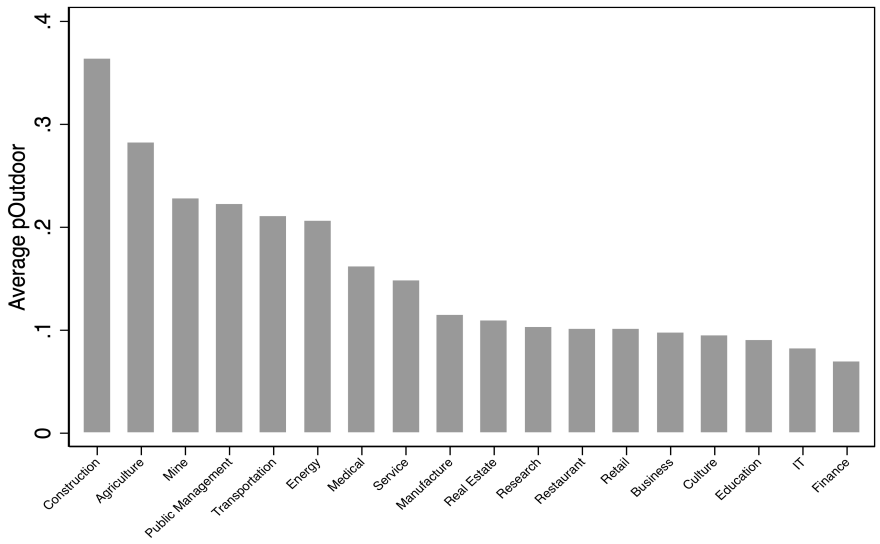
Variables	(1)	(2)	(3)	(4)
	ln(*EmployeeWage*)			
	Governance Level		Financial Constraints	
	Non-duality of CEO and chairperson	Duality of the CEO and chairperson	Higher SA	Lower SA
*Outdoor * Post*	-0.0103	-0.0568**	-0.0683*	0.0155
	(0.0716)	(0.0251)	(0.0386)	(0.0392)
Observations	2,703	8,873	2,695	2,826
R-squared	0.904	0.889	0.905	0.893
Employer FE	YES	YES	YES	YES
Time-variant Attributes	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Notes: This table reports the heterogeneous effects of the heat protection policy on employees' average wages using the listed firm sample. "Independent CEO" means the CEO and board chairperson are two different people, while "dependent CEO" means the CEO is also the chairperson. "Higher SA" means the SA index is higher than the industry median. "Lower SA" means the SA index is lower than the industry median. In all columns, we control for firm-fixed effects, year-fixed effects, and firm-level and city-level time-variant attributes. The standard errors are clustered at the firm level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 8. Effects of Heat Protection Policy on Firms' Performance

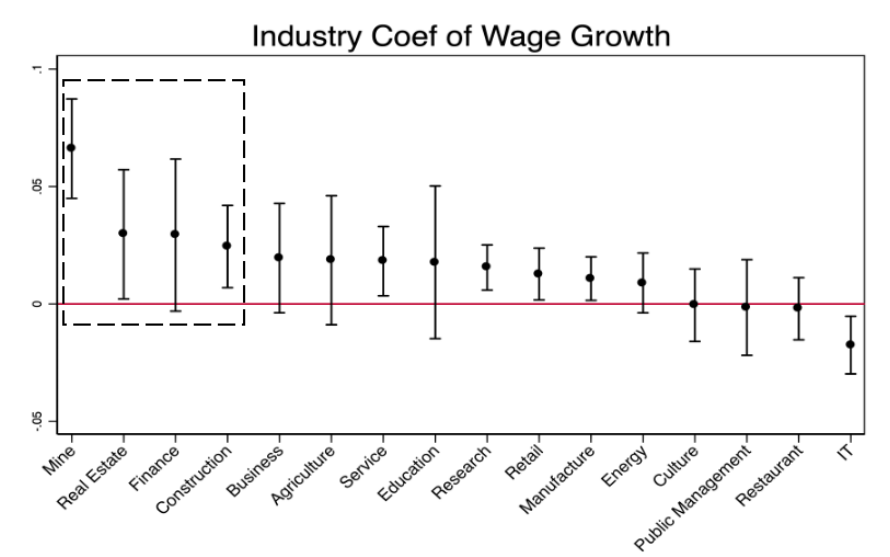
Variables	(1) *rFixAsset*	(2) ln(*EmployeeWage*)	(3) *If_leave*	(4) *TFP_fe*	(5) *TFP_ols*	(6) *TFP_lpacf*	(7) *TFP_opacf*
*Post * pOutdoor*	0.0336*	-0.0392	0.1139*	0.3457*	0.3193*	0.3193*	0.3326*
	(0.0182)	(0.0636)	(0.0640)	(0.1916)	(0.1903)	(0.1890)	(0.1958)
D/A	-0.0846***			-0.5314***	-0.5175***	-0.5181***	-0.5003**
	(0.0249)			(0.2017)	(0.1989)	(0.1983)	(0.1973)
ROE	-0.0281***			0.9728***	0.9414***	0.9439***	0.9468***
	(0.0088)			(0.1977)	(0.1857)	(0.1867)	(0.1857)
CR	-0.0053***			-0.0167**	-0.0091	-0.0097	-0.0113*
	(0.0009)			(0.0067)	(0.0064)	(0.0064)	(0.0065)
Observations	3,421	587,039	299,627	2,619	2,619	2,619	2,619
R-squared	0.898	0.945	0.514	0.864	0.822	0.824	0.824
Employer–employee FE	NO	YES	YES	NO	NO	NO	NO
Industry-year FE	YES	YES	YES	YES	YES	YES	YES
Employer FE	YES	NO	NO	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES

Notes: This table analyzes the effects of the heat protection policy on firms' performance using a sample of 1,522 industrial firms in the anonymous city between 2008 and 2013. Column (1) analyzes whether the heat protection policy in the anonymous city motivates the industrial firms to invest more in fixed assets (relative to staff). Columns (2) and (3) replicate the baseline results shown in column (3) of Table 2 using the 1,522 industrial firm sample merged with the employee-year level database. Columns (4) to (7) report the effect of the heat protection policy in the anonymous city on industrial firms' total factor productivity (TFP) in four different forms. In columns (2) and (3), we control for employee-employer fixed effects. In other columns, we control for employer fixed effects. In all columns, we control for industry-year fixed effects. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.



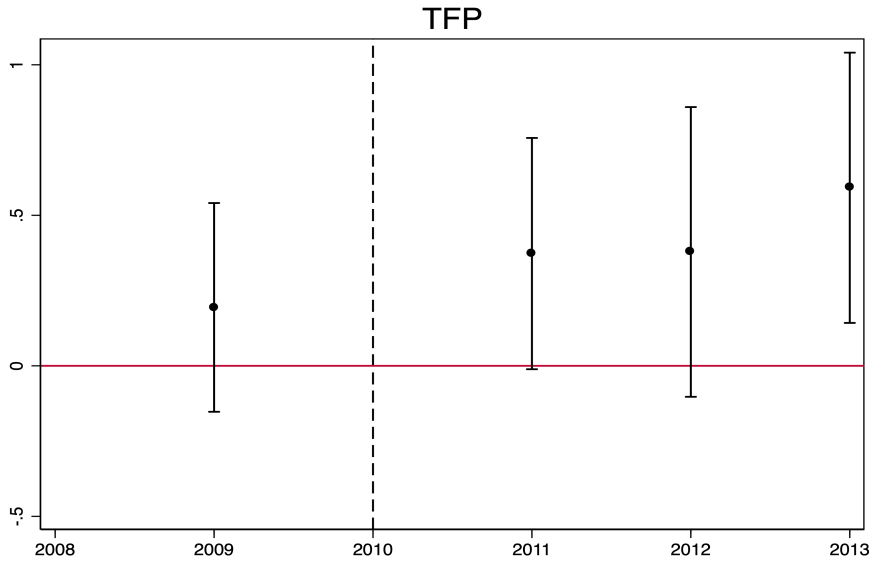
Appendix Figure 1. Estimated Average Outdoor-related Probability By Industry

Notes: This figure depicts the mean value of *pOutdoor* at the firm level by 18 industries.



Appendix Figure 2. Estimated Industry Coefficients of Wage Growth Rate

Notes: This figure reports the estimated coefficients for the wage growth rate of each industry dummy by using the sample before the heat production policy was announced in 2010 in the anonymous city. The regressions used to estimate the industry coefficients also include a series of personal attributes (i.e., *female*, *native*, *ln*(*age*), and its square for firm-level attributes (i.e., *State* and *ln*(*EmployeeNum*)), and year fixed effects as the dependent variables, and standard errors are clustered at the firm level. The omitted industry group is the medical industry. Industries marked by a dashed-line square are excluded from the sample used in column (5) of Table 4.



Appendix Figure 3. Dynamic Effects of Heat Protection Policy on TFP of Industrial Firms

Notes: This figure reports the dynamic effect of the announcement of the national-level heat protection policy on the total factor productivity (TFP) of industrial firms using the subsample of 1,522 firms from 2008 to 2013. The year 2008 is used as the default group. The vertical line marks the time when the local government announced the heat protection policy. Given that the policy shock occurred in mid-2010, we exclude 2010 and treat it as a grace period.

Appendix Table 1. Policy Details of Anonymous City

<table>
<tr><th>Announcement Date</th><th>Quote about heat allowance</th><th>Monthly Minimum Standard</th></tr>
<tr><td>2007/7/17</td><td>The high-temperature allowance for outdoor open-air workers shall be paid at a rate of not less than 60 yuan per person per month.</td><td>60 yuan</td></tr>
<tr><td>2010/7/6
(Policy Shock)</td><td>Since July 2010, the high-temperature allowance for outdoor open-air workers has been adjusted to no less than 120 yuan per person per month…</td><td rowspan="3">120 yuan</td></tr>
<tr><td>2012/7/12</td><td rowspan="2">From June to August each year, the high-temperature allowance for outdoor open-air workers shall be no less than 120 yuan per person per month.</td></tr>
<tr><td>2013/5/12</td></tr>
</table>

Notes: This table summarizes the four heat protection policies in the anonymous city between 2007 and 2014. The 2010 policy, which raised the heat allowance from 60 yuan to 120 yuan per person per month, is used to define *Post* in the main regressions.

Appendix Table 2. *t*-Test of Revenue between Indoor and Outdoor Firms

	Indoor Firms			Outdoor Firms			Diff.
	Obs.	Mean	Std. Err.	Obs.	Mean	Std. Err.	
Profit	30,831	1754.5730	29.1607	3,570	1082.9240	64.2531	-671.6491***
EmployeeNum	34,477	99.2286	1.9019	3,838	129.8051	7.6626	30.5765***
P_Profit	27,591	13.5478	0.1980	3,370	9.2734	0.4629	-4.2744***
ROE	29,289	0.05516	0.0009	3,060	0.0373	0.0024	-0.0179***

Notes: This table reports the *t*-test results for two indicators comparing indoor and outdoor firms, using the 2009 National Taxation Survey data from the anonymous city used in the main analysis. *Profit* denotes the net profit of the firm at the end of the year (in thousand yuan). *EmployeeNum* denotes the total number of employees at the end of the year. *P_Profit* is the net profit per employee (in thousand yuan), which measures the profit generated by each employee. *ROE* equals the ratio of profit to total equity. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table 3. Keywords List to Define Outdoor Industries

Categories	Keywords (in Chinese)	Number of firms (Percentage)
Agriculture, forestry, animal husbandry, and fishing (8 keywords)	Greening (*lv hua*); Farming (*yang zhi*); Planting (*zhong zhi*); Orchard (*guo yuan*); Animal husbandry (*xu mu*); Fishing (*yu ye*); Agriculture (*nong ye*); Agricultural machinery (*nong ji*)	413 (15.9%)
Mining (6 keywords)	Mining (*kuang ye*; *kai cai*); Metallurgy (*ye jin*); Coal (*mei kuang*); Iron (*kuang shi*); Ore (*tie kuang*)	83 (3.2%)
Production and supply of electricity, heat, gas, and water (8 keywords)	Power generation (*fa dian*); Grid (*dian wang*); Electricity (*dian li*); Electricity, etc.'s inspection (*jian xiu*); Heating (*gong re*; *gong nuan*; *re li*); Water (*shui wu*)	488 (15.7%)
Construction (8 keywords)	Building (*jian zhu*); Building materials (*jian cai*); Construction (*shi gong*); Construction supervision (*gong cheng jian li*); Construction site (*gong di*); Cement (*shui ni*); Concrete (*hun ning tu*); Housing construction (*zhu fang jian she*)	775 (29.9%)
Water, environmental, and public facilities management (16 keywords)	Water (*zi lai shui*); Environmental protection (*huan bao*); Roads and bridges (*lu qiao*); Road maintenance (*dao lu yang hu*); Road rescue (*jiu yuan*); Ships (*chuan bo*); Marine (*hang hai*); Shipping (*hang yun*); Ports (*gang kou*); Meteorology (*qi xiang*); Geology (*di zhi*); Security (*bao an*); Sanitation (*huan wei*); Airports *(ji chang*); Infrastracture construction (*ji chu she shi jian she*); Municipalities (*shi zheng*)	508 (19.6%)
Transportation (9 keywords)	Transportation (*yun shu*); Freight (*huo yun*); Logistics (*wu liu*); Courier (*kuai di*); Portage (*ban yun*); Move house (*ban jia*); Load or unload (*zhuang xie*); Delivery (*yun zai*); Rapid transit (*gui dao jiao tong*)	406 (15.7%)

Notes: This table summarizes the 55 keywords (in both English and Chinese, by six categories) we use to define *Outdoor_keyword* used in column (3) of Table 4. The last column counts the number of firms for each industry and the corresponding proportion among all outdoor firms.

Appendix Table 4. Summary Statistics of Multi-City Database

Variables	Explanation	Obs.	Mean	Std. Dev.	Min	Max
EmployeeNum	Number of employees	13,642	4,035	7,215	44	74,453
EmployeeWage	Annual average wage of ordinary employees, in thousand yuan	12,824	95.75	66.90	21.61	487.2
ManagerWage	Annual average wage of executive employees, in thousand yuan	13,632	466.3	350.8	62.03	2,084
Outdoor	1 if the listed firm belongs to the outdoor industry, 0 otherwise	13,642	0.103	0.304	0	1
Post	1 after provincial level policy shock, 0 otherwise	13,642	0.554	0.497	0	1
Post_National	1 after national level policy shock in June 2012, 0 otherwise	13,642	0.579	0.494	0	1
Size	The natural logarithm of total assets	13,642	21.89	1.188	19.63	25.44
Age	The age of the firm	13,642	15.16	5.260	3	28
Leverage	Total debt divided by total assets	13,642	0.427	0.217	0.0454	0.899
M/B	The market value of equity divided by the book value of equity	13,642	4.360	3.879	0.750	27.46
ROA	Profits divided by total assets	13,642	0.0405	0.0502	-0.152	0.191
PPER	The ratio of fixed assets to total assets	13,642	0.224	0.167	0.00231	0.720
Return	Average monthly stock return for the past 12 months	13,642	0.0284	0.0550	-0.0677	0.261
Volatility	Standard deviation of monthly stock returns for the past 12 months	13,553	0.148	0.0774	0.0551	0.552
Cash	The ratio of cash to total assets	13,642	0.206	0.153	0.0134	0.724
Rev_Growth	The growth rate of revenue from year t−1 to year t	12,491	0.203	0.533	-0.569	3.931
Top5	The sum shareholding ratio of the top five shareholders	13,642	0.536	0.156	0.182	0.871
Indept	The percentage of independent directors to total directors	13,642	0.371	0.0513	0.308	0.571
Dual	1 if the CEO and chairperson are the same person, and 0 otherwise	13,500	0.260	0.439	0	1
GDP_growth	City GDP growth rate from year t−1 to year t	13,470	9.992	2.680	3.350	17.50
Population	The natural logarithm of the city population	13,470	15.53	0.655	13.64	17.33
FDI_growth	The growth rate of foreign direct investment from year t−1 to year t	13,369	0.0992	0.348	-0.753	2.323

Notes: This table presents summary statistics for the multi-city listed firm database from 2009 to 2016. Only non-financial listed firms located in cities with at least one heat day (i.e., the daily temperature exceeds 35°C) during the time window are included. The sample covers 2,769 listed firms across 242 prefectural cities in 26 provinces. The last three city-level variables are collected from the Resource and Environmental Science Data Platform (RESDP). Other firm-level variables are collected from the China Stock Market and Accounting Research.

Appendix Table 5. Summary Statistics of China's Industrial Enterprise Database

Variables	Explanation	Obs.	Mean	Std. Dev.	Min	Max
Post	1 after the heat protection policy was announced in 2010, 0 otherwise	4,765	0.613	0.487	0	1
pOutdoor	ChatGPT prediction about outdoor probability, ranging from 0 to 1	4,765	0.130	0.215	0	1
TFP_ols	OLS Cobb-Douglas TFP residual	3,705	4.604	1.175	0.570	7.450
TFP_fe	FE Cobb-Douglas TFP residual	3,705	7.423	1.283	3.570	11.02
TFP_lpacf	Levinsohn–Petrin TFP (ACF-corrected).	3,705	4.831	1.176	0.754	7.744
TFP_opacf	Olley–Pakes TFP (ACF-corrected)	3,705	5.184	1.172	1.218	8.055
rFixAsset	Fixed assets normalized by total assets	4,765	0.192	0.179	0	0.965
DA	Debt-to-asset ratio	4,765	0.492	0.242	0.0208	1
ROE	Rate of return on common stockholders' equity	4,759	0.0909	0.316	-2.821	3.384
CR	Current Ratio	4,747	2.570	3.356	0.158	29.95

Notes: This table reports summary statistics for the industrial firm database for the anonymous city from 2008 to 2013, which are used in Table 8. Firms with negative net assets are excluded. The working sample covers 1,522 employers. Continuous variables are winsorized at the 1% and 99% levels by year.

Appendix Table 6. Effects of Heat Protection Policy on Income Growth Rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variables	*EmployeeWageGrowthRate*						
*Post*pOutdoor*	-0.0322***	-0.0398***	-0.0398***	-0.0317**	-0.0317***	-0.0317**	-0.0317*
	(0.0008)	(0.0008)	(0.0125)	(0.0139)	(0.0013)	(0.0159)	(0.0132)
ln(*Age*)		-1.5791***	-1.5791***	-1.6065***	-1.6065***	-1.6065***	-1.6065***
		(0.0123)	(0.0731)	(0.0849)	(0.0137)	(0.1222)	(0.1026)
$\ln(Age)^2$		0.2001***	0.2001***	0.2035***	0.2035***	0.2035***	0.2035***
		(0.0017)	(0.0100)	(0.0116)	(0.0019)	(0.0166)	(0.0137)
Native		-0.0126***	-0.0126***	-0.0131***	-0.0131***	-0.0131***	-0.0131***
		(0.0003)	(0.0009)	(0.0010)	(0.0003)	(0.0017)	(0.0013)
Female		-0.0076***	-0.0076***	-0.0077***	-0.0077***	-0.0077***	-0.0077***
		(0.0002)	(0.0005)	(0.0006)	(0.0002)	(0.0005)	(0.0007)
Observations	4,895,873	4,895,873	4,895,873	3,908,445	3,908,445	3,908,445	3,908,445
R-squared	0.130	0.151	0.151	0.157	0.157	0.157	0.157
Employer FE	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES
Industry-year FE	NO	NO	NO	YES	YES	YES	YES
Cluster Level	NO	NO	Employer	Employer	Employee	Industry	Industry and Year

Notes: This table reports the effect of the heat protection policy in the anonymous city on employees' income growth rates with different specifications. Each observation of the sample is an employee-year. The dependent variable is the annual wage growth rate from year t-1 to year t for each employee in each year t. In all columns, we control for employer fixed effects and year fixed effects. In column (2), we further control for individual attributes, including age, age in square terms, gender, and whether the employee was born in the same city. In column (3), we adopt robust errors clustered at the firm level. In column (4), we further control for industry-year fixed effects. In the last three columns, we adopt robust errors clustered at the employee level, industry level, and two-way clustered at the industry and year level, respectively. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table 7. Robustness Checks of Different Identifications of Dismissal Probability

	(1)		(2)	(3)
		OLS		Logit
Variables	*If_leave*		*If_leave*	*If_leave*
	Gap>6 months		Gap>1 year	Gap>3 months
*Post*Outdoor*	0.0416***		0.0439***	0.6513***
	(0.0153)		(0.0150)	(0.0499)
Observations	5,144,106		5,144,106	2,933,477
(Pseudo) *R*-squared	0.446		0.451	0.660
Employer-employee FE	YES		YES	YES
Industry-Year FE	YES		YES	YES
Year FE	YES		YES	YES

Notes: This table replicates the regression in column (3) of Table 2, Panel B. In the first two columns, we define *If_leave* as whether the employee leaves the current employer and is not hired by a new employer within 6 months or 1 year, respectively. In the last column, we use Logit instead of OLS, while the definition of *If_leave* remains consistent with our main analysis. In all columns, we control for employer–employee fixed effects, industry-by-year fixed effects, and year-fixed effects. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table 8. Effects of Heat Protection Policies on Firm Size

Panel A. Summary Statistics of Firm-Year Panel

Variables	Explanation	Obs.	Mean	Std. Dev.	Min	Max
EmployeeNum	Total number of employees during the year	145,787	133.6	247.3	11	1,784
Post	1 after the heat protection policy was announced in 2010, 0 otherwise	145,787	0.714	0.452	0	1
pOutdoor	ChatGPT prediction about outdoor probability, ranging from 0 to 1	145,787	0.114	0.219	0	1
State	1 if the employer is state-owned, 0 otherwise	145,753	0.388	0.487	0	1

Panel B. Regression Results

	(1)	(2)	(3)
Variables		ln(*EmployeeNum*)	
	All	Private	State-owned
*Post * pOutdoor*	-0.0685***	-0.0670**	-0.0230
	(0.0264)	(0.0330)	(0.0426)
Observations	82,472	62,860	19,594
R-squared	0.915	0.905	0.945
Employer FE	YES	YES	YES
Industry-year FE	YES	YES	YES
Year FE	YES	YES	YES

Notes: Panel A reports the summary statistics of the employer-employee-matched-data aggregated to the employer-year level from 2007 to 2014. The sample covers 31,572 employers. Employers with no more than 10 employees are excluded from the working sample. Panel B reports the effect of the heat protection policy in the anonymous city on employee size using the employer-level panel. Columns (2) and (3) conduct heterogeneity analysis based on subsamples divided by ownership type. In all columns, we control for employer fixed effects, industry-by-year fixed effects, and year fixed effects. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table 9. Summary Statistics of the Departed Employee Database

Variables	Explanation	Obs.	Mean	Std. Dev.	Min	Max
Post	1 after the heat protection policy was announced in 2010, 0 otherwise	3,244,289	0.970	0.171	0	1
pOutdoor	GPT prediction of outdoor-related probability, ranging from 0 to 1	3,244,289	0.160	0.262	0	1
GoDispatch1	1 if the next job is in a labor dispatch firm and the gap between the two jobs is shorter than one month, 0 otherwise	3,244,289	0.0286	0.167	0	1
GoDispatch2	1 if the next job is in a labor dispatch firm and the gap between the two jobs is shorter than three months, 0 otherwise	3,244,289	0.0387	0.193	0	1
GoDispatch3	1 if the next job is in a labor dispatch firm and the gap between the two jobs is shorter than six months, 0 otherwise	3,244,289	0.0441	0.205	0	1
GoDispatch4	1 if the next job is in a labor dispatch firm and the gap between the two jobs is shorter than one year, 0 otherwise	3,244,289	0.0512	0.221	0	1
Native	1 if the employee is born in the same city, 0 otherwise	3,244,289	0.484	0.500	0	1
Female	1 if the employee is female, 0 otherwise	3,244,289	0.403	0.490	0	1
Age	Age at which they left the job	3,244,289	35.86	9.707	19	65

Notes: This table reports summary statistics for a sample of departed employees from the single-city database from 2007 to 2014. Employees who left their jobs in December 2014 (i.e., the last month of our sample period) are excluded. The sample covers 1,696,179 employees who once worked for 29,523 employers. Employers with fewer than 10 employees are excluded from the working sample.

Appendix Table 10. Effects of Heat Protection Policy on Labor Dispatch

Variables	(1) *GoDispatch1*	(2) *GoDispatch2*	(3) *GoDispatch3*	(4) *GoDispatch4*
	No gap	Gap<3 month	Gap<6 months	Gap<1 year
*Post * pOutdoor*	-0.0161***	-0.0144***	-0.0130**	-0.0077
	(0.0049)	(0.0052)	(0.0056)	(0.0060)
Native	-0.0012**	0.0005	0.0022***	0.0048***
	(0.0005)	(0.0006)	(0.0006)	(0.0007)
Female	-0.0025***	-0.0040***	-0.0047***	-0.0055***
	(0.0005)	(0.0006)	(0.0006)	(0.0006)
ln(*Age*)	-0.0181***	-0.0324***	-0.0414***	-0.0524***
	(0.0018)	(0.0020)	(0.0021)	(0.0022)
$\ln(Age)^2$	-0.0503***	-0.0568***	-0.0565***	-0.0567***
	(0.0080)	(0.0090)	(0.0091)	(0.0089)
Observations	2,727,365	2,727,365	2,727,365	2,727,365
R-squared	0.189	0.166	0.158	0.152
Employer FE	YES	YES	YES	YES
Industry-year FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Notes: This table examines whether the heat-protection policy in the anonymous city increases the probability that an employee who leaves an outdoor firm will join a labor dispatch firm. Each observation is an employee who leaves their current job. The outcome variable equals 1 if their next job is through a labor dispatch firm and 0 otherwise. The defined month intervals between leaving the current job and working for a labor dispatch firm range from zero months (i.e., the strictest definition) to one year (i.e., the loosest definition) from column (1) to column (4). For brevity, we do not report the coefficients of control variables. In all columns, we control for employer-fixed effects, industry-by-year fixed effects, and year-fixed effects. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table 11. Details of Provincial-Level Heat Protection Policies in Table 4

Policy Announcement Date	Province
July 2010	The anonymous city used in the main analysis
June 2011	Jiangsu
August 2011	Fujian
May 2012	Xinjiang
June 2012	*Yunnan, Inner Mongolia, Jilin, Sichuan, Ningxia, Anhui, Guangdong, Guangxi, Hebei, Henan, Hainan, Gansu, Guizhou, Liaoning, and Chongqing*
August 2012	Jiangxi
May 2013	Tianjin, Shanxi
September 2013	Hubei
June 2014	Zhejiang
July 2015	Shandong, Shaanxi

Notes: This table summarizes the heat protection policies of 26 provinces used in Table 5. The policy shock date for raising (or first mentioning) the heat allowance is used to define *Post* in the first two columns of Table 5. The dates in italics indicate that local governments did not announce the policy but followed the requirements of the June 2012 national-level heat protection policy.