Cover Page

Sai Luo
Email: luosaiecon@gmail.com
Affiliation: Shanghai University of Finance and Economics
PhD Completion: 2020, University of Maryland (College Park)
Website: http://sailuo.weebly.com

Summary of Current Research Activity

My research focuses on labor economics in the context of both the United States and China, with an emphasis on labor market dynamics and the factors shaping behavior within these markets. I employ diverse methodologies, including field experiments, primary surveys, historical census records, and restricted-access administrative data.

In a recent experiment (with Jing Cai and Shing-Yi Wang) at an auto manufacturer in China, we randomized worker compensation and monitoring levels to test efficiency wage theory. We found that higher compensation increases worker hours, while higher monitoring enhances performance. Another project (with Hongbin Li and Yang Wang) analyzes how ideological education in Chinese high schools influences students' political and career choices using a college graduate survey we collected (now *R&R* at *Review of Economics and Statistics*). Additionally, a study (with Judith Hellerstein and Sergio Urzua) examines how diverging cognitive skill distributions across American cohorts impact the changing returns to these skills (now *R&R* at *JPE: Microeconomics*).

My ongoing research leverages full-count historical U.S. census data to explore how ethnic enclaves affect the economic assimilation of immigrant children. I also use China's college entrance exam administrative data to investigate how a lack of women role models contributes to the gender gap in college STEM majors.

Distinctive Perspective for the Summer Institute's Research Community

My research spans both U.S. and China topics, allowing me to offer a comparative perspective on economic phenomena across these two major economies. My access to administrative datasets and ability to conduct field experiments in China also provide unique empirical insights that can enrich the institute's discussions on global economic policy and development.

Goals and Expected Benefits from Participation

I hope to share my research with a broader audience and gain feedback from leading scholars. I would like to also explore potential collaborations in future work.

Preferred Summer Institute Session(s)

My research agenda aligns best with the Labor and the Development sessions, as my work covers labor topics in the context of both the U.S. and China. My randomized experiment work in an auto manufacturer, which studies how work compensation and monitoring affects firm and worker performance, also aligns well with the Personnel session.

Thank you for considering my application. I look forward to the possibility of contributing to and learning from the Summer Institute's research community.

Sai Luo
Email: luosaiecon@gmail.com
Website: http://sailuo.weebly.com

APPOINTMENT

Assistant Professor, School of Economics, Shanghai University of Finance and Economics, 2020-

RESEARCH AND RELEVANT WORK EXPERIENCE

Visiting Scholar, Center on China's Economy and Institutions, Stanford University, July 2023
Visiting Scholar, Department of Economics, University of Maryland at College Park, June 2023
Student Research Affiliate, Maryland Population Research Center, 2016-2020

EDUCATION

Ph.D. Economics, University of Maryland at College Park, 2020
M.A. Economics, Tsinghua University, 2015
B.A. Economics (Honors Program), Tsinghua University, 2012

FIELDS OF SPECIALIZATION

Labor Economics, Development Economics, Applied Microeconometrics

WORKING PAPERS

"Curriculum, Political Participation, and Career Choice," with Hongbin Li (Stanford) and Yang Wang (SJTU). Revise & Resubmit at ***Review of Economics and Statistics***.

"Toward an Understanding of the Returns to Cognitive Skills Across Cohorts," with Judith Hellerstein (Maryland and NBER) and Sergio Urzua (Maryland and NBER). Revise & Resubmit at ***Journal of Political Economy: Microeconomics***. Available as NBER Working Paper No.32229.

"Money or Monitoring: Evidence on Improving Worker Effort," with Jing Cai (Maryland and NBER) and Shing-Yi Wang (Wharton and NBER). RCT completed. AEA RCT Registry (RCT ID AEARCTR-0000773). *Draft coming soon*.

"More than a Degree: China's College Expansion and the Marriage Market," with Ming Fang (JNU). *Under review*.

"Racial Gaps in the Early Careers of Two Cohorts of American Men"

SELECTED WORK IN PROGRESS

"Ethnic Neighborhoods and Assimilation: Evidence from Full Count Historical Census Data," with Nathan Schiff (SUFE) and Tianran Dai (SUFE). *Draft available upon request*.

"Role Models and Gender Gaps in STEM," with Hongbin Li (Stanford)

"The Enrollment Impact of Foreign Students on American Public Universities." *Draft available upon request*.

CONFERENCE AND SEMINAR PRESENTATIONS

(*coauthor presented)

2025: NYU-Shanghai, SJTU-Antai, SOLE Annual Meeting (scheduled), BREAD Conference on Development (scheduled), Columbia University*

2024: CUHK-Shenzhen, Peking University-HSBC, The World Bank*, Tulane University*, SEHO Anunal Meeting (SMU)*, Summer Meeting in Urban Economics (PKU)*, Xiamen University*

2023: ES North America Summer Meeting (UCLA), Duke University-Kunshan, Jinan University-IESR, ES Asian Meeting (NTU), SOLE Annual Meeting*, SED Annual Meeting*, ES China Meeting*, WEAI*

2022: APPAM Annual Conference, George Mason University*, SEA Annual Meeting*

2021: NYU-Shanghai, SJTU-Antai, ASSA Annual Meeting, MEA-SOLE, PAA Annual Meeting, AEFP Annual Meeting, ES Asian Meeting, ES China Meeting, UIUC*

2020: Towson University, PAA Annual Meeting, ES DSE Winter School, APPAM Regional Student Conference, CHLR Human Capital Conference

2018-2019: U.S. Bureau of Labor Statistics, University of Chicago-Harris*, George Washington University*, University of Cambridge*, SUNY-Buffalo*

GRANTS AND AWARDS

Faculty Initiation Research Grant, Shanghai University of Finance and Economics, 2020-

University Teaching Grant, Shanghai University of Finance and Economics, 2021-2024

Vernon E. Jordan, Jr. Dissertation Fellowship, the Economic Club of Washington D.C., 2018-2019

Suzanne M. Bianchi Research Grant, Maryland Population Research Center, 2019

Dean's Research Initiative Research Grant, University of Maryland, 2015-2016

Dean's Research Initiative Travel Award, University of Maryland, 2019-2020

Jacob K. Goldhaber Travel Grant, University of Maryland, 2019

REFEREE SERVICES

Journal of Labor Economics, Journal of Human Resources, Journal of Comparative Economics, Economic Development and Cultural Change, Journal of Economic Behavior and Organization, China Economic Quarterly

TEACHING EXPERIENCE

Instructor, Big Data in Economics, Shanghai University of Finance and Economics, 2021-

Instructor, Applied Microeconometrics, Shanghai University of Finance and Economics, 2021-

Instructor, Applied Econometrics, University of Maryland, 2018

Teaching Assistant, Applied Microeconometrics (graduate), University of Maryland, 2017-2018

Teaching Assistant, Labor Economics, University of Maryland, 2018

Teaching Assistant, Intermediate Microeconomic Analysis, University of Maryland, 2015-2016

Research Supervisor, Summer Research Initiative, University of Maryland, 2016-2017

Money or Monitoring: Evidence on Improving Worker Effort*

Jing Cai† Sai Luo‡ Shing-Yi Wang§

April 18, 2025

Preliminary draft. Please do not cite or distribute without permission.

Abstract

Efficiency wages and monitoring are two potential solutions to the moral hazard problem between workers and firms. In a field experiment conducted with new hires at an automobile manufacturing firm in China, we randomly varied both total compensation and monitoring intensity. Both interventions increased worker output but through different mechanisms: higher pay led to longer hours without better performance, while monitoring improved job performance based on managerial evaluations. Additionally, bonuses reduced quit rates, whereas monitoring raised them. These results suggest that firms should carefully consider their primary objectives and weigh these trade-offs when designing optimal labor contracts.

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†University of Maryland. E-mail: cai516@umd.edu.

‡Shanghai University of Finance and Economics. E-mail: luosaiecon@gmail.com.

§University of Pennsylvania. E-mail: was@wharton.upenn.edu.

1 Introduction

Firms in low- and middle-income countries experience significantly higher worker turnover rates than those in high-income countries. Donovan et al. (2023) find that job-to-job transition rates are five times higher in poorer countries, requiring firms to hire and replace workers far more frequently. When hiring new workers, firms face two key challenges: selecting the best candidates and ensuring effort post-hire. One potential solution is to offer wages above the market-clearing level, which can potentially address both issues. Higher wages can attract more qualified and motivated workers, reducing adverse selection (Dal Bó et al., 2013). Moreover, efficiency wage theory suggests that above-market pay can also mitigate moral hazard by incentivizing workers to exert greater effort (Shapiro and Stiglitz, 1984; Yellen, 1984).

Economic theory emphasizes that efficiency wages are particularly valuable when worker monitoring is difficult or costly. This suggests that wages and monitoring are two primary tools for inducing worker effort. However, empirical evidence on the trade-offs firms face when relying on monitoring versus higher wages remains limited. This paper seeks to fill that gap by providing experimental evidence on how compensation structures and workplace monitoring interact to shape worker behavior and firm outcomes.

To investigate this question, we conducted a field experiment with newly hired automobile manufacturing workers in China, randomly assigning them to different compensation packages and levels of workplace monitoring. In the compensation experiment, the control group received the firm's standard compensation package, while the treatment group was offered an enhanced package that included a one-time signing bonus—raising total compensation by approximately 16% over a three-month period. This use of a one-time bonus allows us to test the gift-exchange hypothesis as a mechanism behind efficiency wages (Akerlof, 1982, 1984).

Signing bonuses are a common component of worker compensation. In the United States, over 80% of technical managers and executives receive signing bonuses, along with 50% of sales representatives and nearly 20% of hourly wage workers (Van Wesep, 2010).[1] Firms use signing bonuses to attract job applicants and increase offer acceptance rates. Economic theory suggests that signing bonuses serve as credible signals of a firm's confidence in the quality of a worker-firm match and create incentives for higher effort (Van Wesep, 2010). Empirical research has found

[1]While equivalent statistics for China are unavailable, media reports suggest that signing bonuses have been widely used during periods of high labor demand for manufacturing workers (Liu and Zhu, 2019; Luo, 2021).

a positive relationship between signing bonuses and CEO performance and retention (Xu and Yang, 2016). Additionally, signing bonuses may offer unique benefits for low-income workers, for whom the timing of pay can by particularly important. Receiving compensation upfront may enhance work effort by alleviating financial constraints (Kaur et al., 2025).

Our field experiments on compensation were implemented in a two-stage process, covering 328 job applicants. In the first stage — conducted after interviews but before employment began—the control group was offered the firm's standard compensation package, while the treatment group received an augmented package that included a signing bonus. This bonus was payable one month after the start of employment, conditional on the worker remaining with the firm. To separate selection effects from moral hazard, we further randomized the control group from the first stage into two subgroups. In the treated subgroup, workers received an unexpected bonus of the same amount, paid at the same time, one month after starting work. This two-stage design allows us to distinguish selection effects — arising from differences in worker characteristics — from moral hazard effects. The surprise bonus identifies the effect of incentives on post-hire effort (moral hazard), while the signing bonus captures both selection and moral hazard effects.

In addition to the compensation experiments, we collaborated with the firm to implement a separate monitoring experiment. In this intervention, a subset of workers was randomly assigned to increased monitoring in the form of additional visits by an independent monitoring team. Randomization occurred at the production-line station level, covering 79 stations with an average of 2.7 workers per station.

We use both administrative data from the firm, as well as survey data collected at endline. First, we find that offering a higher compensation package at the interview stage did not lead to improved worker selection. This result aligns with previous studies suggesting that higher wages do not necessarily attract higher-quality applicants (Guiteras and Jack, 2018; Goldberg, 2016). However, it contrasts with studies such as Dal Bó et al. (2013), which find positive selection effects associated with higher compensation for government jobs. A key distinction is that in Dal Bó et al. (2013), higher compensation was randomized and advertised at the recruitment stage, whereas our design captures selection effects post-recruitment, specifically in job acceptance rates.

While observable characteristics of hired workers remained largely unchanged, signing bonuses

increased job offer acceptance rates by 10.6 percentage points. Additionally, higher compensation—whether through the signing or surprise bonus—led to an increase in hours worked and, consequently, monthly earnings. Higher wages also significantly reduced worker quit rates, consistent with efficiency wage theory and the idea that increased pay fosters a gift-exchange response (Akerlof, 1982).

In contrast, the monitoring intervention produced different effects. Increased oversight led to better performance among new hires, as reflected in higher evaluation scores from team managers and increased performance bonuses awarded by other plant managers. However, workers exposed to greater monitoring did not increase their hours and were more likely to quit, suggesting that they found the additional scrutiny undesirable.

We consider whether spillover effects bias our estimates. For the compensation interventions, one concern is that control group workers may have reacted negatively upon learning that some colleagues received bonuses. Using social network data, we find no evidence that control workers with a higher number of treated colleagues reported lower perceptions of fairness or job satisfaction, nor did they differ in performance or hours worked. For the monitoring experiment, two types of spillovers are possible. First, control workers located near treated stations might have felt indirectly monitored. However, we find no evidence that workers in control stations with a higher share of treated neighbors differed in their outcomes. Second, workers in the treatment group may have inferred from the increased monitoring that the firm considered them underperforming. To test this, we examine whether having more treated neighbors affected outcomes among treated workers and find no significant effects. Overall, we find no evidence that spillovers meaningfully influenced outcomes in either the bonus or monitoring experiments.

We also consider an alternative explanation in which hiring managers assigned new hires in the signing bonus treatment and control group workers to different tasks or teams. If so, differences in worker outcomes could stem from task variation rather than compensation differences alone. However, we find no significant differences in task or team allocation between treatment groups.

Our results offer new insights into the roles of efficiency wages — as a form of gift exchange — and monitoring in shaping worker effort. While both higher compensation and increased monitoring enhance effort, they do so by influencing different aspects of worker behavior and are neither substitutes nor complements. Additionally, our findings underscore the importance

of considering worker retention alongside various measures of on-the-job effort, as compensation and monitoring have substantial—but distinct—effects on retention.

Since a significant portion of the costs and benefits stem from the interventions' impact on worker retention, we also examine whether these policies alter the composition of workers who remain at the firm. Higher compensation could either encourage higher- or lower-quality workers to stay longer, while increased monitoring might expedite the departure of poor performers. Interestingly, our findings reveal minimal selection effects driven by worker quits.

We conduct a cost-benefit analysis of all three interventions. The results show that both the signing bonus and the surprise bonus generate benefits that exceed their costs. Notably, their cost-effectiveness is fairly similar, with the pre-hire signing bonus yielding slightly higher returns. This may be due to the fact that, while the surprise bonus leads to larger improvements in retention and hours worked, the signing bonus is less costly and also boosts job offer acceptance and reduces early quits in the first month. In contrast, the costs of increased monitoring outweigh its benefits, primarily because of the high expense associated with recruiting and training replacements for workers who quit in response to greater oversight.

Our research contributes to the existing literature on randomized experiments examining the role of financial incentives in worker productivity. Specifically, we add to three key strands of this literature.

First, we contribute to studies on the selection effects of higher compensation, where findings on the direction of selection remain mixed (Deserranno, 2019; Guiteras and Jack, 2018; Goldberg, 2016). Two prior studies use a design similar to ours, employing a two-stage field experiment to separate selection and effort margins. Kim et al. (2020) analyze government workers in Malawi but compares career versus wage incentives without a control group that experiences neither incentive. Leaver et al. (2021) study teachers in Rwanda, comparing pay-for-performance versus fixed wages but again do not focus on the impact of increased compensation. While prior studies on selection effects of compensation largely focus on government, NGO, or casual day labor jobs, we examine hiring in private-sector firms for longer term positions. This distinction is important, as workers in government or NGO roles may prioritize non-wage benefits, whereas such factors may play a lesser role in private-sector jobs. We are also unique in being able to consider selection by quits in addition to selection driven by job taking.

Second, we provide new insights into how increasing financial incentives affect productivity

among existing workers while holding selection constant. Much of this existing literature focuses on the impacts of introducing performance pay in different ways (e.g. Fehr and Goette, 2007; Friebel et al., 2017; Gosnell et al., 2020; Lazear, 2000; Leaver et al., 2021; Shearer, 2004; Lazear, 2018; Ku, 2019; Brown et al., 2024) or compares non-monetary and monetary incentives (summarized by Cassar and Meier, 2018).[2] With our unique design, where randomization in compensation is not linked to performance or hours, we can look at the impacts of increases in compensation in the absence of direct incentives on working hours or performance.

Third, we contribute to the literature on field experiments examining gift exchange in the workplace. Like our study, the papers in this agenda explore pay structures that are not tied to performance outcomes. de Ree et al. (2018) investigates the effects of an unconditional salary doubling for teachers in Indonesia, finding improvements in teacher satisfaction but no impact on student learning. For data entry workers hired for one-time tasks, DellaVigna et al. (2022) and Gneezy and List (2006) examine the effects of unexpected financial gifts from employers. Similar to our findings from the bonus treatments, DellaVigna et al. (2022) finds that these financial gifts lead to increased effort but no productivity gains. Our study expands this literature by focusing on permanent, private-sector employment, allowing us to examine long-term worker retention in addition to effort.

Beyond financial incentives, our paper contributes to a growing literature on how exogenous changes in monitoring affect worker outcomes (Adhvaryu et al., 2022; Bandiera et al., 2009; Nagin et al., 2002; Dal Bó et al., 2021; de Rochambeau, 2021; Kelley et al., 2024; Friebel et al., 2024; Houeix, 2025; Sen, 2024). The evidence here is also mixed, with both positive and negative effects resulting from additional monitoring. This is perhaps not surprising given that the returns to additional monitoring should depend on the amount of monitoring that they are already doing. While most studies focus solely on monitoring, they often overlook the interplay between monitoring and compensation in addressing moral hazard. One exception is Jackson and Schneider (2015), who study a field experiment requiring auto repair workers to use checklists. They compare the impacts of monitoring with quasi-experimental estimates of the impacts of performance-based pay. However, their study cannot isolate monitoring effects from the potential memory-aiding function of checklists. In contrast, our experiment exclusively increases monitoring visits without introducing additional forms or checklists for workers to fill

[2]Interestingly, while Friebel et al. (2017) discuss monitoring as the primary alternative to these financial incentives to induce worker effort, they do not test this directly.

out. On the compensation side, our study features randomized variation in wages, where the increase is a flat amount rather than performance-based as in Jackson and Schneider (2015). This aligns more closely with the classical efficiency wage hypothesis, as commission-based pay inherently mitigates moral hazard concerns.

We provide additional background on the study context in the next section. Section 3 outlines the field experiments we conducted, followed by a description of the data sets in Section 4. Next, we detail our estimation strategy and present the results in Sections 5 and 6. In Section 7, we discuss the key findings and their implications. Finally, we conclude with a summary of insights and potential directions for future research.

2 Background

We conducted an experiment with an automobile manufacturing company in China that specializes in producing electric cars. The production of electric vehicles (EVs) in China has been growing rapidly in recent years. From 2016 to 2023, global sales of electric cars surged from 0.77 million to 13.4 million units, with China accounting for 58% of these sales in 2023. Notably, in July 2024, electric car sales in China surpassed those of traditional vehicles for the first time.

The company operates three production plants: a welding plant, a painting plant, and an assembly plant. Within each plant, production line workers are organized into teams, and further divided into stations. Workers in the same station perform similar tasks and are located close to each other. The average size of a station is 2.7 workers. There are 32 teams and 298 stations in the three plants. In addition to the three production plants, the company has two departments that are not on the production line: a logistics department and a quality department. Workers in these two departments are organized into 15 teams but are not further organized into stations. In early November of 2023, there were about 1,000 total workers across the three production plants and the two departments.

Hiring and retention are top priorities for the company. In general, manufacturing companies in China, including our partner, face significant challenges with high employee turnover. At our partner company during the period of analysis, the turnover rate for newly hired workers within the first 10 days reached approximately 25%. This level of turnover is costly, as it requires substantial resources to recruit and train replacements.

The company works with local staffing agencies to refer potential job applicants. The staffing

agencies provide information about the jobs that the company is hiring for and provides information on the approximate pay range of the positions.[3] With the list of applicants provided by the staffing agencies, the car manufacturing company conducts its own interviews to screen and select candidates. The interview process is concentrated in one day during which a batch of candidates come into the factory to be assessed by a hiring team from the human resources department. They do interviews and skills tests. During the day, candidates are informed about the compensation package, work schedule, and work environment. At the end of each interview day, the hiring managers contact successful applicants via phone to extend job offers. Those who accept the offer typically report to the company and complete the necessary paperwork the following day. In the year prior to our analysis, the job offer acceptance rate was around 65%.

New workers receive 10 days of initial training. The human resources department first offers training on general company policy for three days. The new workers are then sent to one of the plants or departments, where they receive skill training that is specific to their tasks. For the next three days, the new workers are trained off the production line. Then in the last four days, each worker is assigned to a specific team and station along the production line, and they get trained by working alongside an experienced worker. Workers are paid during the training process.

After a worker accepts the job offer and begins at the company, the company uses several methods to monitor their performance. First, workers are organized into teams, each of which is directly supervised by a team manager. On a monthly basis, team managers from all three plants complete an evaluation form assessing each worker's performance across multiple dimensions.

Additionally, other plant staff conduct regular monitoring visits to the teams and stations. These visits are carried out by the plant head, plant manager, engineers, and staff from the monitoring office. On average, each station receives approximately 11 visits per week from these external monitors, with each visit lasting around 5 minutes. During these visits, plant staff closely observe workers as they perform their tasks. If any issues arise during the monitoring visits, the staff either reports them to the team manager or addresses them directly with the workers. For more serious issues, such as non-compliance with critical safety or quality control procedures, the plant staff records the incident and submits a report to plant management. Each

[3]For example, the advertisements put forth by the staffing agencies for our sample stated a pay range where the maximum of the range was 36% higher than the minimum of the range, depending on the number of hours worked and performance. More detailed information about the compensation and work arrangements are not provided to the candidates on their interview day.

plant operates a performance bonus system linked to these monitoring records.[4] Workers who perform well are rewarded with a bonus, while those with poor performance are penalized with a deduction.

For new hires in the first several months on the job, workers are paid an hourly wage.[5] Variation in their monthly paycheck is predominantly determined by hours. There are other components of their monthly paycheck, including fringe benefits and deductions as well as performance bonuses, that represent a very small fraction of a worker's compensation.[6]

3 Experiment

We conducted two types of experiments in collaboration with our partner firm: a two-stage financial incentive experiment and a monitoring experiment. The experimental design is outlined in Figure 1 and the timeline is shown in Figure 2.

3.1 Financial Incentives Experiments

In our first experiment, we offered additional compensation, in the form of bonus payments, to a randomly chosen set of new workers either at the hiring stage (prior to their accepting the offer) or one month after they joined the company. One key advantage with using a signing bonus is that these are commonly known to be one-time payments limited to the start of the job and should not alter employee's expectations about future bonuses. For the surprise bonus, we specified clearly that this was a one-time bonus only for the start of the job. The experiment involved three hiring batches that occurred in September and October 2023, targeting job applicants for positions across the three plants and two departments.[7]

A total of 328 job applicants successfully passed the interview process across all hiring batches. The first-stage financial treatment was introduced at the time that the firm made the job offer to candidates. As shown in Figure 1 Panel a, candidates who passed the interview

[4]The performance data includes information from the company's quality inspectors, who are responsible for identifying product defects along the production line, particularly in high-risk safety areas. Any defects detected are traced back to a specific worker or team.

[5]New workers start out a temporary contract for the first few months in which they are paid an hourly wage. Workers that remain at the firm after the first few months are given a permanent contract and their pay structure changes to a monthly base salary plus monthly performance based bonuses.

[6]On average, performance bonuses can be deductions (i.e. negative in value) and are less than 1% of their total monthly compensation.

[7]The first batch occurred from September 14 to 16, the second from October 11 to 17, and the third from October 24 to 28.

were randomly assigned to one of two groups: the signing bonus treatment group (ST) and the signing bonus control group (SC).[8] We assigned approximately one-third of the candidates to the ST group, and two-thirds to the SC group. During the phone calls informing candidates that they were receiving the job offer, hiring managers presented bonus control group with the details of the standard compensation package. In contrast, candidates in bonus treatment group were told that, in addition to the standard pay, they would receive a one-time signing bonus of 1,600 RMB. They were told that this bonus would be disbursed via mobile transfer at the end of their first month with the company, and that the signing bonus would need to be returned if they quit the company before the end of December 2023. For context, the monthly pay for a new worker at the partner company was around 3400 RMB. Thus, the signing bonus represented about 47% of a worker's standard monthly compensation. The signing bonus was not advertised to the applicants, and the treatment group was only informed about this additional financial incentive at the time the job offer was made after the interview day.

Each applicant came to the factory to sign their employment contract, typically the following business day, privately in an office with no other new hires in the same room. Individuals in both the treatment and control groups (ST and SC) signed a non-disclosure agreement prohibiting them from discussing the company's compensation scheme with their co-workers.[9] In addition, individuals in the treatment group (ST) also signed an acknowledgment letter that outlined the details of the signing bonus. We also explicitly told the firm that the HR department should not inform anyone, including team managers, about who did and did not receive the signing bonus. The transfer was made as a one-time transfer that was separate from their monthly paychecks for workers still at the firm one month later.

The second stage of the financial incentive experiment took place one month after the workers joined the company.[10] Among those who were not treated in the first stage (SC) and who remained with the company after one month, we randomly assigned those workers approximately equally to two sub-groups: the surprise bonus treatment group (SCT) and the surprise bonus control group (SCC). Workers in group SCT received a surprise one-time bonus of 1,600 RMB, while those in group SCC did not. As in the first stage, workers in group SCT were informed

[8]This was stratified by plant or department.

[9]Prior to the experiment, the company explicitly instructed workers not to discuss wage-related topics with their coworkers. However, workers had not previously been required to sign a formal non-disclosure.

[10]Due to the staggered start dates of the three hiring batches, this stage occurred at different times for each batch, ranging from October 17 for the first hiring batch to November 27 for the third batch. Like the first stage, this randomization was stratified by plant or department.

about the bonus via a phone call from the human resources department.[11] They received the payment at the same time as workers in the first-stage treatment group (ST). These workers were also told that they would need to repay the bonus if they left the company before the end of December 2023. Each worker signed an acknowledgment letter that was identical to the one signed one month earlier by the signing bonus treatment group. This also occurred in a private office with no other workers in the room. The surprise bonus control group (SCC), on the other hand, received no notification and did not sign the acknowledgment letter.

3.2 Monitoring Experiment

In a separate experiment, we increased the monitoring intensity for workers in randomly chosen stations. This experiment began after the disbursal of the bonuses for the last batch of hires. The monitoring experiment was conducted across the three production plants of our partner company.[12] Figure 1 Panel b illustrates the monitoring experiment.

Unlike the first experiment, which randomized participants at the individual worker level, the second experiment was randomized at the station level.[13] We focused on the 79 stations that had at least one new hire from the financial incentives experiments to examine the interaction effects between the two types of experiments. This sample was then randomized into two groups: monitoring treatment and control stations.[14] All workers, including new hires and older workers, in the monitoring treatment stations were subjected to the monitoring intervention. Given the randomization ratio allowed by the firm with 40% treatment stations and 60% control stations, we have 116 workers in the monitoring treatment stations and 200 workers in the monitoring control stations.

In the treatment stations, for each week for six consecutive weeks, one external monitoring staff member at each plant conducted an *additional* visit on top of their regular visits. During these additional visits, the staff completed a monitoring form on-site in addition to their usual tasks.[15] This form required them to record and evaluate the performance of each worker present.

[11] Thus, they were informed at home and not while at the factory. Again, we told the HR department that no information about this bonus should be relayed to anyone else at the firm, including mangers on the production line.

[12] The monitoring experiment was not implemented in the logistics and quality departments, as workers in these two departments were not on the standard production line or organized into stations.

[13] Given that workers are spatially clustered with other members of their work station, we determined that the monitoring experiment could not be implemented at the individual level.

[14] This randomization was stratified by plant and station size.

[15] In their standard monitoring visits, they did not fill out a specific form for each worker at the station for each

The act of filling out the form in the workers' presence may have heightened the perceived importance of this monitoring visit relative to the usual visits.

The monitoring form included the time of the visit and ratings of all workers in the station based on several dimensions: production, safety, quality, attitude, and overall performance. Completing the new requirement added approximately 8 minutes per additional visit at each treatment station. For comparison, a regular monitoring visit typically took about 4–5 minutes per station. In contrast, control stations only received their regular monitoring visits, with no corresponding evaluation forms completed during them.

To ensure the integrity of the experiment, external monitoring staff were explicitly instructed to maintain their usual level of monitoring at the control stations. To compensate staff for the additional time spent at treatment stations, they were paid for the extra effort at their standard overtime rate.

4 Data

Our analysis combines data from three main sources: administrative data from the company, covering both workers from August 2023 to January 2024, station level data filled out by managers and survey data collected at baseline between September and October 2023, with follow-up surveys conducted from December 2023 to February 2024.

4.1 Administrative Data on Workers

There are four main worker-level datasets that we receive from the firm. First, we have monthly metrics on worker performance from the three production plants.[16] This includes two main components. The first is a worker evaluation score that ranges from 0 to 100, assigned by the team manager. This score assesses a worker's overall performance for the month and is calculated as the sum of scores across five categories: production, quality, safety, equipment, and composite, each weighted equally.[17]

visit, but only noted any problems or particularly positive behavior.

[16]The logistics and quality departments do not collect worker performance data.

[17]The production category evaluates meeting production targets as well as the accuracy and efficiency of task completion. The quality category considers the number of products failing quality tests and issues identified through monitoring. Safety accounts for accidents and violations of safety regulations. The equipment category assesses proper maintenance and usage of equipment. The composite category includes any residual factors not covered by the other categories.

The second component of the worker performance metrics is the performance bonus system based on information gathered through regular monitoring by plant staff, as well as worker performance records collected by other personnel, such as quality inspectors. This is also implemented across the three plants. In each month, a worker can have multiple records of good or poor performance, measured as bonus payments or fines. Good performance typically includes actions like identifying defects in car components and resolving unexpected issues at work with bonuses ranging from 10 to 710 RMB. Poor performance, on the other hand, generally involves serious or repeated mistakes and violations of safety protocols with fines ranging from 5 to 260 RMB. For each month, a worker can have multiple records of bonuses or fines for their work performance, or no records at all. We aggregate the data to the worker-month level.

Together, these two components offer different yet complementary metrics of a worker's performance. The evaluation score is collected by team managers, whereas the performance records are compiled by other plant staff. While the evaluation score does not directly impact a worker's monthly pay, the performance bonuses (and fines) directly affect their pay.

The second administrative dataset includes monthly earnings and hours worked. Workers are paid on a monthly basis, and their total earnings are primarily driven by hours worked. In our sample, hours worked account for approximately 88% of the variation in monthly earnings across workers. Other components of monthly earnings include meal allowances, fringe benefits, and deductions for personal leave and absences. Notably, the monthly earnings recorded in this administrative data set by the human resources department do not include the performance bonuses and fines, which are recorded separately a plant-level department.

Third, we have detailed records of the exact dates when workers joined and, if applicable, left the company. Finally, we have administrative data containing worker background information, including education, skill level, gender, and hometown.

We use application form data as baseline data to check for balance between our treatment and control groups.[18] When job applicants came on the interview day, prior to the signing bonus intervention, they filled out a standard application form that collected some basic background information such as age, gender, education, relevant prior skills in the automobile industry, and their hometown. As shown in Table A1, 97% of the workers are male, over 90% have at least secondary school education and the average age is 26 years old. Few have skills in the

[18]The company did not retain the application forms filled out by job applicants who did not pass the interview stage, but we have this data for all job appilcants who passed the screening process.

automobile industry prior to applying for this job (5%). In addition to demographic background characteristics, the application form included questions on the candidates interest in converting to a permanent position in the future, should the opportunity arise, on a scale of 1 to 3. The interviewers also gave an overall assessment of the candidate on a scale of 1 to 4.

4.2 Station-Level Data

In addition to worker-level administrative data, we obtained a station-level data set containing weekly records of the monitoring received by each station, covering three weeks before and six weeks after the monitoring intervention began. We asked team managers to complete a paper form each week, documenting the number of monitoring visits and the average duration of each visit for every station in their team. This data is further categorized by the type of individuals conducting the monitoring visits, which included the monitoring staff implementing our intervention and other company personnel, such as plant managers and engineers. This dataset allows us to assess whether the monitoring treatment and control stations exhibited parallel trends in monitoring intensity prior to the intervention. We also evaluate whether the monitoring staff increased the intensity of monitoring for treated stations following the intervention.

4.3 Follow-Up Survey Data on Workers

We conducted a short follow-up survey to gather additional information that would complement the administrative worker data. The follow-up survey comprised two parts: a paper survey and an online survey. At the end of December 2023, we distributed paper surveys to all workers currently employed at the company. These were completed by January.

Some workers had already quit the firm before we implemented the follow-up survey at the firm. For these workers who participated in our financial incentives experiment and joined the firm but quit prior to the timing of the follow-up survey at the firm, we contacted them via WeChat messaging to complete a follow-up survey.[19] We successfully surveyed 65% of the workers targeted through WeChat.

The follow-up survey needed to be brief to minimize the time that it took out of the workday. The first section included a standard set of questions about work satisfaction, using a five-point

[19]WeChat is the most popular messaging app in China, with 80% of the adult population who are active users as of 2023. Workers could easily access and complete the survey within the WeChat app on their cell phones.

scale ranging from totally agree to totally disagree. The statements included being satisfied with the number of hours, the pay and the job at the firm. They also were asked to concur with statements on whether the company treats workers fairly, whether they get along with their co-workers and managers, and whether the pay scheme is fair. We also posed a general well-being question, asking workers to rate their life by envisioning a ladder with steps numbered from 0 at the bottom to 10 at the top.

The second section asked whether workers discussed their salary or bonuses with others at work. Despite all participants in our first (hiring) experiment signing a non-disclosure agreement prohibiting the sharing of compensation-related information, and the company's general discouragement of such discussions, we wanted to assess the potential concern that treatment group members might reveal the existence of the signing or surprise bonuses to the control group.[20] This data helps us investigate whether this concern is warranted.

The final section asked workers about their friendship networks at work.[21] Specifically, we asked that workers list their three best friends within their team, as well as their three best friends in the company outside of their team. We use this information to examine whether the effects of our experiment spilled over to the friends of treated workers.

5 Estimation Strategy

For the financial bonuses experiment, our primary approach is to estimate the following equation for individual i:

$$y_i = \alpha BonusTreat_i + \beta SurpriseTreat_i + X_i'\gamma + \epsilon_i \tag{1}$$

where the key regressors of interest are $BonusTreat_i$, which is an indicator for whether worker i was randomly chosen to receive information about the signing bonus prior to joining the firm, and $SurpriseTreat_i$, which is an indicator for whether the worker was randomly chosen to receive a surprise bonus one month after joining the firm. We also include a vector of control variables, X_i. In the parsimonious specification, this includes the hiring batch fixed effects and

[20] The company takes data privacy seriously and prohibits staff, including plant managers, team managers, and HR personnel, from disclosing any worker's compensation information to others.

[21] This is the only section that we excluded from the cell phone version for brevity. It was relatively time consuming to write down names in Chinese.

team fixed effects. The randomization was done separately for each hiring batch. We include team fixed effects to account for team-level unobserved differences, including the behavior of the manager. For the main outcomes based on administrative data, we have several months of post-intervention data. Therefore, we include month fixed effects in the estimation to account for any firm-wide aggregate events occurring in a given month. For outcomes from the follow-up survey, we have only one observation per individual and do not include month fixed effects. In a second set of regressions with additional controls, we also include their treatment status in the monitoring experiment.[22] For administrative outcomes, the standard errors are clustered at the individual level.

Appendix Table A1 shows summary statistics from baseline application data collected by the firm combined with administrative data on time-invariant characteristics of the workers. Along all the variables, the two treatments groups are not statistically different from the two control groups. This provides evidence that the treatment and control groups are similar to each other prior to the intervention.

For outcomes y_i capturing worker effort, such as performance and hours worked, efficiency wage theory based on gift exchange predicts that α and β should be positive. Because the surprise treatment occurs *after* workers have already joined the firm, the coefficient estimate of β identifies the moral hazard effects of increasing compensation. In contrast, the signing bonus treatment is offered prior to when job applicants accept the job offer, so α identifies both the moral hazard effect and any additional selection effects of the signing bonus. There are two types of selection effects that may drive differences in α and β: selection of who joins the firm and selection in who quits within the first few weeks of joining.[23]

The monitoring experiment was randomized at the station level within a sample that included the three production plants but excluded the two departments. For administrative data outcomes, we have data from both before and after the start of the monitoring experiment.[24] Therefore, we estimate a difference-in-differences equation of the following form:

$$y_{ist} = \delta MonitorTreat_{is} \times MonitorPost_t + \eta_{is} + \kappa_t + \epsilon_{ist} \quad (2)$$

[22]The total samples for the monitoring experiments and the bonus experiments are not fully overlapping. The bonus experiments included the non-production line departments but the monitoring experiments did not.

[23]As discussed later, Table 1 will test for differential selection in who is hired based on getting the bonus offer versus not and Table 8 will test for differential selection in the types of people who quit.

[24]We do not have pre-intervention administrative data for the signing bonus intervention, so the bonus estimates cannot be derived using the same strategy.

where $MonitorTreat_{is}$ is an indicator for the station s being randomly selected for additional monitoring. $MonitorPost_t$ is an indicator that equals one for the periods after the start of the monitoring intervention. We also include individual fixed effects, to remove any time-invariant individual characteristics, and month fixed effects.[25] The standard errors are clustered at the station level.

We can also run estimates that exploit the periods before and after the start of the monitoring experiment:

$$y_{ist} = \sum_{j=-2}^{2} (\delta_j MonitorTreat_{is} \times MonitorPost_j) + \eta_{is} + \kappa_t + \epsilon_{ist} \tag{3}$$

where $MonitorPost_j$ is an indicator for each period j around the start of the monitoring experiment.

For outcomes where we only have observations after the intervention, such as the ones from the follow-up survey, we estimate the following equation for individual i working in station s:

$$y_{is} = \delta MonitorTreat_{is} + X'_{is}\gamma + \epsilon_{is} \tag{4}$$

where the key regressor of interest is $MonitorTreat_{is}$, which is an indicator for whether the worker i was working at a station s that was randomly selected to receive additional monitoring at the time we implemented the randomization.[26] The parsimonious specification includes hiring batch fixed effects and team fixed effects. In a second set of regressions with additional controls, we also control for the financial treatment indicators.

Appendix Table A2 shows the summary statistics from the administrative data on time-invariant characteristics of the workers in the stations that were in the monitoring randomization sample. The monitoring treatment and control groups are not statistically different from each other along any of the variables available.

[25] The individual fixed effects subsume the additional control variables for the financial experiments.

[26] Note this is an intention to treat variable where if a worker switches stations *after* the start of the monitoring intervention, they are still coded as the treatment status of the station that they were at when the intervention began.

6 Main Results

6.1 Impact of Bonuses on Job Offer Acceptance

We first examine whether giving bonuses affects the selection of qualified candidates who join the firm. Specifically, we assess whether randomly receiving a signing bonus during the interview process influences job acceptance among individuals the firm deemed qualified and extended job offers to. As shown in Appendix Table A3, there are large, positive effects of increasing compensation through bonuses on job acceptance. In the parsimonious specification in column 1, the signing bonus led to a 10.6 percentage point increase in the probability of accepting the job offer, compared to a 68% acceptance rate in the control group, and this estimate is significant at the 5% level. This is consistent with a standard labor supply curve, where higher total compensation attracts more job takers.

Next, we examine whether higher compensation induces positive selection by comparing the observable baseline characteristics of job takers in the treatment and control groups. Table 1 shows the baseline characteristics of job takers for those who received the higher compensation through the signing bonus and those who received the standard compensation package at the time that they made the decision to join the firm. We find no significant differences between the two groups across any measured characteristics. In particular, there is no evidence that higher compensation improves characteristics typically associated with higher quality workers, such as having completed secondary school, having prior experience in the industry, or receiving a higher interviewer assessment score. Similarly, we see no differences in the variables in the application data for workers in the signing bonus treatment and control groups who received a job offer but did not join the firm (Appendix Table A4).

To some extent, the degree of selection that we can measure here is limited because our setup does not allow for differential selection into who responds to a job advertisement. Instead, we focus on selection at a later stage—specifically, who accepts the job offer among those who interviewed and passed the screening process. Thus, while additional bonuses successfully increase job acceptance rates, they do not significantly alter the characteristics of job takers at this stage of the hiring process.

6.2 First Stage Estimates of Monitoring Treatment Assignment

After conducting the monitoring randomization, we provided the firm with a list indicating which stations the external monitor should visit one additional time per week. To assess the effectiveness of this intervention, we first analyze data from weekly forms filled out by *team managers* at the station level. These forms report the number of monitoring visits conducted by the external monitoring staff as well as visits from other monitoring staff, including the plant head, plant manager, plant engineer, quality staff, and any other monitors excluding the external monitoring production staff. Since each team manager oversees multiple stations, some managers may have reported data for both treatment and control stations.

We begin by examining whether the external monitoring team increased its visits to treatment stations relative to control stations (i.e., the first stage). This is shown in column 1 of Table 2. We see that in the control stations, the monitors visited about 1.83 times per week. Following the implementation of the monitoring intervention, external monitors visited treatment stations an additional 0.76 times per week relative to control stations, a difference that is statistically significant at the 5% level.

Since we have weekly data for both treatment and control stations, we further analyze the timing of monitoring visits by estimating the leads and lags of weekly visits interacted with treatment status, as shown in Figure 3.[27] Before the intervention, the number of monitoring visits was similar between treatment and control stations, with magntiude of the coefficients close to zero and not significantly different from zero. However, immediately after the intervention began, the coefficients show a clear upward shift, indicating an increase in monitoring visits to treatment stations.

Overall, the evidence here provides strong evidence that the monitoring intervention successfully increased external monitoring in treatment stations relative to control stations.

While we asked the plant not to adjust any other forms of monitoring, we were concerned that increased monitoring by the external production monitoring team might lead to a reduction in other forms of monitoring. To examine this, column 2 of Table 2 reports the number of monitoring visits conducted by plant staff other than the external production monitoring team, as recorded by team managers.[28] We see no significant change in the number of monitoring

[27]The regression also includes station and week fixed effects. The standard errors are clustered at the station level.

[28]This includes the plant head, the plant manager, the plant engineer, the quality department and any other

visits by other staff following the start of our experiment. In fact, while the coefficient is not statistically significant, it is positive in magnitude—opposite to our primary concern that increased external monitoring might displace other forms of monitoring. This suggests that additional monitoring by the external production monitoring team did not lead to a reduction in oversight from other plant staff.

During these visits, the external monitor was asked to complete a form evaluating the performance of workers at each station. At the end of the intervention, we collected these paper forms to assess whether the monitor visited and recorded evaluations for both control and treatment stations. This is shown in columns 3 and 4 of Table 2. Reassuringly, as shown in the control mean row, very few (1%) of the control group had a monitoring form filled out.[29] We find that individuals in treatment stations were 29% more likely to have a monitoring form completed by the external monitoring team compared to those in control stations in the parsimonious specification and the result remains consistent after adding additional controls, and both estimates are significant at the 1% level. Several factors may explain why this percentage is not closer to 100%. Some workers may have been absent, quit, or transferred to another station by the time the monitor visited. Additionally, monitors may not have completed forms for every worker, or some paper forms may have been lost before data entry. Overall, there is clear evidence that the monitors visited many of the treatment stations and spent time filling out the additional monitoring forms that they did not usually fill out in their standard monitoring visits.

6.3 Results on Performance, Hours and Earnings

We analyze the impact of the interventions on various measures of worker effort. In Table 3, we examine the impact of bonuses and monitoring on two measures of individual performance from the administrative data, including the monthly evaluation score given by team managers and the outcomes of assessment done by plant staffs. For the monitoring intervention, we estimate equation 2 using monthly administrative data, while for the financial interventions, we include month fixed effects in the estimation equation.

In Panel A of Table 3, we report the impact of financial incentive interventions on worker performance outcomes. In columns 1 and 2, we examine the impact on the monthly evaluation

staff.

[29] We think this is driven by workers in control stations moving to treatment stations after the start of the monitoring experiment.

score. We see the magnitudes of the impacts of receiving the bonuses are very small and not significantly different from zero.[30] Similarly, recipients of financial incentives do not receive significantly different performance-based bonuses, which are determined by assessments done by the plant and quality inspectors (columns 3 and 4).

In contrast, the monitoring interventions shows very different impacts. In Panel B of Table 3, we find that increased monitoring by external staff leads to higher evaluation scores given by team managers.[31] In column 1, the estimated effect is an increase of 3.11 points, relative to a control mean of 81.3 points, and is statistically significant at the 5% level. Additionally, performance-based bonuses and deductions assigned by plant-level staff also increase in response to monitoring. As shown in column 3, monitoring raises this amount by 23.3 RMB, a result that is also significant at the 5% level. However, this corresponds to only a few U.S. dollars, so this is not substantially changing their total compensation.

In Panel B of Appendix Table A5, we look at the separate components of worker performance and find that increased monitoring improved workers' performance in quality and safety specifically (and these estimates are significant at the 10% level). There were no significant changes in production, equipment maintenance or a residual category.

In Table 4, we examine another dimension of worker effort—hours worked. Panel A shows the impact of the two bonuses treatments, which suggests that both treatments increased the number of hours worked by the new hires. Specifically, as shown in columns 1 and 2 of Panel A, workers who were informed about the signing bonus before joining the firm increased the total number of hours that they worked per month by about 16.4 to 20.5 hours. These estimates are significant at the 5% and 1% levels, respectively. Workers who received the surprise bonus one month after joining the firm worked an additional 29.6 to 32.9 hours per month, with both estimates significant at the 1% level. Moreover, the larger effects of the surprise bonus are significantly different from those of the early signing bonus.[32]

In columns 3 and 4, we see a substantial increase in workers' monthly earnings.[33] The signing bonus led to a 16–17% increase in earnings, while the surprise bonus resulted in a

[30] As shown in Panel A of Appendix Table A5, none of the components of the performance score change significantly.

[31] Note that the individual fixed effects in Panel B absorb the additional controls.

[32] This is likely driven by selection of who has already quit in the bonus sample prior to the start of the surprise intervention. We discuss this in detail in Section 6.4.

[33] Note that the monthly earnings recorded in this administrative data set from human resources does not include the performance bonuses in Table 3 but those are less than 1% of their total compensation.

25–26% increase. All estimates are statistically significant at the 5% level or higher. Since these new workers are paid hourly, the increase in earnings aligns with the observed increase in hours worked.

Appendix Figure A1 shows the dynamics in the administrative data outcomes over the months after the start of each intervention. The omitted period is the first period of the intervention, so this shows the dynamics after the intervention. There are no strong dynamic patterns over time.

Turning to the monitoring experiment (Table 4, Panel B), we see that there are no significant effects of additional monitoring on workers' total working hours, or monthly earnings. The sizes of the coefficients are also closer to zero than that of the bonus payments.

We also show coefficient estimates corresponding to all of the leads and lags around the start of the monitoring intervention in Appendix Figure A2. The figure supports the assumption of parallel trends across all five outcomes between the monitoring treatment and control stations prior to the intervention. Consistent with the differences-in-differences estimates shown in the table, we observe a shift in the coefficients for evaluation score and performance bonus after the intervention begins. The positive impact on evaluation score strengthens over time, while the effect on performance bonus peaks in the first two months before declining. Although some of the interactions between post intervention time periods and treatment status are not significant at the 5% level, several are significant at the 10% level.

The results in Tables 3 and 4 indicate that both increasing compensation and monitoring are effective at improving effort on the part of workers, but interestingly, they affect different dimensions of worker effort. Workers respond to higher bonuses by significantly increasing the number of hours they work for the firm. Since the increased compensation is provided as a one-time bonus rather than a higher hourly wage, this response does not align with a standard labor supply reaction to wage increases. Instead, it may reflect a gift-exchange dynamic, where workers perceive the bonus as a gesture from the firm and reciprocate with additional effort. However, while they work more hours, their job performance does not improve. These findings are consistent if we assume diminishing returns to effort—longer hours may lead to fatigue and reduced work quality. In contrast, increased monitoring enhances the quality of new hires' work. It may also improve focus and accelerate error correction, which could explain the lack of effect on the number of hours worked.

6.4 Results on Retention

We also examine how the interventions impact worker retention after they join the firm. Panel A of Table 5 presents the effects of bonus payments, estimated using equation 1, where the outcome is an indicator for whether an individual remains employed at the firm by the end of the calendar year, based on administrative data.[34] Workers who received additional payments are significantly more likely to stay at the firm compared to those who did not. Specifically, individuals who received a signing bonus before accepting the job are 18.7% more likely to remain employed at year-end, a result significant at the 1% level. The effect of the surprise bonus, announced after workers had stayed for one month, is even larger at 55.1% and is also significant at the 1% level.

The difference in these two estimates, given by the p-value at the bottom row of Panel A, indicates that the retention effects of the two bonuses are significantly different at the 1% level. This discrepancy may stem from the timing of the interventions. The signing bonus was offered before workers joined the firm, whereas the surprise bonus was announced a month later. Since the highest quit rate occurs within the first month—often because workers realize that production line work in automobile manufacturing is not a good fit—those who left early were not included in the surprise bonus treatment sample. The selection in who stays at the firm after the first few weeks may explain why the surprise bonus had a stronger effect on retention.

Figure 4 estimates a Cox proportional hazard model as well as Kaplan-Meier estimates of the probability of still working at the firm after the start of the interventions. Panel A shows that for the signing bonus, a retention gap between the treatment and control groups emerges almost immediately and then stabilizes. In contrast, Panel B shows that quit rates remain low for both groups immediately after the surprise bonus, but the gap between treatment and control widens later.

The impact of the monitoring intervention on retention, estimated using equation 4, is presented in Panel B of Table 5. Unlike the positive effects of bonuses, additional monitoring decreases the likelihood that workers remain at the firm by year-end. In the parsimonious specification, workers who were randomly assigned to additional monitoring were 12.5% less likely to stay, a result significant at the 5% level. With additional controls, the effect remains similar at 12.1% and is also significant at the 5% level. Panel C of Figure 4 presents the corresponding

[34] This is also when we conduct the endline surveys.

Kaplan-Meier and hazard estimates, showing that the retention gap between the monitoring treatment and control groups emerges about one week after the intervention begins.

While both compensation and monitoring influence worker effort, their effects on retention diverge sharply. Higher compensation improves retention, while increased monitoring reduces it—suggesting that while workers appreciate financial incentives, they dislike being closely monitored.

6.5 Results on Well-Being Measures

We next examine workers' self-reported work satisfaction and overall well-being based on the follow-up survey. Well-being is measured on a 0–10 ladder scale, with 10 representing the highest level. Work satisfaction is constructed as an index combining all survey questions on work satisfaction, using a GLS weighting procedure that assigns lower weights to highly correlated components.[35]

As shown in Appendix Table A6, the surprise bonus treatment led to a large and significant increase in the index of work satisfaction. This is consistent with the idea that workers were happier as a result of the better compensation and were less likely to quit. While the coefficients on the impact of the signing bonus treatment on work satisfaction are positive, they are not precisely estimated and neither significantly different from zero nor significantly different from the coefficients on the surprise bonus treatment. The impact of additional monitoring on work satisfaction is negative but not significant at the standard levels. None of the interventions have significant effects on well-being. Appendix Figure A3 further examines the effects of the three interventions on individual components of work satisfaction. Across these six outcomes, neither the signing bonus nor increased monitoring has a significant impact. However, the surprise bonus has a positive and significant effect on workers' reports of getting along with their co-workers.

[35]The six work satisfaction questions assess agreement, on a five-point scale, with statements regarding overall job satisfaction, pay satisfaction, working hours satisfaction, fair treatment by the company, relationships with co-workers and managers, and perceived fairness of the pay scheme.

6.6 Interaction Effects

We are also interested in examining the potential interaction effects between bonuses and monitoring. To do this, we estimate the following equation:

$$\begin{aligned} y_{ist} =& \nu_1 MonitorTreat_{is} \times MonitorPost_t \times BonusTreat_i + \\ & \nu_2 MonitorTreat_{is} \times MonitorPost_t \times SurpriseTreat_i + \\ & \delta_1 MonitorTreat_{is} \times MonitorPost_t + \delta_2 BonusTreat_{is} \times MonitorPost_t + \\ & \delta_3 SurpriseTreat_{is} \times MonitorPost_t + \eta_{is} + \kappa_t + \epsilon_{ist} \end{aligned}$$

where the key coefficients of interest are the ones on the triple interactions, ν_1 and ν_2. If the coefficients on the triple interaction are positive, this suggests that monitoring and bonuses implemented together produce stronger effects on worker outcomes than either approach alone. Alternatively, a negative estimate of the coefficients of the triple interactions would indicate that bonuses dilute the impact of monitoring or that additional monitoring dilutes the effects of bonuses.

In Appendix Table A7, we see that none of the triple interactions between the monitoring treatment, post monitoring and the financial treatments are significant at the standard levels. The lack of significant interaction effects is not surprising given that each type of intervention led to changes in different outcomes.

7 Robustness

7.1 Potential Spillovers in the Financial Interventions

There are several ways that spillovers could affect the results. One potential concern for the individual-level bonuses is that the control group was upset to learn that other people, who were hired around the same time, received additional bonuses that they did not receive. If this occurred, then we may be concerned that the estimates presented in the paper are overestimates of the impacts that we would expect if this were rolled out to all workers because part of the estimated effect in the experiment is driven by the control group moving in the opposite direction on outcomes. We may also be concerned about a misattribution of the mechanism driving the changes. For example, a worker who realizes they are not receiving the same bonuses as their

colleagues may reduce their effort, work fewer hours, or be more likely to quit. Similarly, if managers are aware that some workers receive extra compensation while others do not, they may adjust their expectations and behavior differently toward each group, further influencing outcomes.

Given the compensation non-disclosure agreements that new hires signed, workers were technically not supposed to discuss their compensation with other people at the firm (outside of the human resources department). However, we do several things to check whether there is evidence that spillovers are driving any of the results. First, we exploit questions in the endline survey that ask workers whether they discussed their salary with managers and with their co-workers. The summary statistics for the control group in Appendix Table A8 shows that the majority of workers did not have salary discussions with either their managers or their co-workers; 8.9% of workers reported having salary discussions with their managers and 28% report having salary discussions with their co-workers. Second, the coefficient estimates shown in columns 1 through 4 of Appendix Table A8 shows that the treatment groups were not significantly more or less likely to discuss their salary with either managers or co-workers relative to the control group.

We also make use of a question in the endline survey about whether the respondent agrees with a statement that the pay scheme of the company is fair. This question is asked on a five point scale with the value of 2 (which is the approximate mean of the control group) corresponding to them agreeing with the statement.[36] In the last two columns of Appendix Table A8, we see the control and treatment groups are not significantly different in whether they think the pay scheme at the company is fair.

Next, we look at whether having more exposure to people who received the bonus pay matters for workers in the financial control groups. We first look at whether having a higher share of teammates getting the signing bonus or surprise bonus affects outcomes of a control group work.[37] In Table 6, we consider four outcomes: the work satisfaction index, whether the worker agrees with the statement that they are being treated fairly by the firm, their performance evaluation score, and the total hours that they work. We picked these because we think that workers who think the firm is not treating them fairly may have lower work satisfaction, be more likely to disagree with statements about being treated fairly, perform worse in the job and be

[36] A value of 1 would correspond to strongly agree and a 5 strongly disagree.

[37] The means of the regressors in Table 6 are shown in Appendix Table A10. Given that teams are comprised both of new hires who were part of our experiments and existing workers who were not, the average share of team members who are treated is low.

unwilling to work extra hours. Across all the outcomes in columns 1, 3, 5, and 7 of Table 6, having more teammates receiving the bonus does not significantly change any of these outcomes. Indeed, most of the coefficients are positive, which is the opposite of what we would expect if they were more likely to learn about the bonuses that other workers received through having more treated teammates.

We also ask two sets of questions about social networks at the firm in the endline survey. We ask about their three closest friends on the team and their three closest friends at the firm (outside of their team). We then are able to construct for each worker what share of their friendship network was in the bonus treatment group. The results for the impact of having more friends who received the financial treatments are shown in columns 2, 4, 6, and 8 of Table 6. Again, none of these coefficients are statistically different from zero at the standard levels. Of the sixteen coefficients, 13 of them are positive, which is in the opposite direction of these workers being unhappy with the firm if they learn from their treated friends about the bonuses.

Overall, there is no evidence to suggest that main results on the financial payments are driven by spillovers in which the control group workers changed their behavior because of dissatisfaction from learning that other people at the company had received a different compensation package. There is also no evidence that there were many salary discussions that workers initiated with team managers, so it is unlikely that managers knew who received bonuses and who did not and treated those groups differently.

7.2 Potential Spillovers in the Monitoring Intervention

There are two potential concerns regarding spillovers with monitoring. First, control stations near the treatment stations may also feel like they are being monitored more frequently if the monitoring team is working nearby more frequently. Unlike for the bonus spillovers, such spillovers here would *attenuate* the estimated effects of increased monitoring. Second, workers in treatment stations may react negatively if they realized that they are being monitored more than control stations.[38] A negative response may be natural if they infer that the firms thinks that they have done something to warrant additional monitoring. If some workers getting more relatively monitoring assume that this is a signal that the firm views them as low quality workers, this can lead to those workers quitting more or exerting more effort.

[38] If everyone at the factory was monitored more, they would not have the same negative reaction.

To test for the presence of both kinds of spillovers, we asked the firm to provide us with a map of the location of the stations so that we could determine which control stations are adjacent to the treatment stations. We use this map to construct a variable for the share of adjacent neighbors who were treated in the monitoring experiment for control stations.[39] For the first type of spillover, we would expect having more neighbors treated for *control* group stations has the same impacts as being monitored directly. In other words, we would expect their evaluation scores to increase with having more neighbors treated. For the second type of spillover, we would expect having more neighbors treated for *treatment* stations to increase their work satisfaction and perception that the firm treats workers fairly and to reduce effort outcomes.

We show the impacts of having a larger share of treated neighbors for control and treatment stations in Table 7. To examine the first type of spillover where we focus on the impact of having more treated neighbors for the control group, we are interested in the coefficient on $Neighbor\ MonitorTreat\ Share$. For the control group, we see no significant impacts of a greater share of treated neighbors on work satisfaction, whether they report fair treatment, their evaluation scores and hours worked. To test for the second type of spillover where we focus on having more treated neighnbors for the treatment group, we are interested in the coefficient on the interaction term $MonitorTreat\times\ Neighbor\ MonitorTreat\ Share$. Again, none of the coefficients are significantly different from zero. Overall, there is no evidence for any kind of spatial spillovers in the monitoring experiment.

7.3 Firm Assignment of Teams

We consider the possibility that hiring managers treated new hires differently based on whether they were in the signing bonus treatment or control group. Although randomization ensures that the ability and skills of workers in both groups should be similar, hiring managers might have perceived the additional bonus as particularly beneficial for certain worker-task or worker-team matches. For example, they may have assigned workers receiving the bonus to teams working on more difficult tasks. If this were the case, the results presented in the paper could be driven by task assignment rather than solely the additional bonus.

To test this, we compare the task and team assignments between treated and control workers

[39] As shown in the last row of Appendix Table A10, control stations have an average of 15% of their neighboring stations treated by additional monitoring.

in the signing bonus experiment in Appendix Table A9. To measure potential differences across teams, we calculate the average characteristics of workers assigned to each task and team, using pre-experiment administrative data. The results show no significant differences in task assignments (Panel A) or team assignments (Panel B), as measured by workers' average total hours worked and earnings.

8 Discussion

8.1 Selection Driven by Worker Separations

As we showed in Table 1, the signing bonus did not induce higher quality workers to accept the offer to join the firm. Given that the additional bonuses did decrease the quit rate (Panel A of Table 5), we now examine whether workers who stayed at the firm differ from those who quit based on measures of worker performance (evaluation scores and performance bonuses) and effort (hours worked and earnings).

We also observed that the monitoring intervention led to significant changes in worker turnover, but in the opposite direction of the financial incentives. Several factors may explain why higher levels of monitoring increase turnover. First, workers may dislike increased monitoring, which could lead to higher quit rates. Second, monitoring may enable firms to identify unsuitable workers more quickly, accelerating inevitable separations. While this second mechanism may initially incur costs related to hiring and training replacements, it could ultimately benefit the firm by replacing low-performing workers with better-suited candidates.

To test for differential selection based on worker quality between quitters and stayers, we estimate a regression similar to Table 5, with the outcome being whether the worker is still employed at the firm at the end of the calendar year. We include several measures of worker quality (whether the individual's average over time is above or below the median in the sample) and their interaction with the interventions. We are particularly interested in the interaction terms, as they indicate whether the workers who quit in the treatment group differ in these characteristics from those who quit in the control group.

As shown in Panel A of Table 8, for the signing bonus intervention, none of the interaction terms are significant at the standard levels. However, for the surprise bonus intervention, the interaction between workers who worked above-median hours and the treatment is negative and

significant at the 1% level. This suggests that workers more likely to stay due to the surprise bonus tend to be those who work fewer hours, indicating a potential retention of lower-effort workers. However, these retained workers do not significantly differ in job performance compared to others. While the signing bonus does not appear to have changed the characteristics of workers who join the firm, there is evidence that the surprise bonus did change the selection of types of workers who quit. This suggests that the differences observed in the coefficient estimates of α and β throughout the paper may be driven by differential selection among quitters.

Turning to the monitoring results, as shown in Panel B of Table 8, none of the interaction terms of significant at the standard levels. This suggests that the composition of workers leaving the firm does not differ significantly between the treatment group, which received increased monitoring, and the control group, which experienced standard monitoring. Therefore, while increased monitoring resulted in higher turnover, it did not disproportionately lead to the separation of lower-quality workers.

8.2 Cost-Benefit Analysis

We now compare the gains from the interventions with the associated costs faced by the firm. Given that the interventions had significant effects on worker retention, one of the key cost and benefits relate to the cost that firm bears to hire and train new employees.[40]

First, the screening process for new hires involves interview costs associated with each hiring batch. In our analysis, the three hiring batches required a total of 15 interview days. Based on data provided by the firm, the salaries of the three human resources staff members conducting these interviews over the 15-day period were used to calculate the cost per hire. Dividing the total salary expenditure by the 253 workers hired, we estimate the interview cost per new hire to be 62 RMB.

New hires undergo three days of general firm-level training, during which they do not contribute to production but are paid for 8 hours per day at a rate of 26 RMB per hour, totaling 624 RMB per worker. Additionally, the firm incurs training-related costs for two staff members conducting these sessions. Based on their salaries, this amounts to an additional 25 RMB per new hire.

This is followed by seven days of job-specific training, which is broken down into three days

[40]Note that in this context the staffing agencies who connect workers to the job interview at the firm are paid by the potential applicants, not by the firm, so the initial matching costs are not born by the firm.

of pure training (where they are generating no output) and four days on the production line working alongside an experienced worker (where the firm estimates they produce 50% of a regular worker's output). During the job-specific training period, workers are paid for 10 hours per day at the same rate of 26 RMB per hour. The firm's cost of a new worker's job-specific training is calculated as 100% of the wage cost for the first three days and 50% of the wage cost for the next four days, totaling 1,300 RMB per new hire. Additionally, new workers are provided with workwear, including uniforms and boots, which costs approximately 182 RMB per worker. In total, the cost of hiring, training and outfitting a new worker amounts to 2,193 RMB.

Starting with the cost-benefit calculation of the signing bonus treatment, it increased the take up of the job offer by 10.6 percentage points (Appendix Table A3) and reduced worker quits by 18.7 percentage points (Table 5, Panel A, column 1). Together, these correspond to a reduction in hiring and training costs of 642 RMB per worker for the firm. Additionally, the treatment increased workers' monthly earnings by 17% (Table 4, Panel A, column 5), with control group workers earning an average of 2,500 RMB per month. Assuming that the firm and workers share the gains from increased effort equally,[41] this translates to an estimated monthly profit gain of 425 RMB for the firm, or 1,275 RMB over the three-month study period. Thus, the total benefits per bonus-treated worker amount to 1,917 RMB. At the same time, the actual cost of the bonus per worker is lower than the nominal 1,600 RMB, as 45% of treated workers left before the payout period, reducing the firm's effective cost per worker to 880 RMB. As a result, the total benefits per bonus-treated worker significantly outweighing the costs.

For the surprise treatment, the intervention reduced worker quits by 55.1 percentage points (Table 5, Panel A, column 1), resulting in training cost savings of 1,208 RMB per worker. It also led to a 26.2% increase in monthly earnings (Table 4, Panel A, column 5), translating to an additional 1,965 RMB per worker over the three-month study period. Assuming that the firm and workers share these productivity gains equally, this implies that the firm also profited by 3,173 RMB per worker. Consequently, the overall gains to the firm exceeds the cost of the surprise treatment (1,600 RMB per worker).

For the monitoring intervention, the time value of the monitors for each station corresponds with 60 RMB.[42] With 33 treated stations, the total cost of the monitoring was 1,980 RMB. The monitoring treatment led to a 12.5 percentage point increase in worker attrition (Table 5, Panel

[41] Kline et al. (2019) find the pass-through from value added per worker to wages is 0.47.
[42] This corresponds to an 8 minute visit per station for 6 weeks.

B, column 1), which raised the costs of hiring and training by 274 RMB per worker. However, the intervention also increased worker bonuses by 23.3 RMB per month and performance scores by 3.1 points (Table 3, Panel B).[43] Given that 114 workers were in the treated stations, the total bonus increase amounted to 11,933 RMB per month. Since our intervention lasted for 1.5 months, this indicates that the intervention improved firm profit (13,261 RMB total) less than the increased in the likelihood of turnover (31,236 RMB), suggesting that the costs of the monitoring treatment significantly outweigh its benefits, which corresponds to losses of 158 RMB per worker. This is consistent with the emerging literature that emphasizes that workers may dislike additional monitoring (de Rochambeau, 2021; Friebel et al., 2024).

Overall, the cost-benefit calculations suggest that the firm gets a much higher return in increasing the total compensation of workers than in increasing monitoring. Interestingly, the cost-effectiveness of both types of bonuses are fairly similar to each other with the signing bonus prior to hire yielding slightly higher returns. While the surprise bonus corresponds to larger effects on retention and hours worked, the signing bonus is less costly and has the additional benefits of increasing take up of the job offer and reducing quits in the initial month after hire.

9 Conclusion

This study provides novel empirical evidence on the effectiveness of financial incentives and monitoring in improving worker and firm outcomes. Using a randomized field experiment conducted at an automobile manufacturing firm in China, we independently vary new hires' compensation packages and the intensity of workplace monitoring to examine their respective impacts on worker effort and retention. Our results highlight the distinct mechanisms through which these interventions operate, showing that financial incentives and monitoring are neither substitutes nor complements, but influence different aspects of worker behavior.

First, we find that offering signing and surprise bonuses significantly increases hours worked and reduces quit rates. Workers who receive financial incentives work longer hours and earn higher overall wages. However, their performance, as evaluated by production line and plant-level managers, does not improve. This suggests that while higher pay encourages workers to

[43]For new hires in the initial period before their conversion to permanent workers, they are not paid bonuses based on their performance scores. However, we are able to estimate the returns for permanent workers where we find that each additional point corresponds to 17.5 RMB per month. Again, we assume that workers and firms split the profit gain from better performance equally.

stay employed and work longer hours, it does not necessarily improve the quality of their output. These findings are consistent with theories of gift exchange and efficiency wages, which posits that higher pay incentivizes effort, but may not directly enhance skill-dependent performance without additional support or feedback.

In contrast, enhanced monitoring leads to better worker performance, as reflected in managerial evaluations, but does not affect hours worked. This performance gain may stem from the increased feedback workers receive under closer supervision. However, intensified monitoring also results in higher attrition, indicating that workers may view increased oversight as burdensome or undesirable. Thus, while monitoring can improve compliance and productivity, its long-term costs—particularly increased turnover—must be carefully weighed against its benefits.

Our cost-benefit analysis reveals that increasing compensation through signing bonuses is a cost-effective strategy for firms seeking to attract and retain workers. Offering signing bonuses not only boosts job acceptance rates but also reduces early quit rates, making it a valuable tool for firms facing high turnover. In contrast, while monitoring improves performance, it is less cost-effective due to the expenses associated with replacing workers who leave in response to heightened scrutiny.

These findings have significant implications for labor market policies and managerial practices. Firms aiming to enhance workforce stability should prioritize financial incentives over monitoring, especially in contexts where recruitment and training costs are high. For firms aiming to improve work quality, monitoring may still prove beneficial — but efforts should be made to mitigate its negative impact on retention. Future research can test strategies to mitigate the negative consequences of monitoring such as combining oversight with mentorship or performance-based rewards.

Overall, our results indicate that both financial incentives and monitoring are powerful tools for firms looking to optimize workforce productivity and retention. Bonuses encourage workers to stay and increase their effort in terms of hours worked, while monitoring enhances performance but may lead to greater turnover. Designing an optimal labor contract requires balancing these trade-offs to foster a productive and stable workforce.

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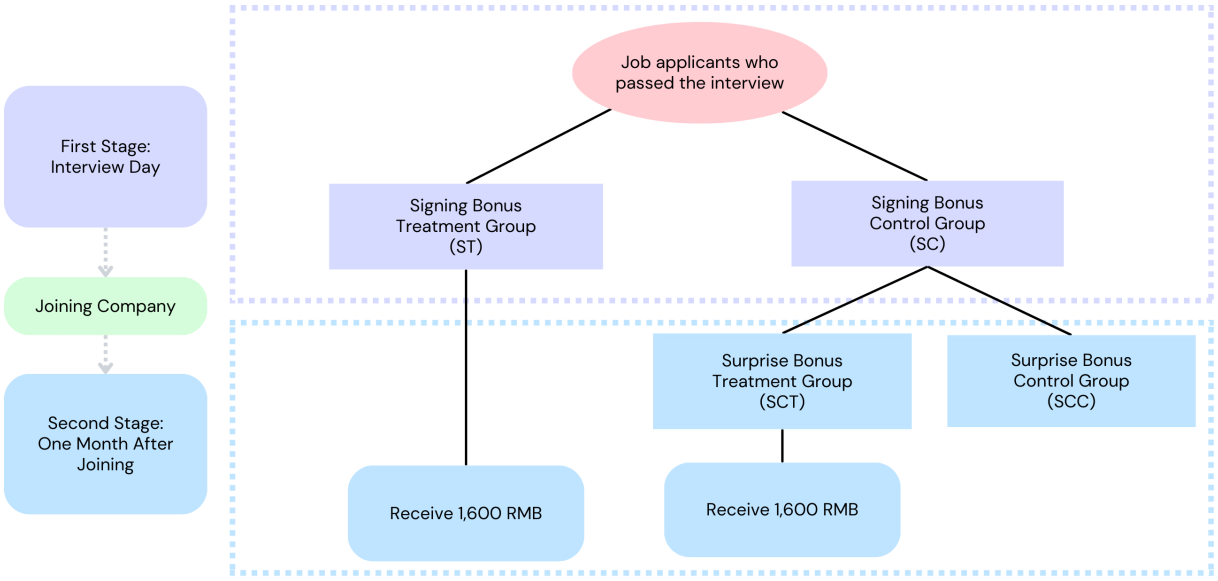
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Figure 1: Design of the Experiments

(a) The Hiring Experiment



(b) The Monitoring Experiment

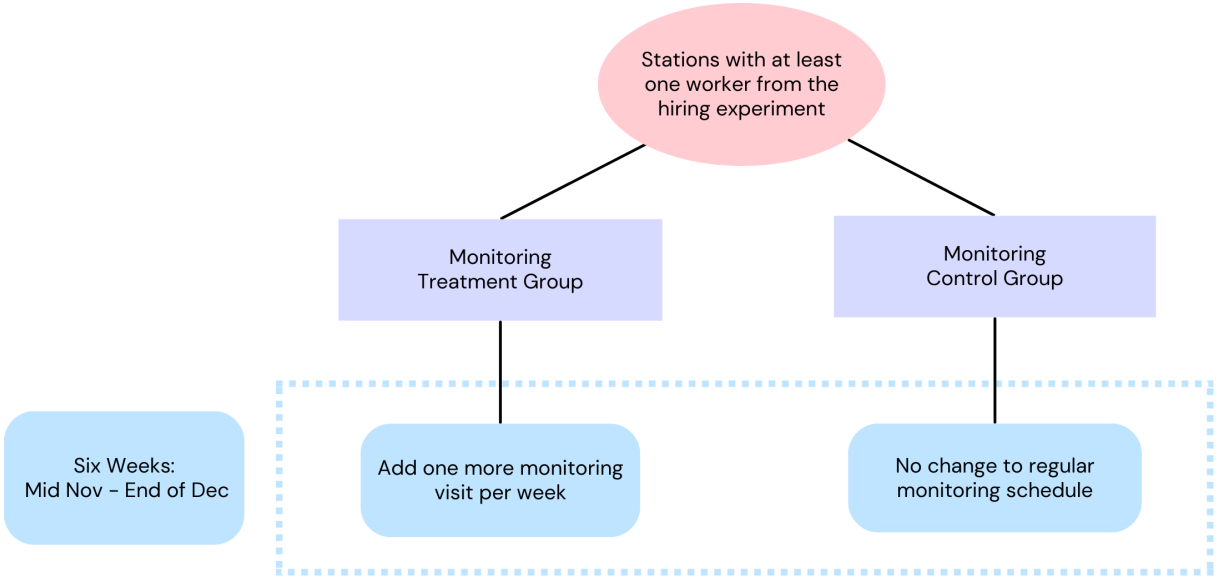


Figure 2: Timeline

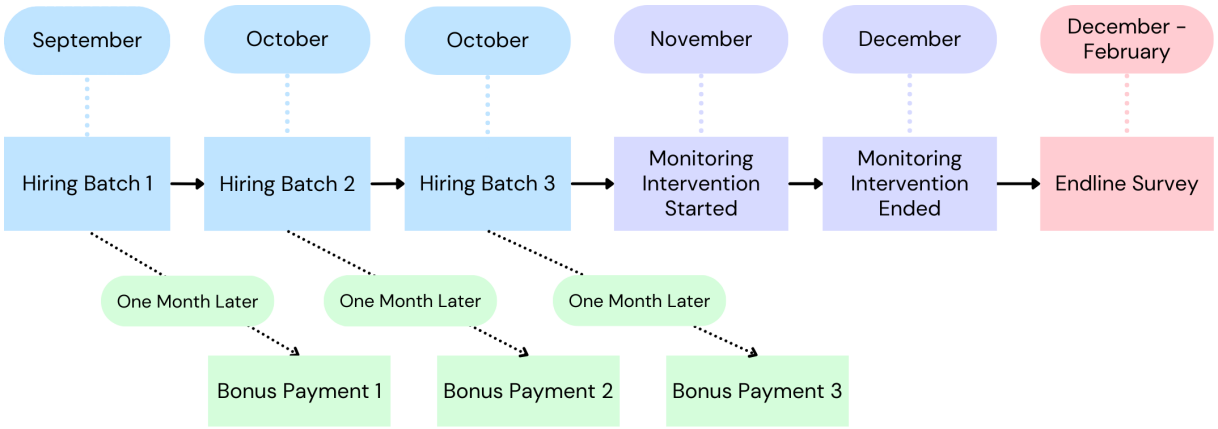


Table 1: Baseline Summary Statistics For Job Takers

	Bonus Treatment		
	Treatment	Control	p-value
Male	0.97	0.97	0.86
	(0.16)	(0.17)	
Secondary School	0.71	0.72	0.87
	(0.46)	(0.45)	
Skilled	0.05	0.05	0.90
	(0.22)	(0.21)	
Local Hometown	0.48	0.33	0.28
	(0.51)	(0.48)	
Age	25.99	25.87	0.90
	(6.78)	(6.80)	
Willingness to Convert	1.92	1.95	0.85
	(0.99)	(0.95)	
Interviewer Assessment	2.53	2.61	0.43
	(0.66)	(0.69)	
Observations	80	173	

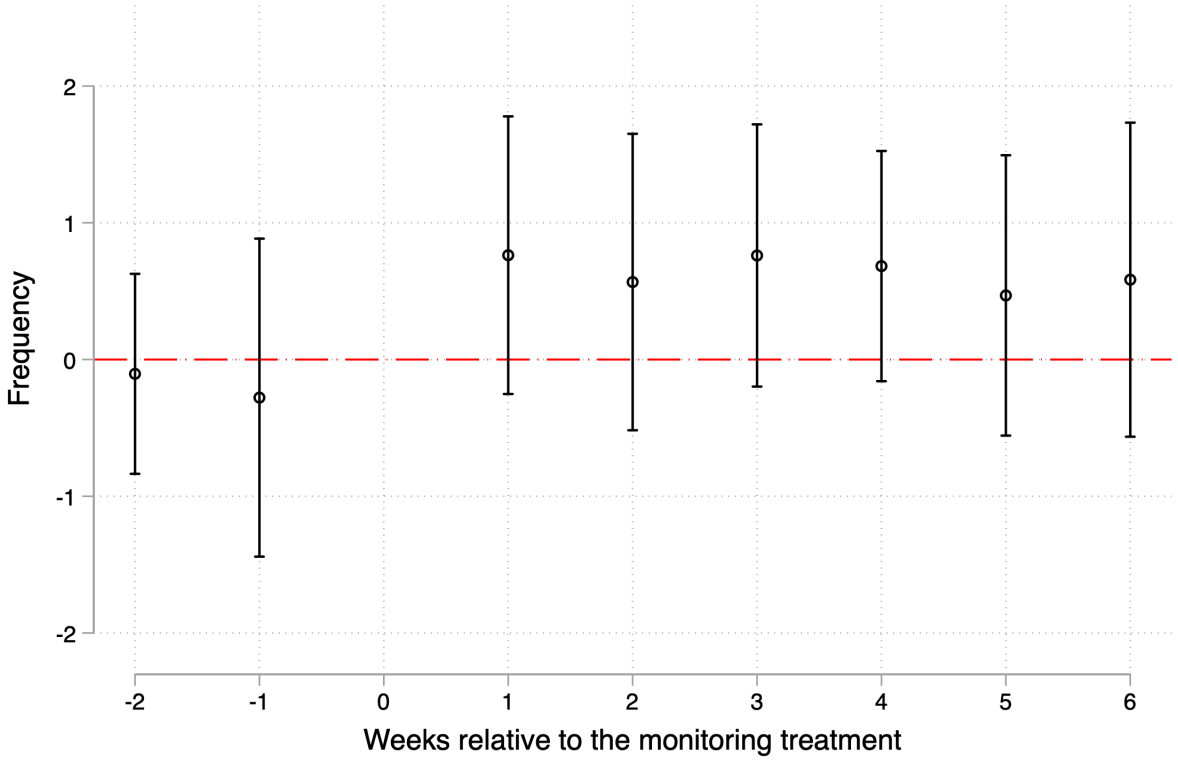
Note: The table shows the mean of each variable with the standard deviation underneath in parentheses. Workers' age, willingness to convert, and ability level are from the baseline application; gender, education, skill levels, and hometown are from administrative data. The p-value is taken from a regression testing the statistcal difference between the treatment and control groups.

Table 2: Impact of Monitoring Intervention on Monitoring Outcomes

	Station Data		Individual Data	
	External Production Monitoring Visits	All Other Monitoring Visits	Completed Monitoring Forms	
	(1)	(2)	(3)	(4)
MonitorTreat × MonitorPost	0.763**	6.041		
	(0.327)	(6.062)		
MonitorTreat			0.290***	0.289***
			(0.050)	(0.049)
Control Mean	1.83	14.9	0.010	0.010
Observations	602	602	320	320
Additional Controls				Y

Note: Columns (1) and (2) include station and week fixed effects. Columns (3) and (4) include team fixed effects and the outcome is an indicator. All other monitoring visits include those by the plant head, the plant manager, the plant engineer, the quality department, and any other staff outside of the external production monitoring team. Column (4) includes controls for the other experiment(s). ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$. Standard errors are displayed in parentheses.

Figure 3: Leads and Lags around the Monitoring Intervention on Monitoring Visits



Note: The data is at the station level. The coefficients show the interactions between the monitoring treatment and the periods around the start of the monitoring experiment. The specification is based on the experimental stations and include station fixed effects and week fixed effects. The outcome is the number of external production monitoring visits. Standard errors are clustered at the station level. The bars show the 95% confidence intervals.

Table 3: Impact of the Interventions on Performance

	Evaluation Score		Performance Bonus	
	(1)	(2)	(3)	(4)
Panel A: Financial Treatments				
BonusTreat	-0.166	-0.168	1.493	1.373
	(1.073)	(1.080)	(5.811)	(5.909)
SurpriseTreat	0.728	0.729	-3.968	-3.954
	(0.959)	(0.961)	(5.006)	(5.003)
Control Mean	80.8	80.8	4.51	4.51
Observations	341	341	352	352
p-value	0.43	0.43	0.42	0.44
Panel B: Monitoring Treatment				
MonitorTreat × MonitorPost	3.113**		23.312**	
	(1.497)		(10.093)	
Control Mean	81.3		5.95	
Observations	262		267	
Additional Controls		Y		Y

Note: Panel A includes batch, team, and month fixed effects. Panel B includes individual and month fixed effects. Standard errors are clustered at the individual level in Panel A and at the station level in Panel B. The p-value in Panel A indicates whether BonusTreat and SurpriseTreat are statistically different from each other. Columns (2) and (4) include controls for the other experiment(s). ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$. Standard errors are displayed in parentheses.

Table 4: Impact of the Intervention on Earnings and Hours

	Total Hours		Monthly Earnings	
	(1)	(2)	(3)	(4)
Panel A: Financial Treatments				
BonusTreat	20.538***	16.447**	0.170***	0.160**
	(6.552)	(6.643)	(0.063)	(0.063)
SurpriseTreat	32.881***	29.588***	0.262***	0.251***
	(6.201)	(6.359)	(0.055)	(0.053)
Control Mean	132.4	132.4	8.51	8.51
Observations	680	680	624	624
p-value	0.029	0.030	0.090	0.10
Panel B: Monitoring Treatment				
MonitorTreat × MonitorPost	-11.234		0.040	
	(17.988)		(0.191)	
Control Mean	179.8		8.88	
Observations	321		321	
Additional Controls		Y		Y

Note: Monthly Earnings has been transformed using the inverse hyperbolic sine function. Panel A includes batch, team, and month fixed effects. Panel B includes individual and month fixed effects. Standard errors are clustered at the individual level in Panel A and at the station level in Panel B. The p-value in Panel A indicates whether BonusTreat and SurpriseTreat are statistically different from each other. Columns (2), (4), and (6) include controls for the other experiment(s). ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$. Standard errors are displayed in parentheses.

Table 5: Impact of the Interventions on Staying at the Firm

	(1)	(2)
Panel A: Financial Treatments		
BonusTreat	0.187***	0.171***
	(0.065)	(0.064)
SurpriseTreat	0.551***	0.511***
	(0.081)	(0.080)
Control Mean	0.38	0.38
Observations	253	253
p-value	0	0
Panel B: Monitoring Treatment		
MonitorTreat	-0.125**	-0.121**
	(0.050)	(0.049)
Control Mean	0.83	0.83
Observations	320	320
Additional Controls		Y

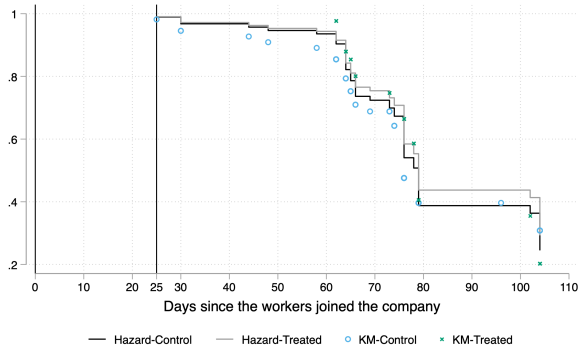
Note: All specifications include batch and team fixed effects, and standard errors are clustered at the station level in Panel B. The p-value in Panel A indicates whether BonusTreat and SurpriseTreat are statistically different from each other. Column (2) include controls for the other experiment(s). ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$. Standard errors are displayed in parentheses.

Figure 4: Kaplan-Meier and Hazard Model Estimates of the Probability of Staying at the Firm

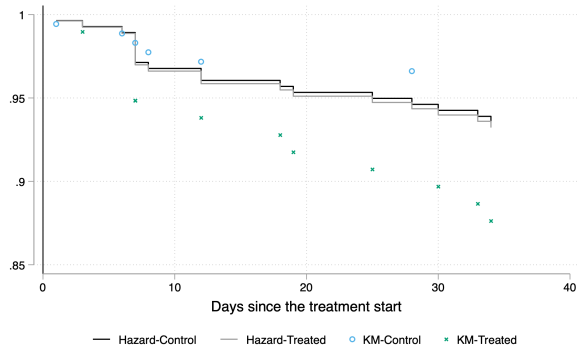
Panel A: Bonus Treatment



Panel B: Surprise Treatment



Panel C: Monitoring Treatment



Note: The figure shows the Kaplan-Meier and hazard estimates of the probability of workers staying at the firm in the days after they joined the firm (in Panels A and B) and following the start of the monitoring treatment (in Panel C). December 31, 2023, is used as the censoring date for workers who have not yet left the firm.

Table 6: Spillover Effects from Financial Treatments

	Work Satisfaction Index		Treat Worker Fairly		Evaluation Score		Total Hours	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Team BonusTreat Share	-4.442		4.183		6.383		100.924	
	(4.327)		(3.325)		(17.740)		(106.940)	
Team SurpriseTreat Share	3.218		-3.831		-35.873		93.302	
	(4.784)		(3.676)		(28.253)		(131.205)	
Team Friends BonusTreat Share		-0.574		0.879		1.005		-4.990
		(1.626)		(1.284)		(5.405)		(47.711)
Team Friends SurpriseTreat Share		-0.640		0.410		1.406		5.346
		(0.520)		(0.411)		(1.969)		(16.403)
Firm Friends BonusTreat Share		-0.622		0.307		0.687		7.207
		(1.491)		(1.178)		(5.147)		(44.995)
Firm Friends SurpriseTreat Share		0.857		-0.340		2.236		9.713
		(0.618)		(0.488)		(2.111)		(18.977)
Outcome Mean	-0.060	-0.066	1.88	1.88	81.5	81.4	182.3	182.0
Observations	43	50	43	50	106	114	165	178

Note: All specifications include batch fixed effects, with month fixed effects added in Columns (5)-(8). ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$. Standard errors are displayed in parentheses.

Table 7: Spillover Effects from Monitoring Treatment

	Work Satisfaction Index	Treat Worker Fairly	Evaluation Score	Total Hours
	(1)	(2)	(3)	(4)
Neighbor MonitorTreat Share	-0.369	0.236	0.717	-4.868
	(0.348)	(0.311)	(2.486)	(15.462)
MonitorTreat × Neighbor MonitorTreat Share	-0.023	-0.040	-2.251	-1.695
	(0.616)	(0.515)	(3.111)	(21.832)
MonitorTreat	0.233	-0.069	0.373	-7.941
	(0.298)	(0.271)	(1.258)	(9.500)
Outcome Mean	-0.24	2.02	83.3	201.7
Observations	137	137	306	367

Note: All specifications include batch fixed effects, with month fixed effects added in Columns (3) and (4). The sample in columns (3) and (4) is limited to post monitoring Standard errors are clustered at the station level. ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$. Standard errors are displayed in parentheses.

Table 8: Impact of the Interventions on Worker Retention by Worker Quality

Interaction Variable =	Evaluation Score (1)	Performance Bonus (2)	Total Hours (3)	Monthly Earnings (4)
Panel A: Financial Treatments				
BonusTreat × *Variable*	-0.114	-0.153	-0.191	-0.239
	(0.210)	(0.241)	(0.179)	(0.479)
SurpriseTreat × *Variable*	-0.215	-0.380	-0.642***	-0.456
	(0.206)	(0.245)	(0.189)	(0.378)
BonusTreat	0.136	0.083	0.175***	0.193***
	(0.102)	(0.101)	(0.064)	(0.072)
SurpriseTreat	0.326***	0.366***	0.627***	0.445***
	(0.105)	(0.105)	(0.085)	(0.086)
Variable	0.251*	0.397**	0.756***	0.425
	(0.138)	(0.163)	(0.131)	(0.319)
Control Mean	0.72	0.71	0.38	0.52
Observations	123	127	253	205
p-value	0.10	0.01	0.00	0.01
Panel B: Monitoring Treatment				
MonitorTreat × *Variable*	0.063	-0.022	-0.029	0.103
	(0.072)	(0.085)	(0.075)	(0.109)
MonitorTreat	-0.114**	-0.064	-0.129**	-0.147**
	(0.053)	(0.055)	(0.058)	(0.058)
Variable	0.033	0.093**	0.266***	0.167**
	(0.040)	(0.043)	(0.062)	(0.076)
Control Mean	0.95	0.93	0.83	0.83
Observations	278	282	320	320

Note: The dependent variable is a binary indicator whether the worker is still at the firm as of December 2023. *Variable* refers to whether the worker is above median in the measure indicated in the column title. All specifications include batch and team fixed effects, and standard errors are clustered at the station level in Panel B. The p-value in Panel A indicates whether BonusTreat and SurpriseTreat are statistically different from each other. ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$. Standard errors are displayed in parentheses.

A Appendix: Tables and Figures

Table A1: Baseline Summary Statistics

	Bonus Treatment			Surprise Treatment		
	Treatment	Control	p-value	Treatment	Control	p-value
Male	0.98	0.97	0.73	0.95	0.96	0.81
	(0.14)	(0.16)		(0.21)	(0.19)	
Secondary School	0.72	0.71	0.93	0.75	0.80	0.53
	(0.45)	(0.45)		(0.44)	(0.40)	
Skilled	0.05	0.04	0.84	0.07	0.07	0.93
	(0.22)	(0.21)		(0.25)	(0.26)	
Local Hometown	0.48	0.35	0.35	0.18	0.50	0.11
	(0.51)	(0.48)		(0.40)	(0.52)	
Age	25.91	25.87	0.96	25.07	25.46	0.74
	(6.73)	(6.74)		(6.23)	(5.59)	
Willingness to Convert	1.88	1.90	0.86	1.98	1.91	0.74
	(0.99)	(0.95)		(0.96)	(1.02)	
Interviewer Assessment	2.51	2.53	0.77	2.65	2.70	0.71
	(0.65)	(0.69)		(0.69)	(0.69)	
Observations	100	228		44	56	

Note: The table shows the mean of each variable with the standard deviation underneath in parentheses. Workers' age, willingness to convert to permanent worker (on a scale from 0 to 3), and interviewer assessment (on a scale from 1 to 4) are from the baseline application; gender, education, skill levels, and hometown are from administrative data. The p-value is taken from a regression testing the statistcal difference between the treatment and control groups.

Table A2: Baseline Summary Statistics by Monitoring Treatment Status

	Monitoring Treatment		
	Treatment	Control	p-value
Male	0.91	0.97	0.18
	(0.28)	(0.17)	
Secondary School	0.86	0.86	0.88
	(0.34)	(0.35)	
Skilled	0.03	0.04	0.59
	(0.16)	(0.20)	
Local Hometown	0.20	0.27	0.23
	(0.40)	(0.44)	
Age	24.15	23.11	0.32
	(7.61)	(6.63)	
Observations	114	206	

Note: Workers' age is sourced from the baseline application, while gender, education, skill levels, and hometown are from administrative records. The sample includes all of the monitoring data. Standard errors are clustered at the station level for the monitoring treatment.

Table A3: Impact of the Bonus Treatment on Job Taking

	(1)
BonusTreat	0.106**
	(0.053)
Control Mean	0.68
Observations	328

Note: The specifications include batch fixed effects. ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$. Standard errors are displayed in parentheses.

Table A4: Baseline Summary Statistics for Applicants who Rejected Offer

	Bonus Treatment		
	Treatment	Control	p-value
Male	1.00	0.99	0.59
	(0.00)	(0.12)	
Secondary School	0.76	0.68	0.48
	(0.44)	(0.47)	
Age	25.48	26.15	0.69
	(6.53)	(6.78)	
Willingness to Convert	1.76	1.84	0.74
	(1.00)	(0.96)	
Interviewer Assessment	2.45	2.38	0.66
	(0.60)	(0.67)	
Observations	21	72	

Note: The sample is job applicants who received a job offer but did not accept. The table shows the mean of each variable with the standard deviation underneath in parentheses. Workers' gender, education, age, willingness to convert to permanent worker (on a scale from 0 to 3), and interviewer assessment (on a scale from 1 to 4) are from the baseline application. The p-value is taken from a regression testing the statistcal difference between the treatment and control groups.

Figure A1: Effects of Financial Treatments over Time

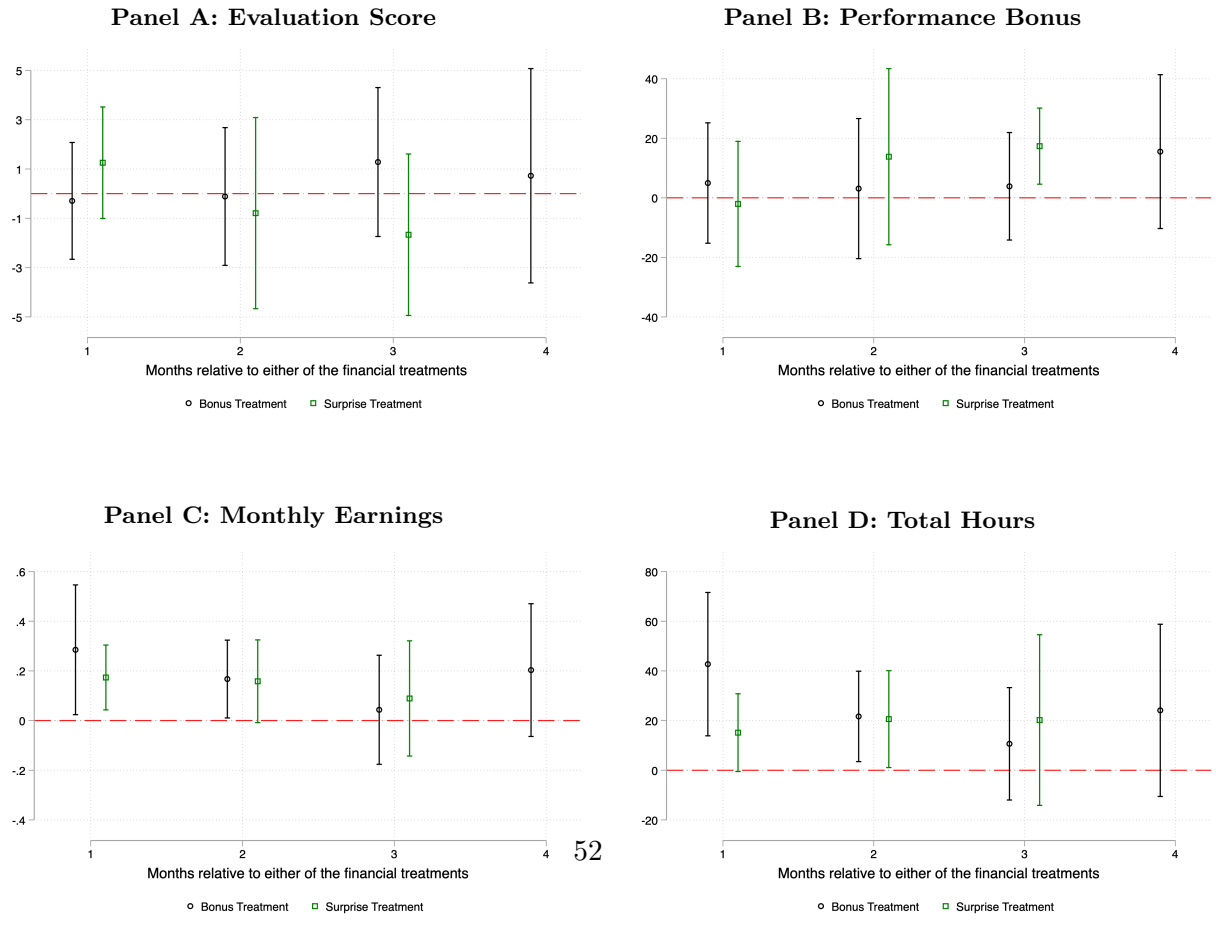
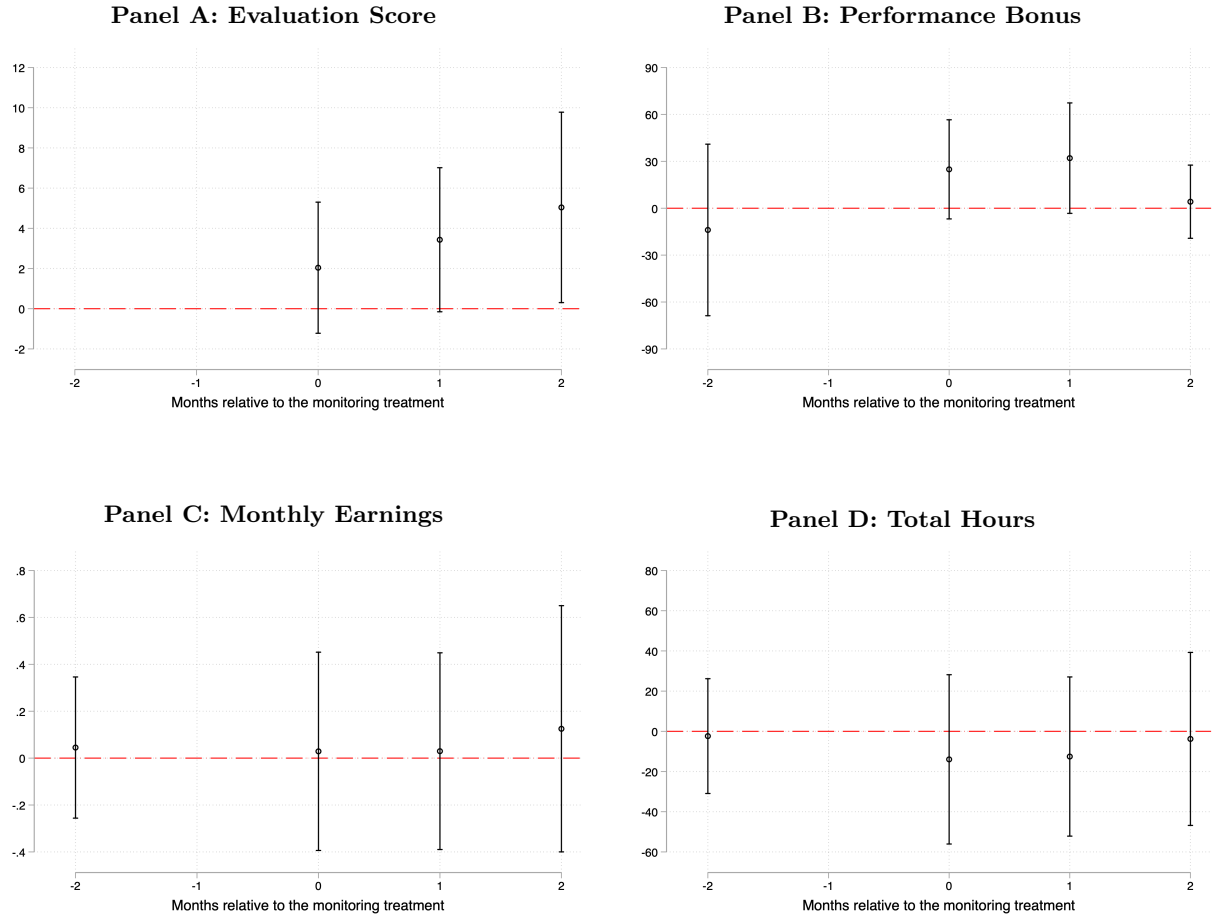


Figure A2: Effects of Monitoring Treatment over Time



Note: Monthly Earnings has been transformed using the inverse hyperbolic sine function. The specifications include individual fixed effects. The dots represent the coefficient estimates for the interaction between the month around the monitoring treatment and the monitoring treatment. The line denotes the 95% confidence interval, with standard errors clustered at the station level. In Panel A, we do not have observations of evaluation score data in the second month prior to the intervention to estimate the coefficient.

Table A5: Impact of the Interventions on Different Dimensions of Performance

	Quality (1)	Safety (2)	Production (3)	Equipment (4)	Composite (5)
Panel A: Financial Treatments					
BonusTreat	-0.411	0.210	0.101	-0.210	0.152
	(0.362)	(0.309)	(0.348)	(0.271)	(0.274)
SurpriseTreat	0.390	0.220	0.085	-0.207	0.249
	(0.324)	(0.283)	(0.280)	(0.262)	(0.300)
Control Mean	20.7	20.8	20.9	21.5	22.0
Observations	341	341	341	341	341
p-value	0.039	0.97	0.96	0.99	0.70
Panel B: Monitoring Treatment					
MonitorTreat × MonitorPost	0.951*	0.819*	0.455	0.192	0.697
	(0.487)	(0.483)	(0.482)	(0.508)	(0.449)
Control Mean	21	21.1	21.1	21.3	21.8
Observations	262	262	262	262	262

Note: The quality score considers the number of products failing quality tests and issues identified through monitoring. Safety accounts for accidents and violations of safety regulations. The production score evaluates meeting production targets as well as the accuracy and efficiency of task completion. The equipment score assesses proper maintenance and usage of equipment. The composite score includes any residual factors not covered by the other categories. Panel A includes batch, team, and month fixed effects. Panel B includes individual and month fixed effects. Standard errors are clustered at the individual level in Panel A and at the station level in Panel B. The p-value in Panel A indicates whether BonusTreat and SurpriseTreat are statistically different from each other. ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$. Standard errors are displayed in parentheses.

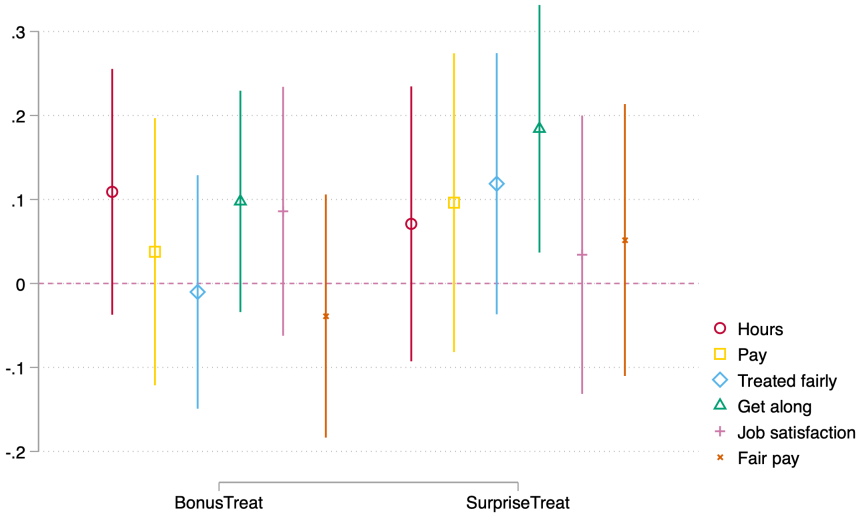
Table A6: Impact of the Interventions on Satisfaction

	Work Satisfaction Index		Well Being Ladder	
	(1)	(2)	(3)	(4)
Panel A: Financial Treatments				
BonusTreat	0.113	0.111	-0.474	-0.474
	(0.198)	(0.198)	(0.348)	(0.349)
SurpriseTreat	0.376*	0.392*	-0.504	-0.500
	(0.221)	(0.222)	(0.389)	(0.391)
Control Mean	-0.14	-0.14	8.45	8.45
Observations	189	189	189	189
p-value	0.26	0.23	0.94	0.95
Panel B: Monitoring Treatment				
MonitorTreat	-0.072	-0.057	0.064	0.015
	(0.183)	(0.186)	(0.260)	(0.256)
Control Mean	-0.16	-0.16	7.86	7.86
Observations	264	264	264	264
Additional Controls		Y		Y

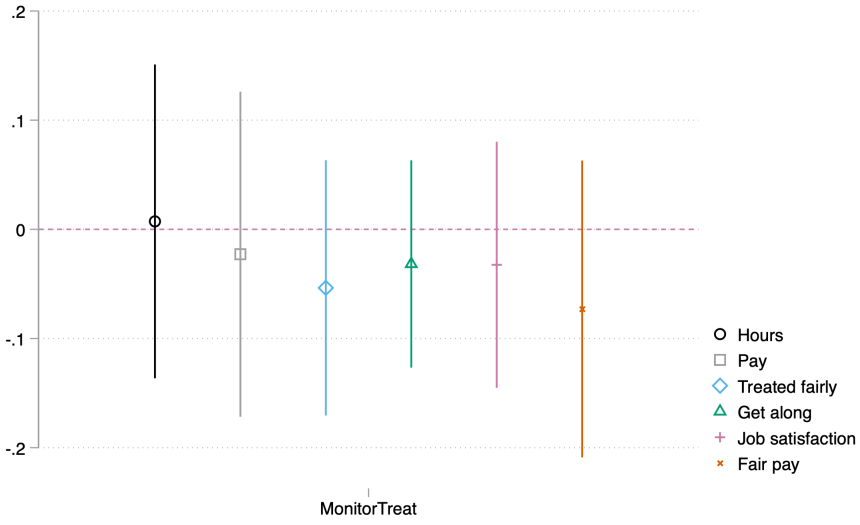
Note: All specifications include batch and team fixed effects, and standard errors are clustered at the station level in Panel B. The p-value in Panel A indicates whether BonusTreat and SurpriseTreat are statistically different from each other. Columns (2) and (4) include controls for the other experiment(s). ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$. Standard errors are displayed in parentheses.

Figure A3: Impacts of Interventions on Components of Work Satisfaction

Panel A: Financial Treatments



Panel B: Monitoring Treatment



Note: The figures show the coefficient estimates of the treatments, with each component as the outcome. The specifications use the full dataset and include batch and team fixed effects.

Table A7: Impacts of the Interventions Interacted on Performance, Earnings and Hours

	Evaluation Score (1)	Performance Bonus (2)	Total Hours (3)	Monthly Earnings (4)
MonitorTreat × MonitorPost × BonusTreat	-2.627	39.934	-29.362	-0.472
	(3.677)	(26.502)	(39.903)	(0.429)
MonitorTreat × MonitorPost × SurpriseTreat	1.670	-11.146	-23.337	-0.131
	(3.245)	(18.913)	(42.243)	(0.468)
BonusTreat × MonitorPost	2.197	3.068	22.929	0.290
	(2.080)	(12.327)	(24.946)	(0.256)
SurpriseTreat × MonitorPost	-0.513	5.493	10.291	0.031
	(1.988)	(10.952)	(21.157)	(0.220)
MonitorTreat × MonitorPost	3.430	16.019	6.318	0.238
	(2.862)	(15.314)	(31.476)	(0.350)
Control Mean	81.4	8.30	181.4	8.87
Observations	262	267	321	321

Note: Monthly Earnings has been transformed using the inverse hyperbolic sine function. All specifications include individual and month fixed effects. Standard errors are clustered at the station level. ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$. Standard errors are displayed in parentheses.

Table A8: Impact of Financial Interventions on Salary Discussions and Pay Fairness

	Salary Discussions with Managers		Salary Discussions with Workers		Fair Pay Scheme	
	(1)	(2)	(3)	(4)	(5)	(6)
BonusTreat	0.027	0.028	-0.102	-0.103	0.145	0.147
	(0.054)	(0.054)	(0.074)	(0.074)	(0.156)	(0.155)
SurpriseTreat	-0.030	-0.027	-0.103	-0.109	-0.158	-0.174
	(0.060)	(0.060)	(0.083)	(0.083)	(0.174)	(0.174)
Control Mean	0.089	0.089	0.28	0.28	1.88	1.88
Observations	183	183	183	183	189	189
p-value	0.37	0.39	0.99	0.95	0.10	0.084
Additional Controls		Y		Y		Y

Note: All specifications are based on the full dataset and include batch and team fixed effects. The p-value indicates whether BonusTreat and SurpriseTreat are statistically different from each other. Columns (2), (4), and (6) include controls for the other experiment(s). ***$p \leq 0.01$, **$p \leq 0.05$, *$p \leq 0.10$. Standard errors are displayed in parentheses.

Table A9: Summary Statistics for Position and Team Assignments of New Hires

	Bonus Treatment		
	Treatment	Control	p-value
Panel A: Position Characteristics			
Total Hours	104.99	105.38	0.93
	(33.24)	(30.88)	
Monthly Earnings	8.26	8.26	0.98
	(0.39)	(0.39)	
Evaluation Score	84.42	83.98	0.09
	(1.70)	(1.60)	
Performance Bonus	4.63	5.37	0.78
	(8.57)	(19.32)	
Panel B: Team Characteristics			
Total Hours	99.75	98.11	0.73
	(38.30)	(33.56)	
Monthly Earnings	8.23	8.23	0.88
	(0.37)	(0.36)	
Evaluation Score	84.94	84.74	0.55
	(2.36)	(2.12)	
Performance Bonus	10.52	14.76	0.31
	(21.45)	(29.37)	
Observations	80	173	

Note: We calculate the mean value of each variable associated with a new hire's initial position and team, using the pre-experiment administrative data of August 2023. Panel A shows the means by the position assignment of new hires, and Panel B shows the means by the team assignments of new hires. Monthly Earnings has been transformed using the inverse hyperbolic sine function. The standard deviation is shown below the mean in parentheses. The p-value is taken from a regression testing the statistcal difference between the treatment and control groups.

Table A10: Summary Statistics for Spillover Variables

	Average Share	Observations
Team BonusTreat Share	0.07	47
	(0.05)	
Team SurpriseTreat Share	0.04	47
	(0.04)	
Team Friends BonusTreat Share	0.04	50
	(0.16)	
Team Friends SurpriseTreat Share	0.11	50
	(0.28)	
Firm Friends BonusTreat Share	0.05	50
	(0.17)	
Firm Friends SurpriseTreat Share	0.09	50
	(0.24)	
Share of Neighboring Substations Treated	0.21	138
	(0.31)	

Note: In the first six rows, the sample is limited to individuals in the bonus and surprise control group. In the last row, the sample is limited to stations in the monitoring control and treatment groups.