Enhancing Decision-Making: The Impact of Information Provision on Worker Task Assignment

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November 5, 2026

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Abstract

Worker-task assignment is a core managerial decision, but managers may lack the information needed to make it effectively. We conduct a randomized field experiment in three garment manufacturing firms in China, covering 42 production teams and 1,130 workers, to study whether providing managers with systematic worker information improves task assignment and firm performance. In one treatment, managers receive reports on several dimensions of workers' efficiency across tasks; in a second treatment, they additionally receive information on workers' stated task preferences. Both treatments increase team efficiency by approximately 10 percent and reduce worker turnover by around 39 percent, with no detectable decline in product quality. Exploring the underlying mechanisms, we find that the intervention substantially improves managers' knowledge along the dimensions of information provided. However, managers use this information selectively: we detect changes in task assignment only along workers' relative efficiency across tasks, with no corresponding improvement in assignment based on other dimensions of worker performance or preferences. The results show that information provision can improve worker-task matching and firm performance, but that the difficulty of weighing multiple dimensions at once may limit how fully managers incorporate the information they learn into complex decisions.

Keywords: Task Allocation, Information Friction, Firm Performance

JEL: D22, O14, M54, J24

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I am deeply grateful to my advisor, Jing Cai, for her tremendous support and guidance throughout this project. I also thank Miriam Bruhn, Ginger Jin, Kenneth Leonard, M. R. Sharan, and Cory Smith for their valuable feedback and insightful comments. The project received approval from University of Maryland College Park IRB (2299046-1). Financial support from University of Maryland AREC and the Chiang Ching-Kuo Foundation is greatly appreciated. I used ChatGPT and Claude for copy-editing assistance. All errors are my own.

1 Introduction

Firm growth is an important driver of aggregate productivity and job creation, yet firms in developing countries grow much less over their life cycle than firms in richer economies (Hsieh and Klenow, 2014). A key determinant of firm performance is management. Differences in management quality are closely related to productivity differences across firms and countries (Bloom and Van Reenen, 2010), and experimental evidence shows that improving management can raise productivity and other firm outcomes (Bloom et al., 2013; Bruhn, Karlan and Schoar, 2018; Cai and Wang, 2022). One core managerial function is the allocation of workers across tasks. Recent evidence shows that worker-task assignment has substantial consequences for productivity and worker outcomes (Chen et al., 2025; Minni, 2026). Existing work has identified several sources of worker-task misallocation, including limited managerial attention (Adhvaryu, Kala and Nyshadham, 2022), weak team-level incentives (Burgess et al., 2010), and favoritism toward socially connected workers (Bandiera, Barankay and Rasul, 2009). A potentially important but less studied friction is information: managers may not know enough about their workers to make effective assignment decisions.

Effective task assignment requires managers to know how workers perform across different tasks. Acquiring and processing this information can be difficult when workers differ along multiple dimensions and managers must allocate many workers across different tasks. At the same time, firms increasingly collect detailed data that could be used to construct systematic measures of workers' task-specific performance. Turning such data into actionable information for managers, however, may require analytical and organizational capabilities (Brynjolfsson and McElheran, 2016). Productivity is also not necessarily the only relevant consideration: workers value non-wage aspects of their jobs (Mas and Pallais, 2017; Maestas et al., 2023) and may have preferences over the tasks they perform (Kwasnitschka et al., 2026). Despite the potential importance of these different dimensions of worker information, we know little about whether limited information is an important constraint on task assignment, whether providing managers with systematic information changes their decisions, and which dimensions

of worker information managers incorporate when making assignments.

To fill these gaps, we conduct a randomized field experiment in three garment manufacturing firms in China, covering 42 production teams and 1,130 workers. Garment production provides a useful setting for studying task assignment because assignment decisions are frequent, complex, and consequential. Each time a team begins producing a new garment style, its manager must reassign dozens of workers across many tasks under tight delivery deadlines, and workers' productivity varies substantially across tasks. Baseline evidence confirms that managers face substantial information gaps. 81 percent of managers in our sample report difficulty identifying workers' relative efficiency and skill specialization. In an objective knowledge test, managers correctly identify, on average, only 2.7 of the five workers with the highest measured efficiency and 0.8 of the five with the greatest measured efficiency variability. Managers also rarely elicit workers' task preferences directly: 57 percent report that preferences play no role in their assignment decisions, and among those who do consider them, only one-third report asking workers directly.

We designed two treatment arms in the experiment. In the first treatment, team managers receive efficiency reports constructed from the previous year's administrative production records for workers on their teams. The reports summarize several dimensions of worker efficiency, including relative efficiency across tasks, learning speed, efficiency variability, and relative performance in multi-worker versus solo tasks. In the second treatment, team managers receive the same efficiency information together with information on workers' task preferences, including preferences over machines, task complexity, and garment parts, elicited through the baseline worker survey. To facilitate managers' use of the information, the reports are introduced in group meetings and then discussed individually with each treated team manager. Teams are randomly assigned to either treatment arm or to a control group.

We combine detailed administrative data from the firms with our survey data to study a comprehensive set of team, worker, and manager outcomes and to examine the mechanisms through which information provision affects task assignment and

performance outcomes. The administrative data contain daily worker-task records of output and time use, task characteristics such as machine type and complexity, team-level efficiency and quality measures, and worker rosters that track turnover. These data allow us to measure worker efficiency across tasks and examine managers' actual worker-task assignments. The surveys measure managers' knowledge of workers' efficiency and preferences before and after the intervention, as well as worker job satisfaction and evaluations of team managers. Together, these data allow us to trace how information provision changes managers' knowledge, how those changes affect task-assignment decisions, and how these decisions translate into firm and worker outcomes.

We find that information provision substantially improves team performance and worker retention. In the efficiency information treatment, team efficiency—measured as realized output relative to the standard production benchmark—increases by 8.5 percentage points, or 10.7 percent relative to the control-group mean. Worker turnover within four months falls by 4.5 percentage points from a control-group mean of 9.7 percent, a reduction of approximately 46 percent. The efficiency and preference information treatment has similar effects: team efficiency increases by 8.1 percentage points (10.2 percent), and worker turnover falls by 3.3 percentage points (34 percent). The effects on both team efficiency and worker turnover are not statistically different across the two treatment groups. For both treatment arms, we find no detectable effect on product quality, suggesting that the efficiency gains do not come at the expense of quality.

We next examine the mechanisms underlying these effects. One month after the intervention, managers in both treatment arms know substantially more about workers' efficiency along the dimensions included in the reports, with gains of roughly 0.7–0.9 standard deviations. The knowledge gains are not statistically different across the two treatment groups. Managers in the efficiency and preference information treatment also learn substantially about worker preferences, with preference knowledge increasing by 1.34 standard deviations. These knowledge gains tend to be larger among managers with weaker pre-intervention management practices. These knowledge gains translate

into changes in task assignment. Using administrative data, we construct a measure of assignment quality that compares managers' realized assignments with the best feasible assignment given the workers and tasks available to the team. Both treatments reduce the shortfall from this benchmark along the worker-efficiency dimension by 13–14 percent, with larger improvements among managers who had less prior knowledge of workers' relative efficiency. We also find suggestive evidence of improvements in workers' job satisfaction, satisfaction with task assignment, and evaluations of team managers, consistent with the reduction in turnover. Taken together, these results show that the intervention changes both what managers know about their workers and how they assign them, providing a plausible mechanism for the gains in team efficiency and worker retention.

At the same time, managers selectively use the information they learn. Although the intervention improves managers' knowledge along every dimension provided, including workers' relative efficiency across tasks, learning speed, efficiency variability, relative performance in multi-worker versus solo tasks, and, when provided, worker preferences, we find that only information on worker efficiency across tasks affects assignment decisions. In particular, despite the large increase in managers' knowledge of worker preferences in the second arm, we find no detectable improvement in preference-based assignment. Follow-up qualitative interviews with managers help explain this pattern. Many managers report that incorporating worker preferences can make task assignment more difficult or conflict with team efficiency, suggesting that they focus on information most directly tied to meeting production targets. More broadly, our results suggest that information improves managerial decisions mainly along dimensions managers already prioritize, while the cost of incorporating additional dimensions limits how far richer information changes their decisions. As a result, once information becomes available, the binding constraint may shift from acquiring information to integrating multiple objectives into managerial decisions.

Our paper makes two contributions. First, we contribute to the literature on management practices and firm performance by studying worker-task allocation as a core

managerial practice. Existing research has established that management matters for firm performance (Bloom et al., 2013; Bruhn, Karlan and Schoar, 2018; Cai and Wang, 2022), but much less is known about how well managers allocate workers across tasks and what frictions limit their ability to do so. Most closely related, Minni (2026) uses detailed personnel data from a multinational firm to show that managers affect worker productivity and career progression through worker-job matching. Other studies highlight specific forces that shape worker allocation, including managerial attention, team incentives, and social connections (Adhvaryu, Kala and Nyshadham, 2022; Burgess et al., 2010; Bandiera, Barankay and Rasul, 2009). We provide, to our knowledge, the first experimental evidence that information frictions constrain worker-task allocation and that reducing these frictions can improve assignment quality and firm performance. Our design allows us to trace how information provision changes managers' knowledge, their task-assignment decisions, and ultimately team performance and worker retention.

Second, we contribute to research on information provision and managerial decision-making. Existing evidence shows that providing managers with better information can improve performance by strengthening employee incentives or changing managers' product-placement decisions (Manthei and Sliwka, 2019; Manthei, Sliwka and Vogelsang, 2023). More broadly, research on information processing shows that having access to relevant information does not necessarily imply that decision makers notice, learn from, or act on it (Hanna, Mullainathan and Schwartzstein, 2014; Handel and Schwartzstein, 2018). Existing studies primarily examine whether providing a particular type of information changes behavior and downstream outcomes. We instead study how managers use multiple dimensions of information when allocating workers across tasks under tight operational constraints. By separately measuring managers' knowledge and their subsequent assignment decisions, we distinguish information acquisition from information use. The preference treatment provides a particularly informative example: although workers value non-wage job attributes (Mas and Pallais, 2017; Maestas et al., 2023), managers learn substantially about workers' task preferences

but do not incorporate them into assignment decisions. Qualitative interviews further help explain why managers use some dimensions of information but not others.

The article proceeds as follows. Section 2 describes the institutional setting and the task-allocation problem in the garment manufacturing plants. Section 3 presents the experimental design and the information reports provided to managers. Section 4 describes the administrative and survey data. Section 5 presents the effects of the intervention on worker and firm outcomes and underlying mechanism. Section 6 shows the cost-effectiveness analysis of the intervention. Section 7 concludes.

2 Background

2.1 Study Context

We collaborate with three garment manufacturing plants in China to study how information provision can improve managers' task allocation decisions and firm outcomes. The garment industry offers a particularly suitable setting for this research agenda.

Garment manufacturing plants of the type we study are widespread across the developing world. The production processes and organizational structure we describe below are common to garment factories throughout China, Bangladesh, Vietnam, India, and other major garment exporting countries. The textile and garment sector employs around 94 million workers globally (International Labour Organization, 2023). This type of labor-intensive manufacturing absorbs a large share of the low-skilled workforce in developing countries and constitutes a key entry point into formal employment, particularly for women and rural-to-urban migrants (Bárcia de Mattos et al., 2022; International Labour Organization, 2023). Improvements in how these plants allocate and manage labor therefore have direct implications not only for firm productivity but also for the earnings and welfare of a large population of workers.

Moreover, a large literature documents that management practices in developing-country manufacturing firms often fall short of best practices (Bloom and Van Reenen,

2007, 2010; Bloom et al., 2013), and the plants in our sample share many of these features. Over 78% of team managers in our sample plants have education level of junior high school or below, and have served as managers for 4.2 years on average. Almost all of them were promoted from the ranks of experienced sewing workers, and plants don't provide any formal management training. As a result, management decisions—including the allocation of workers to tasks—are made largely on the basis of personal experience and intuition rather than systematic information or formal decision-making tools.

2.2 The Sample Plants

The three plants in our sample are highly similar in their organization and production processes. Each plant receives production orders from apparel retailers, purchases garment materials from upstream textile firms, and produces a variety of garment styles. Production within each plant is organized into three workshops: cutting, sewing, and finishing. Fabric is first cut into garment parts in the cutting workshop; these parts are then stitched into finished garments in the sewing workshop; finally, garments are inspected, ironed, and packaged in the finishing workshop.

We focus on the sewing workshop, which is the most labor-intensive stage of production and employs the majority of each plant's production workers. Sewing workers operate a variety of sewing machines (e.g., flat sewing machine, covering stitch machine, overlock machine, chain stitch machine and so on) and perform a wide range of tasks that differ in required techniques and difficulty. Workers are organized into sewing teams—42 teams across the three plants in our sample—with each team comprising around 30 workers on average. Each team functions as an independent production unit: it produces a garment product, such as a T-shirt or a jacket, by sequentially stitching cut garment parts into a finished product, with each worker on the team responsible for one or several tasks along the assembly sequence. Sewing workers' monthly earnings are determined by task-specific piece rates and output volume. Team managers' monthly compensation consists of a fixed salary plus a bonus tied to team

performance—specifically, team-level efficiency and quality.[1]

2.3 Style Changeovers and the Task Allocation Problem

A central feature of production in these plants is the frequent introduction of new garment styles. Because different styles require distinct sets of tasks and machinery, each style changeover requires the team manager to reassign workers to tasks. This reassignment problem is nontrivial: the manager must match roughly 20-40 workers to a new configuration of tasks under tight delivery deadlines, with limited information about how productive each worker would be at each of the new tasks. Team managers in our sample expressed considerable difficulty in determining the optimal match between workers and tasks during changeovers. Around 81% of team managers in our sample reported in the baseline survey that identifying workers' expertise and skills is challenging.

Our discussions with managers and workers in these plants indicate that, as in manufacturing firms in other industries and countries, the style changeover period is critical and vulnerable, and significantly impacts profitability. Output drops sharply at the beginning of each changeover and recovers gradually as workers learn their newly assigned tasks. Any inefficiency in task allocation during this period can therefore lead to delayed deliveries, increased costs, and forgone profits.

The frequent style changes at these garment manufacturers, combined with the information frictions managers face, provide an ideal context to study the impact of information provision on managers' task allocation decisions and its downstream effects on team productivity and worker welfare.

3 The Experimental Design

To examine whether providing managers with information on worker efficiency and task preferences improves managers' knowledge of their workers, changes task

[1]Managers' bonuses reflect team-level efficiency and quality, though the exact formula varies across plants: at one plant, the bonus depends on team efficiency, quality, and the number of style changeovers; at a second, it is based on the team's monthly output value; and at a third, it depends on team efficiency and quality alone.

allocation decisions, and ultimately affects team efficiency, job satisfaction, and worker retention, we conducted a randomized field experiment in three garment plants in China. Our sample comprises 42 sewing teams and 1,256 sewing workers. Teams were randomly assigned to one of three experimental groups: teams in the first treatment arm received reports on worker efficiency; teams in the second treatment arm received reports on both worker efficiency and workers' task preferences; and teams in the control group received no reports.

3.1 The Efficiency and Preference Reports

Prior to the intervention, we held meetings with plant directors, workshop managers, team managers, and industrial engineers at each plant to identify the metrics that managers considered most useful for allocating tasks and that the plants wished us to compile.

We constructed the efficiency reports using administrative production records covering the year preceding the intervention.[2] From these data, we computed measures of worker efficiency along four dimensions: (i) each worker's within-team efficiency ranking by machine type and task complexity level; (ii) each worker's within-team ranking in the rate of learning during the first five days of a new production cycle, when teams begin producing a new garment style; (iii) each worker's within-team ranking in the variability of efficiency by task complexity; and (iv) each worker's comparative advantage in multi-worker tasks relative to solo tasks.[3]

We constructed the preference reports using responses to the baseline worker survey, in which each worker was asked to select one to three preferred machines, one or two preferred task complexity levels, and one to three preferred garment parts, and to explain the reasons for these choices. Notably, very few managers in these plants had ever asked workers about their task preferences. Workers, in turn, were unwilling to com-

[2]Output data are automatically logged by machines each time a worker completes one piece of a task. The data include daily output by worker and task, the total seconds a worker takes to complete each task, a description of the task, the standard allowable minutes (SAM) of the task, the machine required to perform the task, and the complexity level of the task.

[3]All four dimensions were recommended by team managers and industrial engineers during our pre-intervention meetings.

municate their preferences to managers absent some form of external facilitation. Many workers reported that they refrained from raising their preferences with their managers because they worries that doing so would be seen as resisting task assignments, or that it would impose an additional burden on management.

Reports were shared only with treated teams. The delivery of reports proceeded in two stages. First, each plant organized a formal introductory meeting for treated team managers. At this meeting, a senior plant manager introduced the report-sharing program, explained why the reports could be useful for task allocation, and provided concrete examples of how managers might use them. The research team then presented the content of the reports with illustrative slides, explained how each metric was computed, and described how the metrics could inform task allocation. Second, over the following weeks, the research team met individually with each treated team manager to walk through that team's report, explain how to interpret and apply each metric, and answer questions. Each individual session lasted approximately 40 minutes.

One month after the reports were delivered, we followed up with treated team managers regarding their use of the reports. To measure whether the reports updated managers' information about their workers, we also administered an in-person knowledge test to both treated and control team managers, in which managers answered questions about their workers' efficiency and task preferences without consulting the reports.

3.2 Randomization

We randomized treatment at the team level, stratifying by team size and past team performance. Team size is measured as the number of permanent workers on the pre-intervention roster. Past performance is measured using the team manager's average monthly performance bonus over the eight months preceding the intervention. For each stratification variable, we created an indicator equal to one if the team was above the within-plant sample median. Crossing the two indicators yields four strata within each plant. Within each stratum, teams were randomly assigned to one of the three

experimental groups, resulting in 13 teams (358 workers) in treatment arm 1, which received only efficiency reports; 15 teams (370 workers) in treatment arm 2, which received both efficiency and preference reports; and 14 teams (402 workers) in the control group. Figure 1 illustrates this randomization explicitly.

[Insert Figure 1 Here]

3.3 Timing

Because preparing the reports required substantial data processing, the intervention was rolled out sequentially across the three plants. The first wave began in October 2025 in the first plant, with five teams assigned to the efficiency arm and four teams assigned to the efficiency-and-preference arm. The second wave began in February 2026 in the second plant, with two teams in the efficiency arm and three teams in the efficiency-and-preference arm. The third wave began in April 2026 in the third plant, with six teams in the efficiency arm and seven teams in the efficiency-and-preference arm.

Figure 2 shows the timeline for each individual plant. In Phase 1, we administered a baseline survey, held discussions with plant managers, and compiled reports for all teams simultaneously. Phase 2 constituted the intervention itself, beginning with an introductory meeting and followed by individual meetings with participants, as described above. One month after the intervention, Phase 3 involved manager follow-up together with a knowledge test to assess managers' understanding of the material covered. Finally, two months after Phase 3, Phase 4 consisted of a follow-up survey to evaluate the intervention's effects over time.

[Insert Figure 2 Here]

4 Data

Our analysis combines administrative data from the three plants with survey data collected from workers and team managers at baseline and follow-up between September 2025 and July 2026.

4.1 Administrative Data

We obtain three main sources of administrative data from the three plants. First, we receive daily worker-level production data. The production lines automatically record each worker's cumulative output and time spent on each assigned task in real time. The data also contain information on task descriptions and the production line on which each worker performs the task. In addition, the industrial engineering departments provide information on task characteristics, including the required machine, complexity level,[4] and standard allowable time (SAT) in seconds.[5] We merge the daily production and task-characteristics data to calculate worker-level daily efficiency as $\frac{SAT \times \text{daily output}}{\text{time spent on the task}}$.

Second, we obtain monthly team-level performance data. Specifically, the plants provide monthly team efficiency and qualification rates calculated by their HR departments. The three plants use somewhat different measures of monthly efficiency. In two plants, covering 22 teams, monthly efficiency is calculated as $\frac{\sum_o N_o \times SAT_o}{\#\text{workers} \times \text{work hours per day} \times \text{work days per month}}$, where N_o and SAT_o denote the monthly output of task o and its standard allowable time, respectively. In the third plant, covering 20 teams, monthly efficiency is calculated as realized efficiency divided by target efficiency. Qualification rates are measured consistently across the three plants. Each garment is inspected before packaging, and the qualification rate is the share of inspected products that pass the quality check.

Third, we obtain worker roster data collected once before the intervention and again

[4]The plants classify tasks into five complexity levels, from A to E, with A denoting the most complex tasks and E the simplest.

[5]SATs are drawn from the General Sewing Data (GSD), a widely used database in the garment manufacturing industry. Industrial engineers decompose each task into a sequence of basic movements, such as grabbing and aligning the garment, sewing a specified length, and turning the garment. The GSD assigns a standard time to each basic movement when performed by a qualified worker using the standard machine. Worker piece rates are typically determined based on the SAT and complexity level of the task.

four months after the intervention. The rosters record current sewing workers, their team and position, as well as demographic characteristics including gender, age, and education, and their year of joining the company. We also obtain worker turnover records, including the date on which workers left the company, for the 8 months preceding the intervention and 4 months post intervention.

Table 1, Panel A reports summary statistics for variables constructed from the administrative data. The first two sets of variables describe demographic characteristics of managers and workers in the sewing departments. Managers and workers are both in their early 40s on average. Approximately 60% of team managers and 90% of workers are female. Managers have approximately four years of tenure at the plant on average, compared with three years for workers. Both managers and workers have approximately nine years of schooling on average, corresponding roughly to a junior-high-school education.

The sewing teams in our sample have approximately 30 workers on average. The average worker turnover rate during the eight months preceding the intervention is 15.2%. Average team efficiency and qualification rates over the six months preceding the intervention are 86.6% and 86.3%, respectively. Managers receive approximately 922 RMB performance bonus per month on average during the six months preceding the intervention. We find no statistically significant differences across the three treatment arms in these administrative characteristics.

4.2 Survey Data

We collect baseline surveys from workers and team managers prior to the intervention, sequentially across the three plants in August 2025, December 2025, and February 2026. We conduct follow-up surveys approximately three months after the intervention, in January 2026, May 2026, and July 2026.

Table 1, Panel B reports summary statistics for worker job satisfaction and evaluations of team managers. The worker survey includes questions on several dimensions of job satisfaction, measured on a five-point scale, with some questions phrased positively

and others negatively. Several questions directly concern task allocation, including whether workers are satisfied with the tasks assigned to them during the preceding three months. Other questions capture workers' experiences with their team managers, including whether managers care about workers' feelings, treat workers fairly, whether workers would like to transfer to another team, and whether workers are frequently criticized by their managers. The survey also includes broader measures of satisfaction with the company, such as satisfaction with income during the preceding three months and whether workers have considered resigning from the company. Appendix Table A1.1 provides the complete list of questions. The average worker satisfaction score is approximately 3.3 out of 5.

We also ask workers to evaluate their team managers along nine dimensions: organizing production, handling emergencies and conflicts, treating workers fairly, listening to workers' opinions and suggestions, caring about workers' work and personal lives, clearly communicating task requirements, providing ongoing guidance, encouraging workers during challenges, and being trustworthy. Each item is measured on a five-point scale. Appendix Table A1.2 provides the complete list of evaluation questions. The average manager evaluation score is approximately 3.7 out of 5.

[Insert Table 1 Here]

4.3 Survey Evidence on Managers' Knowledge

Baseline survey evidence indicates that team managers face substantial information and management constraints in allocating workers across tasks. When asked about the management problems they had encountered during the preceding three months, 78.6 percent of team managers reported challenges directly related to task allocation, including missed production deadlines, quality shortfalls, and assigning workers to tasks for which they were insufficiently qualified. Consistent with these difficulties, 81 percent of team managers reported that identifying workers' relative efficiency and skill specialization was challenging.

These self-reported difficulties are corroborated by an objective measure of managers' knowledge about worker productivity, collected prior to the intervention. We asked each team manager to identify the five workers on their team whom they believed to be the most efficient on the flatbed sewing machine—the most widely used machine on the factory floor—and, separately, the five workers whom they believed to exhibit the greatest volatility in efficiency. We then compared these nominations with workers' actual daily output records over the preceding year, which allow us to construct objective rankings of workers by mean efficiency and efficiency volatility. Table 2 reports managers' scores, defined as the number of correct matches, out of five, between their nominations and the corresponding objective rankings. On average, team managers correctly identify only 2.7 of the five most efficient workers and 0.8 of the five workers with the greatest efficiency volatility. The substantial gap between managers' beliefs and workers' realized performance points to a meaningful information friction in managers' knowledge of worker efficiency.

Complementary evidence suggests that team managers rarely incorporate worker preferences into task-assignment decisions. In a multiple-response question regarding the criteria used in task assignment, 97.6 percent of team managers reported considering productivity-related factors, including skill, learning ability, and work attitude. By contrast, only 42 percent reported considering worker preferences, while the other 57 percent stated that preferences played no role in their assignment decisions. Moreover, even among managers who reported taking worker preferences into account, information is typically acquired indirectly rather than through direct communication: 66.7 percent reported inferring preferences from their observations of workers' behavior on the production line, whereas only 33 percent reported eliciting preferences directly from workers.

This limited consideration of worker preferences coincides with substantial challenges in workforce management. Over the same three-month period, 78.6 percent of team managers reported difficulties related to worker turnover, low motivation, or low job satisfaction.

[Insert Table 2 Here]

5 Main Results

5.1 Estimation Strategy

Teams were randomly assigned within randomization strata to one of three experimental arms: a control group, an efficiency-information treatment (T1), or an efficiency-and-preference-information treatment (T2). For each outcome, we estimate an ANCOVA specification of the form

$$Y_{it} = \alpha + \beta_1 T1_i + \beta_2 T2_i + \rho, \overline{Y} * i^{\text{pre}} + \delta * s + \gamma_t + \varepsilon_{it}, \tag{1}$$

where i indexes the unit of observation and t indexes the post-intervention period. For outcomes measured repeatedly after the intervention—weekly team efficiency, monthly team quality, and daily task-assignment match quality—i indexes teams and t indexes the corresponding calendar period. In these specifications, $\overline{Y}_i^{\text{pre}}$ denotes team i's mean value of the same outcome over the pre-intervention periods, the term δ_s denotes randomization batch $\times$ stratum fixed effects, which account for the stratified randomization design, and γ_t denotes plant $\times$ calendar-time fixed effects, which absorb common shocks in outcomes across plants and over time.

For outcomes measured once after the intervention—worker turnover, satisfaction, team-level task assignment quality, and the knowledge test administered to team managers—t indexes the single post-intervention observation, and the period fixed effect γ_t is omitted. In these specifications, $\overline{Y}_i^{\text{pre}}$ is replaced by Y_i^{base}, the corresponding individual-level baseline measure.

The coefficients β_1 and β_2 capture the effects of the T1 and T2 treatments, respectively, relative to the control group, conditional on the corresponding baseline outcome. We additionally test the equality $\beta_1 = \beta_2$ to assess whether providing information about worker preferences in T2 generates effects beyond those of the efficiency information

provided in both treatment arms. Standard errors are clustered at the team level, corresponding to the unit of randomization.

5.2 Results on Efficiency, Quality, and Turnover

We first examine the effects of the information intervention on team-level performance. Table 3 reports estimates from the ANCOVA specifications described above. We find that providing managers with information about worker efficiency increases team efficiency. In Column (1), the efficiency information treatment increases raw weekly team efficiency by 9.8 percentage points, relative to a control mean of 58.9%, corresponding to an increase of approximately 17 percent. Providing both efficiency and preference information increases raw efficiency by 6.2 percentage points, although the estimate is somewhat less precisely estimated.

Raw efficiency may partly reflect differences in the complexity of the products assigned to teams. In particular, efficiency is mechanically more difficult to achieve for teams producing more complex styles, for which workers are required to perform more tasks simultaneously and face higher performance standards. To account for these differences, Column (2) reports results using efficiency adjusted for the complexity of the styles performed by each team. Complexity-adjusted efficiency nets out task complexity, measured as the number of distinct operations in a team's style. To avoid potential treatment effects, we estimate the coefficients on the number of operations using pre-intervention team-days only and then subtract this component from raw efficiency for each post-intervention team-day.[6] We provide details of the calculation of complexity-adjusted team efficiency in Appendix A.2. The efficiency information treatment increases complexity-adjusted efficiency by 8.5 percentage points, corresponding to a 10.7% increase relative to the control mean. The treatment providing both efficiency and preference information increases complexity-adjusted efficiency by 8.1 percentage points, or 10.2% relative to the control mean. The estimated effects of the two treatments are therefore very similar in magnitude. We cannot reject equality of the Treatment 1

[6]Appendix Table A1.4 shows that 1 additional task is associated with around 0.2% decrease in team efficiency, after controlling for learning speed.

and Treatment 2 effects for either measure of efficiency, with p-values of 0.373 and 0.906, respectively.

In contrast, we find no evidence that the intervention affects team quality. The estimated effects on monthly team quality are small and statistically insignificant for both Treatment 1 and Treatment 2. The point estimates are -0.007 and 0.001, respectively, relative to a control mean of 0.835, and we cannot reject equality of the two treatment effects. This result is consistent with the design of the intervention, which did not provide managers with information about worker performance on quality. Taken together, these results indicate that providing managers with information about worker efficiency improves team efficiency without detectable effects on output quality.

[Insert Table 3 Here]

We next examine whether the information intervention affects worker turnover. Table 4 reports estimates from the ANCOVA specification on turnover effect. The sample includes workers who were still employed at the time of intervention, and the outcome is an indicator for whether a worker leaves the firm within four months following the intervention. We find that interventions significantly reduces worker turnover. Providing managers with worker efficiency information reduces the probability of leaving the firm by 4.4 percentage points, relative to a control mean of 9.5%, corresponding to a reduction of approximately 46 percent. Providing both efficiency and preference information produces a similar effect, reducing turnover by 3.1 percentage points, or approximately 33 percent relative to the control mean. Both estimates are statistically significant at conventional levels, and we cannot reject their equality, with a p-value of 0.345. Thus, we still find no evidence that providing additional preference information generates an incremental reduction in turnover beyond the efficiency information provided in both treatment arms.

[Insert Table 4 Here]

5.3 Results on Knowledge Improvement

The improvement in team efficiency and worker retention suggest that the intervention may have changed how managers make task-assignment decisions. We therefore turn to managers' knowledge of workers' skills and preferences to examine whether the intervention improved managers' ability to identify workers' skills and preferences, thereby enabling them to make more informed and effective task-assignment decisions.

We first examine whether the information intervention improved team managers' knowledge of workers' skills and preferences. We conducted an in-person knowledge test with all team managers one month after the intervention. The test consisted fo 12 questions, with managers asked to identify workers who matched specific characteristics. For each question, managers were asked to provide three names. The first eight questions assessed knowledge of worker efficiency, including efficiency and learning speed across different levels of task complexity, as well as efficiency variability and performance on teamwork versus solo tasks. The remaining four questions assessed knowledge of worker preferences over task assignments. Appendix Table A1.6 provides a detailed list of these questions. The ground truth for the efficiency questions was constructed from administrative production records, while the ground truth for the preference questions was based on workers' self-reported preferences from the worker survey.

To construct the post-intervention knowledge measures, a manager receives one point for each correctly identified worker among the top three workers in the corresponding team-by-task cell. We then standardize each question-level score using the control group mean and standard deviation, and construct each outcome as the mean of the standardized scores across its component questions.[7] Thus, the coefficients in Table 5 are expressed in control-group standard deviation units, with a control group mean

[7]We follow the standardization approach used in Bloom and Van Reenen (2007). Although each question is scored on the same 1-3 scale, the distribution of responses may differ across questions because some management practices may be more difficult to assess than others. Standardizing each practice to have mean zero and standard deviation one before averaging ensures that each question receives equal weight in the composite index, rather than allowing questions with greater variation in their raw responses to disproportionately influence the index.

of zero by construction. The dependent variable in Table 5, Column (1) is the average z-score across the first six questions on worker efficiency and learning speed (Q1–Q6 in Appendix Table A1.6); Column (2) is the average z-score across the two questions on efficiency variability and teamwork (Q7–Q8); and Column (3) is the average z-score across all eight questions related to worker efficiency (Q1–Q8). Column (4) measures knowledge of worker task preferences and is the average z-score across Questions 9–12, while Column (5) is the average z-score across all questions in the knowledge test.

Table 5, Columns (1)–(3) show that the intervention substantially improved managers' knowledge of worker efficiency. Efficiency information treatment increases the knowledge score for efficiency and learning speed by 0.72 standard deviations, while both efficiency and preference information treatment increases it by 0.81 standard deviations. For the two questions on efficiency variability and teamwork, the both efficiency and preference information treatment is 1.03 standard deviations, while the efficiency information treatment effect is 0.72 standard deviations and is less precisely estimated. The effects on the broader measure combining all eight efficiency-related questions are similarly large, at 0.72 and 0.86 standard deviations for efficiency information treatment and efficiency + preference information treatment, respectively. We cannot reject equality of the two treatment effects for any of these efficiency-related measures, with p-values ranging from 0.554 to 0.787. Thus, both information treatments substantially improved managers' knowledge of worker efficiency, with no statistically detectable difference between the two treatments.

Columns (4) and (5) reveal a different pattern for knowledge of worker preferences and overall knowledge. The efficiency information treatment has a small and statistically insignificant effect on managers' knowledge of worker preferences, increasing the score by only 0.18 standard deviations. In contrast, the efficiency and preference information treatment increases preference knowledge by 1.34 standard deviations, and we strongly reject equality of the two treatment effects ($p = 0.007$). For overall knowledge, the efficiency information treatment increases the score by 0.54 standard deviations, compared with a 1.02-standard-deviation increase under the efficiency and

preference information treatment. The difference between the two treatment effects is statistically significant at the 10% level (p = 0.058), suggesting that the additional preference information provided under the efficiency and preference information treatment arm generated larger overall gains in managers' knowledge.[8]

[Insert Table 5 Here]

We further examine whether the improvement in managers' knowledge varies with their baseline management practices. We measure baseline management practices using 12 survey questions, which are reported in Appendix Table A1.3. We construct a management-practice score from managers' responses and standardize it using the mean and standard deviation of the control group, with higher values indicating better baseline management practices. Table 6 reports heterogeneity in knowledge gains by baseline management practices. Given the relatively small number of teams, we pool the two treatment arms and interact the treatment indicator with an indicator for above-median baseline management practices. The treatment effects are positive across all five knowledge measures, ranging from 0.91 to 1.06 standard deviations for managers with below-median baseline management practices. The interaction terms are negative across all five outcomes, ranging from -0.48 to -0.95, although only the interaction for overall knowledge is statistically significant at the 10% level. This pattern suggests that the intervention may generate larger knowledge gains among managers with weaker baseline management practices.

[Insert Table 6 Here]

[8]Figure 3 compares the distribution of post-intervention knowledge test scores across treatment arms. Control managers cluster tightly around zero, consistent with the standardized index being centered on the control group. Both treatment arms shift the distribution upward. The efficiency information treatment arm has a mean of around +0.6 SD with a fairly wide spread, ranging from about -0.4 to +2.1. The efficiency and preference information treatment arm has a slightly higher mean, near +1 SD, ranging from +0.2 to +1.7, reflecting the additional knowledge gains from preference information.

5.4 Results on Task Assignment

Having established that the intervention improved managers' knowledge of workers, we next examine whether these knowledge gains translate into changes in task-allocation decisions. We construct a match score to measure the quality of managers' task assignments. Specifically, for each team-day, workers are assigned to task-category cells, where the relevant cell is defined by machine type, complexity level, garment part, or a machine-complexity combination, depending on the outcome family. Each worker-task match is assigned a percentile rank based on the worker's performance relative to other workers who have performed the same task, with higher percentiles indicating better performance. For each cell, we define the realized match score as the average performance percentile of the workers assigned to that cell. A higher value therefore indicates a better match between workers and tasks.

The realized match score, however, reflects not only the quality of the manager's assignment decision but also the number of positions that must be filled and the pool of workers available to perform the task. Both factors may constrain the quality of feasible assignments independently of managerial decision-making. For example, a team may achieve a high realized match score because relatively few positions need to be filled and many qualified workers are available, even if the manager's assignment decision is no better than usual. Conversely, a team may achieve a low score when demand is high relative to the available pool of qualified workers, forcing the manager to assign less-efficient workers to the task. To account for these constraints and reduce noise in the outcome measure, we benchmark each team-day's realized assignment against the best assignment feasible given that day's task demand and the team's available pool of qualified workers. Specifically, for each cell, we calculate the highest average performance percentile that could be attained given the number of positions to be filled and the team's roster of qualified workers. We then define the cell gap as the difference between the realized match score and this best-feasible benchmark. The gap is non-positive by construction and measures the shortfall of the realized assignment from the best assignment feasible given the day's demand and worker pool. We aggregate

these cell-level gaps to the team-day level using worker-hours as weights, yielding a demand-adjusted match score. This measure isolates the component of assignment quality attributable to managers' allocation decisions, conditional on the constraints they face. We describe the detailed construction of the demand-adjusted match score in Appendix A.3.

Table 7 reports treatment effects on the team-day demand-adjusted match score for each of the seven outcome families. Both treatment arms improve task assignment along the efficiency dimension. The efficiency information treatment increases the efficiency match score by 0.041, while the efficiency and preference information treatment increases it by 0.039; both effects are statistically significant at the 5 percent level. Relative to the control-group mean of -0.298, these estimates correspond to reductions in the average shortfall from the best-feasible benchmark of approximately 13–14 percent. We cannot reject equality of the two treatment effects ($p = 0.94$). Effects on the remaining outcome families are smaller and generally not statistically distinguishable from zero.

We find no significant improvement in assignment quality along the preference dimension even when managers are explicitly provided with information on workers' preferences. The treatment providing both efficiency and preference information raises match scores for machine, complexity-level, and garment-part preferences by 0.044, 0.009, and 0.035, respectively, but none of these effects is statistically significant at conventional levels. The limited use of preference information is consistent with managers' stated priorities and their perceptions of the costs of incorporating worker preferences into task assignment. At baseline, roughly one-quarter of workers reported that their team manager never considered their preferences when assigning tasks, and a similar share were dissatisfied with the tasks assigned to them over the previous three months. At the same time, when asked which factors they consider when assigning tasks, all managers reported considering efficiency-related factors, whereas only 42 percent reported considering worker preferences. Among the 57 percent who reported not considering preferences, approximately 83 percent cited concerns that doing so could reduce team efficiency or make task assignment more difficult. These responses suggest that, when

provided with both efficiency and preference information, managers may prioritize the information they view as most important for meeting production objectives. Although incorporating worker preferences could potentially improve workers' satisfaction with their assignments, doing so alongside efficiency considerations may make an already demanding assignment problem more complex. The absence of a detectable effect on preference-based assignment is consistent with managers prioritizing efficiency when faced with multiple dimensions of worker information.

[Insert Table 7 Here]

We further examine heterogeneity in the improvement in task assignment quality along the efficiency dimension. Given the relatively small number of teams, we pool the two treatment arms and interact the treatment indicator with an indicator for below-median baseline knowledge of workers' relative efficiency across tasks. Table 8 reports the results. The effect of the intervention is concentrated among managers with below-median baseline knowledge. For managers with baseline knowledge at or above the median, the estimated treatment effect is small and statistically insignificant (0.015). By contrast, coefficient of the interaction term is 0.067 and statistically significant at the 5% level, indicating a larger treatment effect among managers with less prior knowledge of worker efficiency. This pattern is consistent with information provision generating greater improvements in task assignment among managers who were initially less informed about workers' relative efficiency.

[Insert Table 8 Here]

Taken together, these results suggest that managers selectively incorporate the information provided by the intervention into their task-allocation decisions. Treated managers use information on workers' relative efficiency when assigning workers to tasks, resulting in significant improvements in assignment quality along the efficiency dimension, and these improvements are larger among managers with less prior knowl-

edge of workers' relative efficiency. At the same time, despite substantial improvements in managers' knowledge of other worker characteristics—including learning speed across task types, performance variability across complexity levels, relative performance on multi-worker versus solo tasks, and preferences over different tasks—we find no corresponding improvements in task assignment along these dimensions. This pattern together with evidence from survey data suggests that managers prioritize information they view as particularly important for task assignment, rather than incorporating all available dimensions of worker information. Managers must assign 20–40 or more workers across 30–100 or more tasks within a short period of time, incorporating multiple dimensions of worker characteristics may make an already complex assignment problem more difficult. More broadly, the results highlight that improving managers' information does not necessarily translate into broader changes in managerial decisions: managers may selectively apply newly acquired information based on the importance they attach to different dimensions of the assignment problem.

5.5 Results on Worker Satisfaction and Happiness

The evidence above shows that the intervention changed managers' task-allocation decisions, particularly by helping them make greater use of information about worker efficiency. We next examine whether these changes in managerial decisions translate into workers' experiences on the job. Using the follow-up worker survey, we study whether the intervention affects overall job satisfaction, satisfaction with task allocation, and workers' evaluations of their team managers.

Table 9 reports the effects of the intervention on three worker-level outcomes constructed from the follow-up survey. Job satisfaction is an index that averages standardized responses across 14 items covering workers' satisfaction with different aspects of their jobs, including workers' satisfaction towards task assignment, their team managers, and the company, as we described in Section 4. Task-allocation satisfaction is the individual item measuring workers' satisfaction with how tasks are assigned. Team-manager evaluation is an index averaging workers' evaluations of their manager along nine dimensions (as we described in Section 4): organizing production, managing

conflicts, fairness, listening to workers' voice, caring about workers, communicating clearly, providing guidance, maintaining morale, and being trustworthy. For each outcome, follow-up responses are standardized using the control-group mean and standard deviation at follow-up.

The results in Table 9 show little evidence that the intervention affected workers' overall job satisfaction or satisfaction with task allocation. The efficiency information treatment increases the job satisfaction index by 0.003 standard deviations and task-allocation satisfaction by 0.027 standard deviations; neither estimate is statistically significant. The corresponding effects of the efficiency and preference information treatment are 0.010 and 0.077 standard deviations, respectively, and are also statistically insignificant. We cannot reject equality of the two treatment effects for either outcome, with p-values of 0.897 and 0.537. In contrast, the efficiency and preference information treatment improves workers' overall evaluation of their team manager by 0.136 standard deviations, marginally significant at the 10 percent level, while the efficiency information treatment has a small and statistically insignificant effect of 0.007 standard deviations. We still cannot reject equality of the two treatment effects at conventional levels ($p = 0.130$). Thus, while the intervention generated measurable changes in managers' task-allocation behavior, these changes do not translate into detectable improvements in workers' overall job satisfaction or satisfaction with task allocation. There is, however, some evidence that providing both efficiency and preference information improves workers' perceptions of their managers.

[Insert Table 9 Here]

Appendix Table A1.7 decomposes the satisfaction and manager-evaluation indices into their underlying survey items. The individual job-satisfaction items show no systematic pattern of treatment effects. The efficiency and preference information treatment increases reported satisfaction with fairness by 0.151 standard deviations. The remaining job-satisfaction items do not show statistically significant differences from

zero for either treatment arm. Given that these estimates represent a decomposition of the overall satisfaction index across multiple items, we do not interpret the isolated significant coefficients as evidence of broad changes in workers' job satisfaction.

The individual manager-evaluation items provide somewhat more consistent evidence for the positive effect of the efficiency and preference information treatment. Workers in this treatment arm report higher evaluations of their managers on listening to workers' voices, caring about workers, communicating clearly, and trustworthiness, with effects ranging from 0.140 to 0.182 standard deviations and significant at the 10 percent level. The effects on organizing production, conflict management, fairness, guidance, and morale are positive but not statistically significant. The efficiency information treatment produces small and statistically insignificant effects across all nine manager-evaluation dimensions. Overall, the item-level results are consistent with the composite index: the main detectable improvement is in workers' evaluations of managers under the efficiency and preference information treatment, rather than in workers' overall job satisfaction or satisfaction with task allocation.

5.5.1 Attrition and Sensitivity Analysis

Although we find no statistically significant treatment effects on worker satisfaction or team-manager evaluations, differential attrition across treatment arms raises a potential concern for these outcomes. Appendix Table A1.8 shows that the follow-up rates are 72.6 percent and 75.6 percent in the two treatment arms respectively, compared with 68.8 percent in the control group. Panel B shows that differences in turnover account for a substantial share of this gap. Overall, 12.5 percent of baseline respondents left the firm before the follow-up survey, compared with 16.6 percent in the control group and approximately 10.3 percent in each treatment arm. By contrast, the shares of workers who remained on the roster but did not respond to the follow-up survey are similar across treatment arms, as are the shares whose employment status cannot be determined because their personal information could not be matched to administrative records.

We formally examine the sources of differential follow-up in Appendix Table A1.9.

Both treatment arms are more likely to be re-surveyed than the control group, although the difference is statistically significant only for the efficiency and preference information treatment. The treatment effects on the probability of leaving the firm in the baseline respondents sample are -0.0649 and -0.0722 for the two treatment arms, respectively, both statistically significant. In contrast, we find no statistically significant treatment effects on non-response among workers who remained employed or on unmatched workers. These results indicate that the higher follow-up rates in the treatment arms are largely attributable to the intervention's reduction in worker turnover, rather than differences in survey response among workers who remained employed. Consequently, the composition of workers observed at follow-up may differ across treatment arms: the intervention may retain workers who would otherwise have left the firm.

This differential retention could affect our estimates of worker satisfaction if workers who would otherwise leave differ systematically from those who remain. To assess this possibility, Appendix Table A1.10 compares baseline satisfaction and team-manager evaluations between workers who remained with the firm and those who left before the follow-up survey. Leavers report significantly lower satisfaction with promotion opportunities, higher quit intentions, lower wage satisfaction, and a lower willingness to remain with the firm in the long term. They also have lower point estimates for task-allocation satisfaction and the overall job satisfaction index, although these differences are not statistically significant. These patterns suggest that workers retained by the intervention may be negatively selected in terms of baseline satisfaction, which could attenuate estimated treatment effects on satisfaction at follow-up. The direction and magnitude of this selection, however, cannot be established conclusively from these comparisons alone.

We therefore conduct a Lee (2009) bounds analysis to assess the sensitivity of the satisfaction results to differential attrition. Appendix Table A1.11 reports bounds separately for each treatment arm relative to the control group, trimming the treated outcome distribution by the difference in follow-up rates under the monotonicity assumption that treatment affects selection in only one direction. Although the resulting bounds in-

clude zero for job satisfaction in both treatment arms and for task-allocation satisfaction and team-manager evaluations in their lower bounds, the upper bounds permit moderately positive effects on task-allocation satisfaction and team-manager evaluations. Thus, while differential attrition introduces uncertainty into the interpretation of the null estimates, the bounds do not rule out modest positive effects on some worker job satisfaction and team-manager evaluation outcomes.

6 Cost-Effectiveness Analysis

We assess the cost-effectiveness of the intervention by translating its estimated effects on team efficiency and worker turnover into monetary benefits and comparing these benefits with the costs of implementing the intervention. We measure the benefits as the increase in annual operating profit generated through two channels: (i) higher team efficiency, which increases production output, and (ii) lower worker turnover, which reduces replacement costs. We then compare these benefits with the annual costs a plant would incur if it implemented the intervention independently. Gains in workers' piece-rate earnings are reported separately in Appendix A.4.2 and are not included in the plant-level benefits.

6.1 Productivity Channel

We first quantify the monetary benefits from improvements in team efficiency. Team efficiency is defined as the ratio of standard minutes produced to actual minutes worked, where standard minutes are calculated using the standard allowed minutes (SAM) for each operation (Appendix A.4.1). This measure allows us to express production output in garment equivalents, accounting for differences in the time required to complete different operations.

Consider a team of N workers, each working H hours annually, with average efficiency $\bar{e}$. Let s_g denote the standard minutes required to produce one garment. Annual output, measured in garment equivalents, is given by

$$Q = \frac{60\,\bar{e}\,H\,N}{s_g}. \tag{2}$$

Holding working hours, team size, and the composition of garment styles constant, output increases proportionally with efficiency. Therefore, an estimated intention-to-treat (ITT) effect on efficiency of β_e, relative to the control-group mean $\bar{e}_0$, implies a proportional increase in output of

$$\frac{\Delta Q}{Q_0} = \frac{\beta_e}{\bar{e}_0}. \tag{3}$$

To value the additional output, we use the plant's average operating profit per garment, calculated as annual operating profit divided by annual production:

$$\bar{\pi} = \frac{\Pi_{op}}{Q_{plant}}, \tag{4}$$

where Π_{op} and Q_{plant} denote the plant's annual operating profit and total output, respectively. The annual monetary benefit from improved productivity is then

$$G_{prod} = \frac{\beta_e}{\bar{e}_0} Q_0 \, \bar{\pi}, \tag{5}$$

where Q_0 is the baseline annual output of the teams receiving the intervention.

This calculation assumes that the plant can sell the additional output, as would be the case when production capacity constrains its ability to fulfill orders. Valuing additional output at average operating profit per garment implicitly assumes that operating costs increase proportionally with production. In practice, because the additional output is produced using the existing workforce and equipment, fixed costs need not increase. The resulting estimate is therefore conservative if the incremental profit per garment exceeds the plant's average operating profit per garment. When production is instead constrained by a fixed volume of orders, efficiency improvements generate savings in labor time rather than additional output. We discuss this alternative scenario in Appendix A.4.2.

6.2 Retention Channel

We next quantify the savings from reduced worker turnover. Let β_s denote the estimated ITT effect on the probability of leaving the plant within T months of the intervention, and let p_0 be the corresponding probability in the control group. We convert these probabilities into constant monthly separation hazards, λ_0 and λ_1, for the control and treatment groups, respectively (Appendix A.4.3).

Under this assumption, the expected number of separations avoided over one year among the initial workforce of N workers is

$$A = N\left[(1-\lambda_1)^{12} - (1-\lambda_0)^{12}\right] \approx -\beta_s \cdot \frac{12}{T} \cdot N. \tag{6}$$

Each avoided separation reduces the costs associated with replacing a departing worker. We decompose the replacement cost into four components:

$$C_{sep} = C_{hire} + C_{train} + C_{ramp} + C_{vac}, \tag{7}$$

where C_{hire} represents recruitment costs, C_{train} captures training expenses, C_{ramp} measures productivity losses during the new hire's adjustment period, and C_{vac} captures production losses while the position remains vacant.

The annual monetary benefit from reduced turnover is therefore

$$G_{ret} = A \cdot C_{sep}. \tag{8}$$

An important consideration is that lower turnover may also improve measured team efficiency by reducing the share of hours worked by inexperienced new hires. Consequently, including both the full productivity gain and the productivity losses avoided during the adjustment period could lead to double counting. To address this concern, we exclude ramp-up costs from the replacement cost in our main calculation. Appendix A.4.4 discusses this adjustment and an alternative approach that removes the

workforce-composition effect from the estimated efficiency gain.

6.3 Implementation Costs and Benefit–Cost Ratio

Combining the two channels, the total annual benefit of the intervention is $B = G_{prod} + G_{ret}$.

We calculate the annual implementation cost from the perspective of a plant that independently operates the intervention. The recurring costs consist of two components. First, the plant employs a data analyst to process worker-level production data and prepare information reports for team managers. This includes the analyst's monthly salary (w_{an}), employer social insurance contributions (s_{an}), and software expenses (c_{sw}). Second, team managers devote time to reviewing and using the information, which we value at their hourly wage.

For plant p with M_p team managers, the total annual implementation cost is

$$K_{1,p} = 12\left(w_{an} + s_{an} + c_{sw}\right) + M_p \cdot f \cdot h_{int} \cdot \frac{w_m}{h_m}, \tag{9}$$

where f is the number of interventions per year, h_{int} is the number of hours each manager spends per intervention, and w_m/h_m is the manager's hourly wage, calculated using monthly earnings and working hours.

We evaluate the intervention over a one-year horizon, excluding one-time setup costs. The annual net benefit and benefit–cost ratio (BCR) are calculated as

$$NB = B - K_1, \qquad BCR = \frac{B}{K_1}. \tag{10}$$

A BCR greater than one indicates that the monetary benefits exceed the implementation costs.

We evaluate each treatment arm separately, assuming that the intervention is implemented across all teams within a plant. The treatment arms therefore represent alternative implementation strategies, and their benefits are not additive.

Finally, because several parameters used in the calculations are available only as ranges, we consider two scenarios. In the *pessimistic* scenario, we use the lower bound of each benefit-related parameter and the upper bound of each cost parameter. The *optimistic* scenario reverses these assumptions. These scenarios capture uncertainty in the monetary inputs rather than statistical uncertainty in the estimated treatment effects. To account for the latter, we implement a cluster bootstrap that resamples teams within randomization strata and treatment arms. In each bootstrap iteration, we re-estimate both the efficiency and turnover effects using the same resampled teams, preserving their correlation when constructing confidence intervals for the cost-effectiveness measures.

6.4 Calibration

We calibrate the cost-effectiveness analysis using the pooled intention-to-treat (ITT) estimates from the three participating plants, applying the same estimated treatment effects to each plant. For the productivity channel, we use the estimated effect on complexity-adjusted team efficiency, which accounts for differences in the number of operations required by each garment style, and express it relative to the corresponding control-group mean. For the retention channel, we use the estimated effect on the probability of leaving the plant during the approximately four-month post-intervention period ($T = 4$). Plant-specific annual output, workforce size, and standard minutes per garment are calculated using each plant's production records.

To monetize these effects, we draw on information provided by the participating garment plants and make assumptions about several cost parameters. First, the plants report an average operating profit of around 2 RMB per garment.[9] Second, we estimate the average cost of replacing a departing worker at 2,265 RMB. This estimate includes recruitment expenses, such as job advertisements and pre-employment medical examinations, as well as training-related costs. We also account for additional material costs and machine depreciation associated with new workers' adjustment to produc-

[9]Profit per garment varies depending on several factors, including order volume, client tier, technical requirements, and rework rates. Plants report profits of 1–3 RMB per garment. We use 2 RMB per garment as the average.

tion processes and equipment. Third, we estimate the annual cost of implementing the intervention based on the personnel, software, and managerial time required. We assume that each plant employs one data analyst, with an average monthly salary of 12,500 RMB, employer social insurance contributions of 29.8% of the monthly salary, and teh average software expenses of 750 RMB per month. Each team has one manager earning approximately 6,500 RMB per month, who spends five hours per intervention communicating with the data analyst, learning to interpret the performance metrics, reviewing the reports, and participating in follow-up discussions. Finally, we consider intervention frequencies ranging from one to four rounds of information provision per year.

6.5 Results

Table 10 summarizes the annual benefits and costs for the three plants together, in US dollars per year, with bootstrap standard errors and 95% confidence intervals. The productivity and retention channels account for most of the benefits in both cases. The two treatment arms give similar results. Overall, the intervention generates an annual net profit of $107,248–$126,301, with total benefits amounting to approximately 2.24–2.45 times the costs.

[Insert Table 10 Here]

7 Conclusion

We use a randomized field experiment in three garment manufacturing plants in China to examine whether reducing information frictions improves managers' task-allocation decisions. Providing managers with systematic information about workers substantially improves their knowledge and improves task assignment along the efficiency dimension. These changes translate into economically meaningful gains: team efficiency increases by about 10 percent and worker turnover falls by around 39 percent, with no detectable decline in product quality. Managers, however, use the information

selectively. Although they learn along each dimension provided, including workers' task preferences when that information is given, only information on workers' relative efficiency across tasks measurably affects assignment decisions. Taken together, the results show that information frictions can constrain managerial decisions, but reducing these frictions does not ensure that managers incorporate all available information into complex allocation decisions.

These findings suggest a potentially cost-effective way for firms to improve management: transform routinely collected production data into information that frontline managers can use in day-to-day decisions. Our cost-effectiveness analysis indicates that the intervention generates annual benefits of $172 - $189 per worker, about 2.24-2.45 times its implementation cost, with most of the gains coming from higher productivity and additional savings from lower worker turnover. This is notable because the intervention improves managerial decisions without changing worker incentives or production technology. At the same time, our results show that better information alone is not sufficient when managers must balance multiple objectives. For dimensions such as worker preferences, complementary changes in decision processes, incentives, or managerial support may be needed for the information to affect actual allocation decisions. More broadly, the results point to a potential additional return to investments in data infrastructure: detailed worker-level production data are valuable not only for record keeping and monitoring, but also as inputs into managerial decisions. While our experiment does not estimate the returns to collecting such data, the results suggest that investments in generating and organizing administrative data may enable improvements in management that would otherwise not be possible.

Our findings also point to several directions for future research. A key question is how managers can better integrate multiple dimensions of information when making complex decisions. Simpler summary measures, training, or decision-support tools may help, but it remains unclear whether managers would rely on such tools when their recommendations conflict with managers' own judgment or tacit knowledge of workers. Future work could also examine how frequently information needs to be updated as

worker skills and team composition change over time, and whether similar information frictions arise in other settings in which managers allocate workers across tasks.

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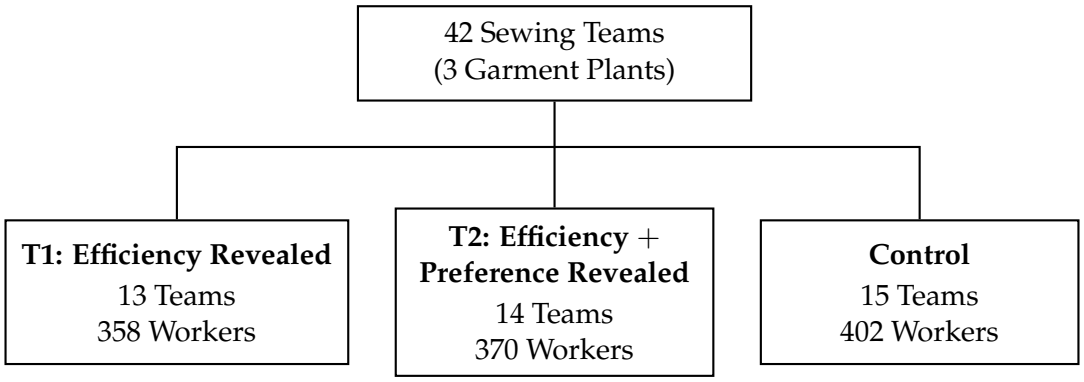
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Figure 1: Randomization Design



Note: The randomization is conducted at the team level and stratified by team size and performance within each plant.

Figure 2: Intervention Timeline

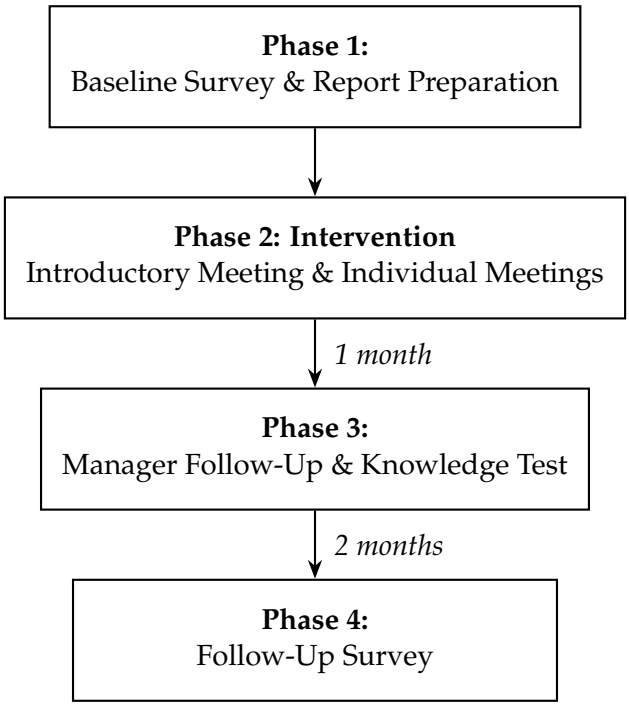


Figure 3: Distribution of Post-Intervention Knowledge Test Scores

This figure displays the distribution of knowledge-test z-scores constructed from the follow-up knowledge test across treatment arms.

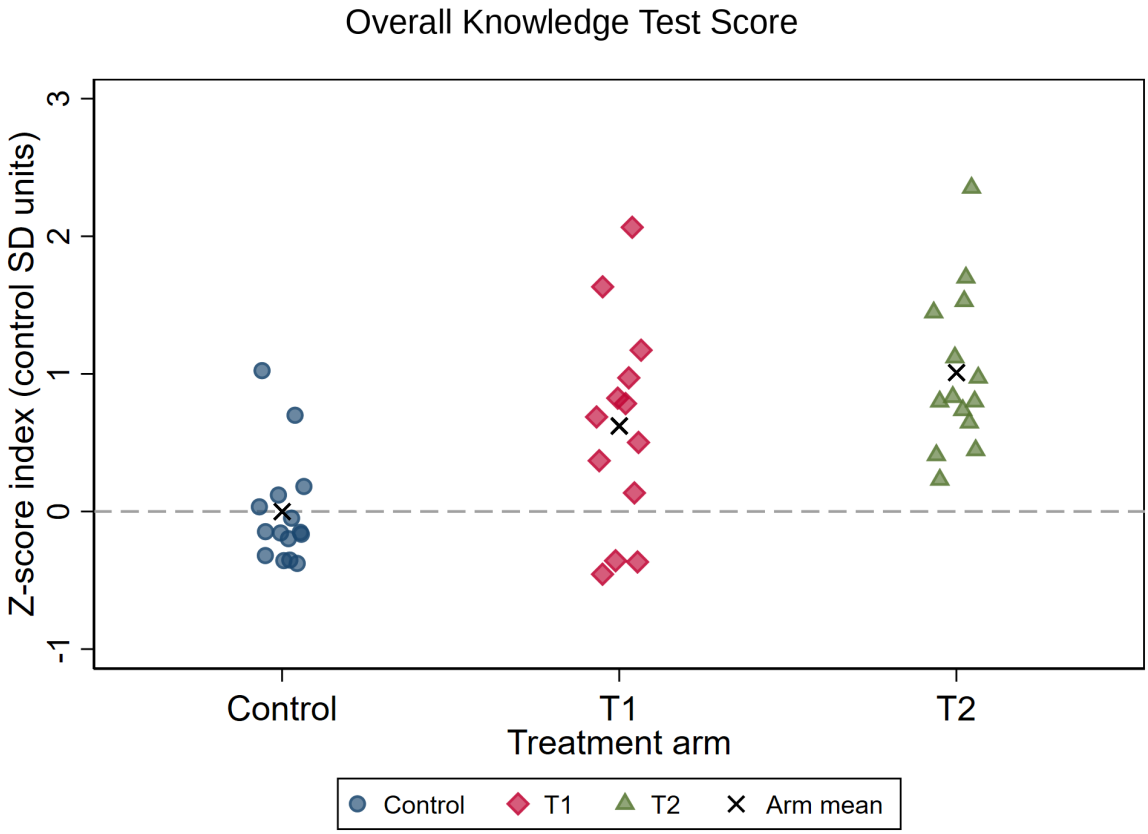


Table 1: Pre-intervention Summary Statistics for the Administrative and Survey Data

	(1) Control	(2) Treatment 1	(3) Treatment 2	(4) Difference (2)-(1)	(5) Difference (3)-(1)	(6) N
Manager characteristics						
Age	44.667	41.692	41.071	-2.974	-3.595	42
	(8.200)	(4.679)	(5.811)	(0.238)	(0.179)	
Female	0.667	0.692	0.571	0.026	-0.095	42
	(0.488)	(0.480)	(0.514)	(0.889)	(0.612)	
Tenure	5.023	3.980	4.038	-1.043	-0.985	42
	(2.251)	(2.463)	(2.660)	(0.251)	(0.290)	
Education(yrs)	9.400	9.462	10.071	0.062	0.671	42
	(1.549)	(1.127)	(1.900)	(0.904)	(0.305)	
Team composition						
Age	43.370	43.038	42.858	-0.331	-0.512	42
	(2.395)	(2.234)	(2.443)	(0.707)	(0.573)	
Female	0.873	0.912	0.904	0.039	0.031	42
	(0.083)	(0.052)	(0.064)	(0.135)	(0.270)	
Tenure	3.184	3.409	3.331	0.226	0.148	42
	(1.203)	(1.160)	(1.184)	(0.617)	(0.741)	
Education(yrs)	9.130	9.251	9.217	0.120	0.086	42
	(0.250)	(0.359)	(0.244)	(0.317)	(0.353)	
Team Performance						
Size	29.467	30.462	28.143	0.995	-1.324	42
	(8.184)	(8.038)	(6.826)	(0.748)	(0.638)	
TurnOver	0.162	0.156	0.137	-0.006	-0.025	42
	(0.111)	(0.071)	(0.087)	(0.873)	(0.510)	
Productivity	0.871	0.860	0.868	-0.011	-0.003	42
	(0.171)	(0.169)	(0.183)	(0.863)	(0.967)	
Quality	0.862	0.863	0.865	0.001	0.003	42
	(0.027)	(0.060)	(0.032)	(0.954)	(0.799)	
Bonus	945.035	898.600	919.680	-46.435	-25.355	42
	(806.030)	(702.930)	(776.799)	(0.871)	(0.932)	
Survey Data						
Satisfaction	-0.000	-0.032	-0.018	-0.032	-0.018	1086
	(0.515)	(0.552)	(0.508)	(0.562)	(0.724)	
Evaluation	-0.000	-0.089	0.034	-0.089	0.034	1086
	(0.862)	(0.943)	(0.835)	(0.442)	(0.639)	

Notes: Columns (1) to (3) report means for each group, with standard deviations in parentheses. Columns (4) and (5) present coefficients from regressions of each outcome variable on treatment indicators, with p-values in parentheses. The control group serves as the baseline category in all regressions. Satisfaction and Evaluation are composite z-scores: each of the 15 satisfaction and 9 team-manager evaluation survey items is standardized by subtracting the control-group mean and dividing by the control-group standard deviation, and the item z-scores are then averaged.

Table 2: Summary Statistics: Managers' Baseline Knowledge-Test Scores

	(1) Observations	(2) Mean	(3) Min	(4) Max	(5) Median
Efficiency Knowledge	42	2.714	0	4	3
Variability Knowledge	42	0.833	0	3	1

Notes: Each row is a team-level score, from the managers' baseline knowledge test: the number of the manager's named workers who are actually in the team's ground-truth top-5. For the ground-truth of efficiency knowledge, we first use efficiency measure by machine and complexity level calculated using the past one year output data. We first take the flatbed sewing machine-A workers in ranking order; if fewer than five names have been accumulated, we continue with the flatbed sewing machine-B workers in their own ranking order, skipping any workers already taken from flatbed sewing machine-A. Since level A is the most complex task, we treat the workers ranked in the top five on the most complex task as the most efficient workers in that machine category. To obtain the ground truth for variability knowledge, we first calculate the downside volatility of each worker-task pair, then pooled into a single score per worker, weighted by the number of observations for each task. We rank workers within each team by overall variability, with rank 1 denote the most volatile worker. The top-5 ground truth consists of workers with a within-team rank of 5 or lower.

Table 3: Effect of Information on Team Efficiency and Team Quality

	(1) Raw Efficiency	(2) Complexity-Adjusted Efficiency	(3) Quality
Treatment 1	0.0980**	0.0848**	-0.0074
	(0.0439)	(0.0376)	(0.0239)
Treatment 2	0.0616	0.0806**	0.0014
	(0.0419)	(0.0354)	(0.0156)
Control mean	0.5887	0.7915	0.8346
p-value (T1 = T2)	0.373	0.906	0.725
Observations	450	450	116
Clusters (teams)	42	42	42
Randomization batch x stratum FE	Yes	Yes	Yes
Time fixed effects	Plant x week	Plant x week	Plant x month
Equal team weights	Yes	Yes	Yes
Controls for pre-intervention mean	Yes	Yes	Yes

Notes: The sample is post-intervention team-weeks, event weeks 0-10. Dependent variable in Column (1) is the raw weekly efficiency, and dependent variable in Column (2) is raw weekly efficiency net of style complexity, where complexity is the number of distinct operations of the style performed by the team, and its coefficients are estimated on pre-intervention team-days only then held fixed. Log style tenure is included in that first-stage regression, so the complexity coefficients are not confounded by learning, but it is not subtracted, any treatment effect operating through the learning curve therefore remains in the outcome. We include plant by calendar-week fixed effects and randomization batch x stratum fixed effects throughout, and weight by the inverse of the team's number of observed post-intervention weeks, so each team contributes equal total weight. We control pre-intervention mean in all regressions, which is the team's average over event weeks-11 to-1, an 11-week baseline window matching the 11-week post window. Standard errors clustered by team in parentheses; stars use those cluster-robust standard errors. *** p<0.01, ** p<0.05, * p<0.10.

Table 4: Effect of Information on Worker Turnover

	(1) Workers Leaving Firms
Treatment 1	-0.0441***
	(0.0145)
Treatment 2	-0.0310***
	(0.0107)
Control Mean	0.0954
p-value (T1=T2)	0.345
Number of Clusters	42
Observations	1130

Note: The sample includes workers in roster who had not already left at the time of intervention. The dependent variable is an indicator for whether the worker left the company within 4 months after the intervention. We control for randomization batch by strata fixed effects, and the team's turnover rate in the 3-month window prior to the intervention. Standard errors clustered by team in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 5: Effect of Information Treatment on Manager Knowledge

	(1) Efficiency	(2) Variability & Teamwork	(3) All Efficiency Questions	(4) Preference	(5) Overall Knowledge Test Scores
T1 (Efficiency treatment)	0.7184**	0.7165	0.7180**	0.1816	0.5392**
	(0.3149)	(0.4457)	(0.2980)	(0.3469)	(0.2468)
T2 (Efficiency + preference treatment)	0.8051***	1.0288**	0.8610***	1.3371***	1.0197***
	(0.2019)	(0.4076)	(0.1901)	(0.3560)	(0.1955)
Control Mean	0.0000	0.0000	0.0000	0.0000	0.0000
Test T1=T2 (p-value)	0.787	0.554	0.627	0.007	0.058
Number of Clusters	42	42	42	42	42
Observations	42	42	42	42	42

Notes. Columns (1)-(5) report the average z-scores for efficiency and learning speed, efficiency variability and teamwork, all efficiency-related questions, worker task preferences, and overall knowledge, respectively. Each question-level score is standardized using the control-group mean and standard deviation, and each dependent variable is the mean of the standardized scores across its component questions. Thus, coefficients are expressed in control-group standard deviation units, and the control-group mean is 0 by construction. For scoring rule, managers receive one point for each correctly identified worker in the corresponding team-by-task cell. All columns include randomization batch $\times$ stratum fixed effects. Standard errors clustered by team are reported in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 6: Heterogeneous Effects of Treatment on Manager Knowledge by Management Practice Scores (Pool Treatment)

	(1) Efficiency	(2) Variability & Teamwork	(3) All Efficiency Questions	(4) Preference	(5) Overall Knowledge Test Scores
Treatment	1.001***	0.908**	0.977***	1.056**	1.004***
	(0.370)	(0.367)	(0.324)	(0.462)	(0.255)
High baseline mgmt practice z-score (>plant median)	0.186	1.068**	0.406*	1.233**	0.682***
	(0.276)	(0.430)	(0.227)	(0.502)	(0.250)
Treated x High baseline mgmt practice z-score (>plant median)	-0.481	-0.509	-0.488	-0.950	-0.642*
	(0.427)	(0.635)	(0.369)	(0.680)	(0.334)
Control mean (>plant median)	0.105	0.459	0.194	0.602	0.330
Observations	42	42	42	42	42

Note: This table examines whether the improvement in managers' knowledge differs by baseline management practices. The dependent variable is the z-score of managers' knowledge measured in the follow-up knowledge test, constructed as in Table 5. Treatment is an indicator equal to 1 for teams assigned to either treatment arm and 0 for control teams. High baseline management practice is an indicator equal to 1 if the team manager's baseline management-practice z-score is above the plant-specific median and 0 otherwise. All specifications include randomization batch $\times$ stratum fixed effects. Standard errors clustered at the team level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Effects on Task Assignment

	Efficiency	Learning	Variability	Multi-worker	Preference	Machine	Level	Part
Treatment 1	0.0408**	0.0248	-0.0349	-0.0269	0.0113	-0.0016	-0.0342	0.0094
	(0.0192)	(0.0232)	(0.0211)	(0.0214)	(0.0210)	(0.0296)	(0.0220)	(0.0325)
Treatment 2	0.0375**	0.0214	-0.0111	0.0007	0.0367	0.0443	0.0087	0.0351
	(0.0156)	(0.0195)	(0.0191)	(0.0232)	(0.0238)	(0.0304)	(0.0291)	(0.0365)
Control mean	-0.2981	-0.3016	-0.2816	-0.2554	-0.3924	-0.1429	-0.3784	-0.4321
p-value: T1=T2	0.8792	0.8847	0.2883	0.2072	0.3004	0.1036	0.0623	0.4789
Observations	2522	2523	2523	2533	2521	2520	2519	2519
Clusters (teams)	42	42	42	42	42	42	42	42
Randomization batch x stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plant x week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Equal team weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls for pre-intervention mean	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable is the team-day demand-adjusted match score for each outcome family, defined as the team's actual match score minus the best-feasible benchmark match score attainable given that day's task mix; higher values indicate task assignments closer to the demand-adjusted benchmark. Control mean reports the raw, unweighted mean of the dependent variable among Control-arm team-days during the post-intervention period. All regressions include randomization-strata and plant $\times$ calendar-week fixed effects, control for the team's pre-intervention mean of the outcome, and weight by the inverse number of team-days so that every team contributes equally. Clustered-robust standard errors by team are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Efficiency Adoption Heterogeneity by Below-Median Baseline Knowledge (Pooled Treatment)

	Match Score (Efficiency)
Treatment	0.0148
	(0.0175)
Treatment x Low Score	0.0674**
	(0.0308)
Low Score	-0.0354
	(0.0250)
Observations	2522
Clusters (teams)	42
Plant x week FE	Yes
Randomization-Stratum FE	Yes
Equal team weights	Yes

Notes: The dependent variable is the demand-adjusted match score, as in Column (1) of Table 7. Treatment is an indicator equal to one for teams assigned to either treatment arm and zero for control teams. Low score is an indicator equal to one if the team manager's baseline knowledge score on the efficiency dimension is below the sample median (median = 3), and zero otherwise. The Treatment coefficient captures the treatment effect for managers with baseline knowledge at or above the median, while the interaction coefficient captures the additional treatment effect for managers with below-median baseline knowledge. All regressions control for randomization-stratum and plant $\times$ calendar-week fixed effects and use equal weights across teams. Cluster-robust standard errors by team are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Effect of Information on Worker Survey Outcomes

	(1) Job satisfaction	(2) Task-allocation sat.	(3) Team-manager eval.
Treatment 1	0.0029	0.0269	0.0074
	(0.0461)	(0.0885)	(0.0841)
Treatment 2	0.0100	0.0771	0.1358*
	(0.0501)	(0.0899)	(0.0779)
Control mean	-0.0000	-0.0000	0.0000
p-value (T1=T2)	0.897	0.537	0.130
Observations	823	823	823
Clusters (teams)	42	42	42

Note: One observation per worker re-surveyed at follow-up. Outcomes are z-scored using the mean and standard deviation of the control group at the follow-up survey; the baseline z-score control is separately z-scored using the mean and standard deviation of the control group at baseline. Every column controls for the worker's baseline z-score of the same outcome. Randomisation batch x stratum fixed effects are included. Standard errors clustered by team in parentheses; stars are based on those cluster-robust standard errors. Because treated workers were more likely to be re-surveyed, these estimate the effect among workers observed at follow-up; see the accompanying Lee bounds. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 10: Annual Benefits and Costs of the Intervention

	Treatment 1 (USD per year)	Treatment 2 (USD per year)
Productivity gain	172,938 (44,729) [82,592, 256,020]	164,380 (43,443) [77,371, 248,574]
Retention gain	40,181 (12,087) [16,027, 62,311]	29,686 (9,696) [10,348, 49,170]
Intervention cost	86,818	86,818
Net benefit	126,301 (52,403) [17,187, 222,407]	107,248 (42,300) [21,751, 186,631]
Benefit–cost ratio	2.45 (0.60) [1.20, 3.56]	2.24 (0.49) [1.25, 3.15]

Notes: Net benefit is calculated as the sum of productivity and retention gains minus intervention costs. Bootstrap standard errors are reported in parentheses, and 95% percentile confidence intervals in square brackets, based on 1,999 joint cluster-bootstrap replications at the team level, stratified by randomization stratum and treatment arm. Both efficiency and turnover effects are re-estimated in each replication to account for their correlation. Intervention costs are treated as fixed and therefore have no associated sampling uncertainty.

Appendix

Enhancing Decision-Making: The Impact of Information Provision on Worker Task Assignment

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A.1 Additional Tables and Figures

Table A1.1: Job Satisfaction Survey Items

No.	Statement
1	Overall, I am satisfied with the tasks assigned to me in the past three months.
2	My supervisor does not consider the feelings of subordinates.
3	I have not been treated fairly by my supervisor.
4	If I feel that my task allocation is unfair, I will calmly communicate with my supervisor.
5	I want to transfer to another sewing team/production line.
6	I feel that my efforts have not been adequately rewarded.
7	My workload is too heavy.
8	My promotion opportunities are too limited.
9	Due to the limited abilities of my colleagues, my workload has increased.
10	I sometimes feel that my work is meaningless.
11	I am frequently criticized by my supervisor.
12	In the past three months, I have considered resigning from the company multiple times.
13	Overall, I am satisfied with my salary income over the past three months.
14	I am willing to work at this company for the long term (e.g., 3 years or more).

Note: Respondents indicated their agreement with each statement on a 5-point scale ranging from Strongly Disagree (1) to Strongly Agree (5).

Table A1.2: Worker Evaluation of Team Manager Performance

No.	Item
1	Organizing production
2	Handling emergencies and conflicts
3	Fairness
4	Listening to workers' opinions and suggestions
5	Caring about workers' work and life
6	Clearly communicates the requirements of my job
7	Provides continued guidance on my work
8	Inspires team morale when facing challenges
9	Trustworthey

Note: Respondents rated their team manager's performance over the past three months on a 5-point scale ranging from Very Poor (1) to Very Good (5).

Table A1.3: Manager Survey: Management Practices

No.	Question
Retrospective Analysis and Employee Management	
1	How often do you do the following retrospective analysis: What went right and how can I assure it will happen again?
2	How often do you do the following retrospective analysis: What are the issues I can avoid being repeated next time?
3	How often do you do the following retrospective analysis: What can I do to ensure employees see me as the best manager possible?
4	How often do you do the following retrospective analysis: What can I do to motivate my employees better?
5	If you find that an employee is performing poorly (possibly due to a lack of motivation or insufficient skills), what measures would you take to address the issue?
6	If a high-performing employee plans to resign, what measures would you take to persuade them to stay?
Management Philosophy	
12.1	Maintaining daily communication with workers helps identify problems and improve efficiency.
12.2	As long as there are no obvious problems, I usually do not need to adjust my task allocation strategy.
12.3	I tend to make management decisions based on past experience.
12.4	As long as workers complete tasks as required, they do not need to express their opinions.
12.5	I am used to communicating with subordinate workers in an authoritative, unquestionable tone.
12.6	In the past three months, I have proactively inquired about situations where workers' physical health, emotions, or family matters affected their work.

Note: Items 1-4 use a 6-point frequency scale (Never, Less than once a month, Once a month, Once a week, Every other day, Every day); items 5-6 are check-all-that-apply; items 12.1-12.6 use a 5-point agreement scale ranging from Strongly Disagree (1) to Strongly Agree (5).

Table A1.4: Garment Complexity and Raw Daily Team Efficiency, Pre-Intervention Sample

	(1) Raw Team Efficiency
Number of operations	-0.002327**
	(0.000895)
Log style tenure	0.131345***
	(0.010403)
Plant fixed effects	Yes
Observations	14,168
Clusters (teams)	42
Adjusted R-squared	0.3853

Notes: OLS regression of raw daily team efficiency on the number of distinct tasks in the team's style that day, a garment-level complexity measure, and log style tenure, estimated on each plant's pre-intervention team-days only. Clustered standard errors by team in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A1.5: Effect of Information on Team Efficiency: Style FE vs. Style FE + Tenure

	Style FE Only	Style FE + Log Tenure
Treatment 1	0.0565*	0.0592**
	(0.0280)	(0.0249)
Treatment 2	0.0714**	0.0561**
	(0.0286)	(0.0236)
Control mean	0.6422	0.6321
p-value: T1 = T2	0.5795	0.8893
Observations	450	450
Clusters (teams)	42	42
Randomisation batch x stratum FE	Yes	Yes
Plant x week FE	Yes	Yes
Equal team weights	Yes	Yes
Controls for pre-intervention mean	Yes	Yes

Note: The sample is post-intervention team-weeks, event weeks 0-10. The dependent variables in Column (1) and Columns (2) are weekly team efficiency residualized on style fixed effects and weekly team efficiency residualized on both style fixed effects and log style tenure that accounts for the learning rate of teams. Pre-intervention mean is the team's average of the same measure over its 11 pre-intervention weeks. Randomization batch $\times$ stratum fixed effects and plant $\times$ calendar week fixed effects are included in all columns. We also weight observations by the inverse of the team's number of observed post-intervention weeks, so every team contributes the same total weight. Standard errors clustered by team in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A1.6: Follow-up Knowledge Test Items

No.	Question
Part 1 – Efficiency Questions	
1	List the three workers in your team who are most efficient at operating flatbed sewing A-level, ranked from highest to lowest efficiency.
2	List the three workers in your team who have the highest learning speed when performing flatbed sewing A-level tasks at the start of a new style, ranked from fastest to slowest learning speed.
3	List the three workers in your team who are most efficient at operating flatbed sewing B-level tasks, ranked from highest to lowest efficiency.
4	List the three workers in your team who have the highest learning speed when performing flatbed sewing B-level tasks at the start of a new style, ranked from fastest to slowest learning speed.
5	List the three workers in your team who are most efficient at operating flatbed sewing C-level tasks, ranked from highest to lowest efficiency.
6	List the three workers in your team who have the highest learning speed when performing flatbed sewing C-level tasks at the start of a new style, ranked from fastest to slowest learning speed.
7	List the three workers in your team whose efficiency on B-level tasks fluctuates the most (i.e., frequently and noticeably falls below their personal average level), ranked from highest to lowest degree of fluctuation.
8	List the names of workers in your team whose efficiency is noticeably higher when performing multi-worker tasks than when working alone.
Part 2 – Preference Questions	
9	List the names of three workers in your team who prefer performing flatbed sewing A-level tasks.
10	List the names of three workers in your team who prefer performing flatbed sewing B-level tasks.
11	Suppose there were a staff shortage for A-level tasks — among workers who do not normally do A-level work, who do you think would be willing to fill in? Please give the names of three workers.
12	Please list the names of three workers in your team who prefer doing pocket-related tasks.

Table A1.7: Effect of Information on Individual Survey Items

	Treatment 1	Treatment 2	p(T1=T2)
Panel A: Job satisfaction items			
Task assignment	0.0269	0.0771	0.5375
	(0.0885)	(0.0899)	
Empathy	-0.0006	0.0146	0.8388
	(0.0751)	(0.0752)	
Fairness	0.1012	0.1514*	0.5741
	(0.0938)	(0.0859)	
Voice	0.0394	0.0193	0.7496
	(0.0808)	(0.0770)	
Transfer	0.0668	0.0133	0.5901
	(0.0874)	(0.0996)	
Reward effort	0.0192	-0.1070	0.2252
	(0.0896)	(0.0843)	
Workload	-0.1165	0.0478	0.0397
	(0.0862)	(0.0839)	
Promotion	0.0671	0.0103	0.5154
	(0.0845)	(0.1058)	
Colleagues' ability	-0.0239	-0.0413	0.7851
	(0.0628)	(0.0710)	
Job meaning	-0.1434	-0.0432	0.3108
	(0.0878)	(0.0880)	
Criticism	0.0719	-0.0214	0.3261
	(0.0909)	(0.0860)	
Quit	-0.1570	-0.0222	0.1579
	(0.0980)	(0.0713)	
Wage	-0.0087	0.0451	0.7627
	(0.1329)	(0.1480)	
Tenure	0.0760	0.0024	0.4728
	(0.1036)	(0.0796)	
Welfare	-0.0734	-0.0057	0.5778
	(0.1219)	(0.1142)	
Panel B: Team-Manager evaluation items			
Organize production	-0.0189	0.1371	0.1327
	(0.1107)	(0.1026)	
Conflict management	0.0006	0.0662	0.4527
	(0.0871)	(0.0755)	
Fairness	0.0131	0.0650	0.5788
	(0.1014)	(0.0881)	
Listen	-0.0136	0.1408*	0.0550
	(0.0902)	(0.0744)	
Care	0.0382	0.1514*	0.1797
	(0.0853)	(0.0791)	
Communicate clearly	-0.0004	0.1821*	0.0699
	(0.1041)	(0.1017)	
Guidance	0.0186	0.1596	0.2176
	(0.1024)	(0.1016)	
Morale	-0.0253	0.1364	0.1443
	(0.1061)	(0.0953)	
Trustworthy	0.0040	0.1709*	0.1043
	(0.0911)	(0.0887)	

Notes. Each row reports a separate regression of one survey item on the two treatment indicators. All regressions control for the worker's baseline value of the same item and randomization batch $\times$ stratum fixed effects. Follow-up items are standardized using the control-group mean and standard deviation at follow-up; baseline items are standardized using the corresponding control-group distribution at baseline. One observation per worker re-surveyed at follow-up. Standard errors clustered by team are reported in parentheses. The last column reports the cluster-robust p-value for the test of equality between the two treatment coefficients. The 24 regressions decompose the composite indices reported in the main text. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A1.8: Decomposition of Survey Non-Response at Follow-Up

	Control	T1	T2	All
Baseline respondents at risk	404	387	349	1140
Panel A: Follow-up status				
Followed	278	281	264	823
	(68.8%)	(72.6%)	(75.6%)	(72.2%)
Not followed	126	106	85	317
	(31.2%)	(27.4%)	(24.4%)	(27.8%)
Panel B: Decomposition of non-follow-up				
Left the firm	67	40	36	143
	(16.6%)	(10.3%)	(10.3%)	(12.5%)
Still on roster, not re-surveyed	41	35	34	110
	(10.1%)	(9.0%)	(9.7%)	(9.6%)
Left status unknown	18	31	15	64
	(4.5%)	(8.0%)	(4.3%)	(5.6%)

Note: This table decomposes survey non-response at follow-up by treatment arm. The sample includes all respondents to the baseline survey. Panel A reports the proportions of baseline respondents who were successfully followed at follow-up and those whom we were unable to follow. Panel B further decomposes non-follow-up into three categories: respondents who left the company before the follow-up survey; respondents who remained on the company roster but did not respond to the follow-up survey; and respondents who provided incorrect personal information, preventing us from matching them to the company roster and determining their employment status. Percentages in parentheses are calculated relative to the number of baseline respondents in each treatment arm.

Table A1.9: What Accounts for the Differential Follow-Up Rate?

	(1)	(2)	(3)	(4)
	Re-surveyed	Not re-surveyed, because:		
	at follow-up	Left firm	Still employed	Unmatched
Treatment 1	0.0540	-0.0649**	-0.0216	0.0326
	(0.0501)	(0.0292)	(0.0345)	(0.0204)
Treatment 2	0.0853**	-0.0722***	-0.0099	-0.0031
	(0.0418)	(0.0260)	(0.0314)	(0.0157)
Control mean	0.6881	0.1658	0.1015	0.0446
p-value (T1 = T2)	0.453	0.780	0.717	0.103
Observations	1140	1140	1140	1140
Clusters (teams)	42	42	42	42

Note: This table examines the sources of differential follow-up non-response across treatment arms. Each column reports a separate OLS regression of the indicated outcome on treatment-arm indicators, using all baseline survey respondents. The dependent variables are mutually exclusive and exhaustive indicators that partition the baseline sample. The dependent variable of Column (2) indicates that the respondent was not re-surveyed and was recorded as having left the firm before the follow-up survey. The dependent variable of Column (3) indicates that the respondent was not re-surveyed but remained on the firm roster, representing survey non-response among workers who remained employed. The dependent variable of Column (4) indicates that the respondent was not re-surveyed and could not be matched to administrative records due to reporting errors, so employment status cannot be determined. All regressions include randomization batch-by-stratum fixed effects. Standard errors, clustered at the team level, are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A1.10: Baseline Satisfaction and Team-Leader Evaluation of Stayers and Leavers

	Stayers (1)	Leavers (2)	Difference (3)	p-value (4)	Obs. (S / L) (5)
Panel A: Indices					
Job satisfaction index (15 items)	0.019	-0.077	-0.095	0.142	908 / 152
Task-allocation satisfaction	0.057	-0.046	-0.103	0.182	908 / 152
Team-leader evaluation index (9 items)	-0.014	0.037	0.050	0.577	908 / 152
Panel B: Job-satisfaction items					
Task allocation	0.057	-0.046	-0.103	0.182	908 / 152
Supervisor empathy	0.039	0.115	0.076	0.525	908 / 152
Fairness	-0.017	0.044	0.060	0.595	908 / 152
Voice	-0.030	-0.020	0.010	0.924	908 / 152
Transfer	0.020	0.082	0.062	0.594	908 / 152
Effort	0.003	-0.130	-0.133	0.245	908 / 152
Workload	-0.040	-0.168	-0.129	0.208	908 / 152
Promotion	0.029	-0.241	-0.269***	0.008	908 / 152
Colleagues	-0.016	-0.117	-0.102	0.309	908 / 152
Meaningfulness of work	0.030	-0.007	-0.036	0.720	908 / 152
Criticism	0.018	0.053	0.035	0.739	908 / 152
Quit intention	0.105	-0.202	-0.307***	0.009	908 / 152
Wage	0.026	-0.176	-0.202*	0.091	908 / 152
Wants to stay long term (3+ yrs)	0.144	-0.278	-0.422***	0.000	908 / 152
Welfare	-0.089	-0.058	0.031	0.809	908 / 152
Panel C: Team-leader evaluation items					
Organises production	-0.009	0.055	0.064	0.498	908 / 152
Manages conflict	-0.025	-0.002	0.023	0.827	908 / 152
Fairness	-0.002	0.065	0.067	0.475	908 / 152
Listens	0.001	0.068	0.067	0.483	908 / 152
Cares about workers	-0.030	0.028	0.059	0.596	908 / 152
Gives clear instructions	-0.007	0.095	0.102	0.411	908 / 152
Provides guidance	-0.007	0.069	0.076	0.498	908 / 152
Boosts morale	-0.009	-0.022	-0.013	0.896	908 / 152
Trustworthy	-0.034	-0.026	0.008	0.936	908 / 152

Note: Sample is baseline survey respondents matched to the turnover panel (908 stayers, 152 leavers, 42 teams). Leavers are workers who showed up in the baseline survey but left the company before the follow-up survey; stayers did not. All variables are baseline values in z-score units, standardized on the baseline control group; indices are the unweighted mean of item z-scores. Column (3) is the difference between leavers and stayers. Column (4) is the p-value of a regression of the variable on a leaver dummy with standard errors clustered by team. Column (5) reports the number of non-missing stayers (S) and leavers (L). *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A1.11: Lee (2009) Bounds on the Effect of Information on Worker Survey Outcomes

Panel A: Follow-up rate by arm			
	Baseline respondents	Follow-up rate	Trim share
Control	404	0.6881	—
Treatment 1	387	0.7261	0.0523
Treatment 2	349	0.7564	0.0903
Panel B: Trimmed ANCOVA bounds			
	Job satisfaction	Task-allocation sat.	Team-leader eval.
Treatment 1: Lower bound	-0.0685	-0.0418	-0.0566
	(0.0411)	(0.0899)	(0.0784)
Treatment 1: Upper bound	0.0245	0.1771**	0.0795
	(0.0417)	(0.0829)	(0.0760)
Treatment 2: Lower bound	-0.0712*	-0.0424	0.0423
	(0.0378)	(0.0780)	(0.0715)
Treatment 2: Upper bound	0.0749	0.2680**	0.2480***
	(0.0505)	(0.0982)	(0.0758)
Observations (T1 vs control)	544	544	544
Observations (T2 vs control)	518	518	518

Note: Panel A reports the share of baseline respondents in each treatment arm who were successfully re-surveyed at follow-up. Because follow-up rates are higher in the treated arms, we trim the outcome distributions of each treated arm by the fraction shown in the final column, $(s_a - s_c)/s_a$, where s_a denotes the follow-up rate in treated arm a and s_c denotes the follow-up rate in the control arm. The control distribution is left unchanged. Panel B reports Lee bounds obtained after trimming. For each treated arm, we estimate the treatment effect separately against the control group, excluding the other treated arm because Lee bounds are defined for binary treatment comparisons. Hence observations are the estimation sample for each pair of bounds: control and the indicated treated arm only, restricted to workers re-surveyed at follow-up and net of the trimmed observations. Each specification regresses the outcome on a treatment indicator, the baseline z-score of the same outcome, and randomization batch $\times$ stratum fixed effects; standard errors are clustered by team. The lower bound trims the highest outcomes from the treated distribution, corresponding to the assumption that workers retained because of treatment have the highest satisfaction or evaluation scores. The upper bound trims the lowest outcomes, corresponding to the assumption that these workers have the lowest scores. Ties are broken randomly using a fixed seed. Identification requires monotonicity, i.e., that treatment affects selection in only one direction; the estimated reduction in turnover is consistent with this assumption. Statistical significance is based on cluster-robust standard errors for the corresponding bound. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

A.2 Construction of Team Efficiency Measures

This appendix describes how we construct the three team efficiency measures used in the paper: raw team efficiency, team efficiency residualized on style fixed effects and the learning rate, and complexity-adjusted team efficiency. We also explain why the adjusted measures, rather than raw efficiency, are our preferred outcomes.

A.2.1 Raw Team Efficiency

The three plants shared detailed daily output data for each worker and task, the standard allowable time (SAT) of each task, and the actual seconds worked by each worker per task and day. We calculate daily team efficiency for team j on day d as

$$\text{Eff}_{jd} = \frac{\sum_{i=1}^{n_{jd}} \sum_{o=1}^{k_i} \text{output}_{io} \times \text{SAT}_o}{\sum_{i=1}^{n_{jd}} H_{ijd}}, \tag{11}$$

where n_{jd} is the number of workers on team j on day d, k_i is the number of tasks performed by worker i, output_{io} is worker i's completed output of task o, SAM_o is the standard allowed minutes for task o, and H_{ijd} is the actual seconds worked by worker i on team j on day d. The numerator is thus the total standard time of all tasks the team completed in a day, and the denominator is the actual time the team spent working; their ratio is the team's realized output relative to the benchmark. Weekly efficiency is the mean of daily efficiency over the week. This construction is the standard measure of line/team efficiency used in the garment manufacturing industry.

Raw efficiency contains variation that is unrelated to treatment and adds noise to the estimation. Equation 11 measures a team's realized output relative to a benchmark, but does not account for the fact that efficiency across teams may not always be comparable: some are producing more complex styles than others, and some have had more days of practice on their current style than others. Because treatment is randomized, there is no reason to expect these factors to be correlated with treatment status in expectation, so raw efficiency comparisons across arms are not biased by them. However, style and tenure differences still generate team-to-team variation in raw efficiency that has nothing to do with the treatment. With a modest number of clusters (42 teams), this nuisance variation inflates the standard errors of the treatment-effect estimate and reduces statistical power.

A.2.2 Team Efficiency Residualized on Style Fixed Effects and the Learning Rate

Raw team efficiency (equation 11) bundles together two sources of variation that are unrelated to the intervention itself: (i) which garment style a team happens to be producing in a given period, since styles differ in production difficulty, and (ii) how long the team has already been producing that style, since efficiency rises mechanically with practice (learning-by-doing). To remove both sources of variation, we estimate, separately for each plant p,

$$\text{eff}_{jd} = \alpha_p + \lambda_s + \theta \ln(\text{tenure}_{jd}) + u_{jd}, \tag{12}$$

where λ_s is a fixed effect for the style s that team j is producing on day d, and tenure_{jd} is the team's cumulative number of production days on that day's style. The adjusted efficiency measure is the regression residual, re-centered on the sample mean

so that it remains on the same scale as raw efficiency:

$$\widetilde{Y}_{jd} = \hat{u}_{jd} + \overline{\text{eff}}. \tag{13}$$

Weekly values are the team-week means of the adjusted daily measure $\widetilde{Y}_{jd}$.

We also report a variant that residualizes on style fixed effects alone, dropping the $\theta \ln(\text{tenure}_{jd})$ term from equation 12; this isolates the contribution of style/complexity without also purging learning-by-doing, since treatment can affect learning rate.

Treatment effects on this measure are estimated with a post-treatment weekly panel ANCOVA,

$$Y_{jt} = \alpha + \beta_1 T1_j + \beta_2 T2_j + \rho\, \overline{Y}_j^{\text{pre}} + \delta_s + \gamma_t + \varepsilon_{jt}, \tag{14}$$

where $\overline{Y}_j^{\text{pre}}$ is the team's mean of the same measure over its pre-intervention weeks, δ_s denotes randomization batch $\times$ stratum fixed effects, and γ_t denotes calendar-week fixed effects.

Appendix Table A1.5 shows the regression results. The results are robust to controlling for style FE and learning. With style fixed effects alone, the intervention increases team efficiency by 0.057 (by 8.8%) for the efficiency information treatment and 0.071 (by 11.1%) for the efficiency and preference information treatment, both statistically significant. Controlling additionally for log style tenure yields similar estimates of 0.059 (by 9.4%) and 0.056 (by 8.9%), respectively, both significant at the 5% level. The treatment effects are not statistically different across the two treatment arms in either specification (p-values = 0.580 and 0.889).

A.2.3 Complexity-Adjusted Team Efficiency

Some styles are produced by only a handful of teams, so a full set of style dummies can partly absorb team-level fixed effects rather than purely capturing style complexity, which risks netting out genuine cross-team (and potentially treatment-related) variation along with the nuisance complexity variation.

To avoid this issue, we construct an alternative adjustment that nets out product complexity using the number of distinct tasks a garment style requires, rather than a full set of style dummies. Using only pre-intervention team-days, we estimate, separately by plant,

$$\text{efficiency}_{jt} = \alpha + \beta_1\, \text{ntasks}_{jt} + \theta \log(\text{tenure}_{jt}) + u_{jt}, \qquad t \in \text{pre-intervention}, \tag{15}$$

where ntasks_{jt} is the number of distinct operations required by team j's style on day t, a continuous proxy for product complexity. Because this regression is estimated only on pre-intervention data, the complexity adjustment cannot be contaminated by any treatment-induced change in efficiency or learning speed that occurs after the intervention begins.

We then construct the complexity-adjusted daily efficiency by subtracting only the estimated complexity component from raw efficiency:

$$\text{cadj_efficiency}_{jt} = \text{efficiency}_{jt} - \hat{\beta}_1 \, \text{ntasks}_{jt}. \tag{16}$$

Note that only the complexity component is removed; the learning term $\theta \log(\text{tenure}_{jt})$ is *not* subtracted, so any treatment effect that operates through the learning curve remains in the outcome. Finally, we aggregate to the team-week level:

$$\text{cadj_efficiency}_{jw} = \frac{1}{D_{jw}} \sum_{t \in D_{jw}} \text{cadj_efficiency}_{jt}, \tag{17}$$

where D_{jw} is the set of production days for team j in week w.

A.3 Construction of Demand-Adjusted Match Scores

For each team-day (j, t), workers are assigned to operation cells c, where a cell is a task category (a machine–complexity-level pair), a machine type, or a complexity level, depending on the outcome family f. For the multi-worker task allocation family there is no cell structure, and the score below is defined directly at the team-day level. Let i index workers. Each worker-task observation carries a percentile rank $\rho_i \in [0, 1]$ of the worker's performance, measured relative to a comparison group of size N_{jc} drawn from the team's roster of qualified workers, with higher percentiles denoting better performance.

A.3.1 Actual match score.

Let K_{jtc} denote the number of workers assigned to cell c on team-day (j, t). The cell-level actual match score is

$$\bar{a}_{jtc} = \frac{1}{K_{jtc}} \sum_{i=1}^{K_{jtc}} \rho_i. \tag{18}$$

A.3.2 Best-feasible benchmark.

The benchmark b_{jtc} is the highest average percentile attainable in cell c given the day's demand (K_{jtc} slots) and the team's roster of N_{jc} qualified workers. If percentiles are assigned by rank, $\rho(\text{rank} = r) = \frac{N_{jc}-r}{N_{jc}-1}$, with $r = 1$ denoting the best-performing worker in the comparison group, the highest attainable average percentile among any K_{jtc} of the N_{jc} candidates is the mean percentile of the top K_{jtc} ranks:

$$b_{jtc} = \frac{1}{K_{jtc}} \sum_{r=1}^{K_{jtc}} \frac{N_{jc} - r}{N_{jc} - 1} = \frac{N_{jc} - \frac{K_{jtc}+1}{2}}{N_{jc} - 1}. \tag{19}$$

A.3.3 Cell gap.

The cell gap is

$$g_{jtc} = \bar{a}_{jtc} - b_{jtc} \leq 0, \tag{20}$$

non-positive by construction, and measures the shortfall of the realized allocation from the theoretically best allocation attainable that day given the cell's demand and the team's worker pool.

A.3.4 Aggregation to the team-day level.

For the six cell-structured outcome families — efficiency, learning, variability, task preference, machine preference, and level preference — cell gaps are aggregated into a single team-day match score by weighting each cell by the worker-hours it absorbed that day. Let τ_{ijtc} denote worker i's actual time on cell c on team-day (j, t), and let $\omega_{jtc} = \sum_i \tau_{ijtc}$, summed only over workers whose match status is observed, so that each cell's time weight is consistent with the population over which g_{jtc} is computed. The team-day demand-adjusted match score for outcome family f is

$$\text{Gap}_{jt}^{f} = \frac{\sum_c \omega_{jtc}\, g_{jtc}}{\sum_c \omega_{jtc}}. \tag{21}$$

For the multi-worker task allocation family, the gap is defined directly at the team-

day level by applying equations (18)–(20) with K_{jt} and N_{jt} denoting team-day demand and supply.

A.4 Additional Derivations on Cost-Effectiveness Analysis

A.4.1 Efficiency in pieces and garment equivalents

Production is organised in teams. A garment is made by passing it through a sequence of operations (attaching a collar, setting a sleeve, overlocking a seam, and so on). Each operation has a standard time, the number of minutes a trained worker working at a normal pace needs to complete one piece; in the industry this is called the standard allowed minute (SAM). For a garment whose operations are indexed $i = 1, \ldots, I$, we write s_i for the SAM of operation i and $s_g = \sum_i s_i$ for the SAM of the garment as a whole, i.e. the standard minutes of work contained in one complete garment.

Efficiency in pieces. Consider a worker who performs operation i for h_i minutes and completes q_i pieces. At standard pace those minutes would have produced $q_i^* = h_i / s_i$ pieces, which we call theoretical (standard) output. Efficiency e_i is actual over theoretical pieces:

$$e_i = \frac{q_i}{q_i^*} = \frac{q_i \, s_i}{h_i}. \tag{22}$$

An efficiency of 1 means the worker produced exactly at standard pace; 0.6 means the worker produced 60% of the standard output for the time worked. The product $q_i s_i$ is the standard minutes of work contained in the worker's output.

At the team level, the index i runs over all the worker–operation spells in the team during the period, so $\sum_i h_i$ is the total minutes worked by the team and $\sum_i q_i s_i$ the total standard minutes it produced. Pieces of different operations are not comparable (one piece of a long operation contains more work than one piece of a short operation), so team efficiency is not computed by adding up pieces across operations. Instead, the team's output is converted into *garment equivalents*, $Q_{team} = \sum_i q_i s_i / s_g$: the total standard minutes produced, divided by the standard minutes in one garment, i.e. the number of complete garments the team's work amounts to. This is compared with the theoretical number of garments the team's minutes could produce at standard pace, $Q^*_{team} = \sum_i h_i / s_g$. Team efficiency e is their ratio:

$$e = \frac{Q_{team}}{Q^*_{team}} = \frac{\sum_i q_i s_i}{\sum_i h_i} = \sum_i \frac{h_i}{\sum_j h_j} \, e_i. \tag{23}$$

Team efficiency is therefore actual over theoretical garments, and equals the hours-weighted average of the workers' piece-based efficiencies. We measure team output in garment equivalents rather than in finished garments because this credits each operation for the work it actually completes, so that team output is consistent with the efficiency of the workers who produce it and is not distorted by short-run fluctuations in work in progress. This is also the standard way line efficiency is computed in garment manufacturing: the standard minutes produced (pieces times SAM, summed over operations) divided by the minutes worked, which is exactly (23); garment equivalents simply express the numerator in units of complete garments. On a balanced line, garment equivalents equal finished garments up to changes in work in progress.

Output over a year. Consider a group of N workers who each work H hours per year, so that the group works $60\,H\,N$ minutes in total, and let $\bar{e}$ denote their hours-weighted

average efficiency as defined in (23). In the application, N is the number of workers in the study teams and H their average annual hours worked, both measured in our rosters and output data. Because efficiency is standard minutes produced divided by minutes worked, the standard minutes the group produces over the year are

$$\sum_i q_i s_i = \bar{e} \cdot 60\, H\, N.$$

Each complete garment contains s_g standard minutes of work, so dividing by s_g converts this into garment equivalents, which gives (2). Equation (2) is thus the definition of efficiency solved for output and expressed in garments rather than in standard minutes. When the group makes several styles, s_g is the average standard time per garment, weighted by each style's share of output. Holding hours, headcount and the style mix (hence s_g) fixed, output is proportional to efficiency.

Estimated effects. The treatment effects that enter the calculation are:

- β_e: the ITT effect on team efficiency, i.e., the difference in efficiency between teams whose managers were offered the information and control teams, regardless of how much managers used it. $\bar{e_0}$ is the control-group mean efficiency, the baseline against which the effect is measured;
- β_s: the ITT effect on the probability that a worker leaves the plant within a horizon of T months after the intervention, and p_0, the control-group probability of separating over the same horizon. A negative β_s means the intervention reduces turnover.

A.4.2 Productivity channel

Additional output. From (2), raising efficiency from $\bar{e_0}$ to $\bar{e_0} + \beta_e$ with unchanged hours and style mix raises output by $\Delta Q = (\beta_e / \bar{e_0})\, Q_0$, where Q_0 is the annual output of the teams to which the effect applies, evaluated at baseline efficiency $\bar{e_0}$. The expression follows because, with H, N and s_g fixed in (2), output is proportional to efficiency, so a rise of β_e from a base of $\bar{e_0}$ raises output by the fraction $\beta_e / \bar{e_0}$.

Average operating profit per piece. In (4), Π_{op} is annual operating profit from garment manufacturing and Q_{plant} the number of garments the plant produced in the same year. Π_{op} excludes items unrelated to production (investment income, fair-value changes, gains on asset disposal, government subsidies).

Capacity-constrained plant: more output, same hours. We write G^A_{prod} for the gain in (5) when the plant can sell or take on additional orders. If the effect applies to the whole plant ($Q_0 = Q_{plant}$), it reduces to

$$G^A_{prod,\,plant} = \frac{\beta_e}{\bar{e_0}}\, \Pi_{op}, \tag{24}$$

so the productivity gain as a share of profit equals the proportional efficiency gain.

Worker side. Under piece rates, workers' earnings rise by $\Delta Q \cdot \bar{r}$, where $\bar{r}$ is the sum of the operation piece rates per garment. Piece wages are already deducted in Π_{op}, so this is reported separately as the worker-side benefit and is not added to the firm's gain.

Demand-constrained plant: same output, fewer hours. If orders are fixed, higher efficiency lets the plant produce the same garments with fewer hours. By (2), producing

the baseline output Q_0 at the higher efficiency $\bar{e_0} + \beta_e$ requires only $HN \cdot \bar{e_0}/(\bar{e_0} + \beta_e)$ worker-hours instead of HN, so the worker-hours saved per year, ΔH^B, are

$$\Delta H^B = HN\Big[1 - \frac{\bar{e_0}}{\bar{e_0} + \beta_e}\Big], \tag{25}$$

and the gain is the avoidable cost of those hours, where c_h is the cost the plant avoids for each worker-hour it no longer needs:

$$G^B_{prod} = \Delta H^B \cdot c_h, \qquad c_h = o_h + \theta\, \pi_{ot} + g \tag{26}$$

Here o_h is the overhead that varies with operating hours (supervision, electricity, machine running cost), π_{ot} the overtime premium per hour, θ the share of saved hours that would otherwise be overtime, and g the saving on minimum-earnings top-ups per saved hour. Under pure piece rates, wages follow output rather than hours, so only hour-based costs are saved.

What is reported. The plant's answer to whether it turns away orders or runs short of work determines which of G^A_{prod} and G^B_{prod} is relevant; a mixed case is a weighted average with the share of the year the plant is capacity-constrained as the weight. The application in Section 6 reports G^A_{prod}.

A.4.3 Retention channel

Separations avoided per year. The turnover effect is estimated over a horizon of T months, which need not be a year. Let $p_1 = p_0 + \beta_s$ be the probability of separating within T months under the intervention. To express p_0 and p_1 on an annual basis, we convert each into a constant monthly hazard λ, the probability of leaving in any given month for a worker still employed at its start. A worker stays for T months with probability $(1-\lambda)^T$, so setting $(1-\lambda)^T = 1 - p$ gives

$$\lambda_0 = 1 - (1 - p_0)^{1/T}, \qquad \lambda_1 = 1 - (1 - p_1)^{1/T}. \tag{27}$$

The probability of staying a full year is then $(1-\lambda)^{12}$, and A in (6) is N times the difference in these one-year retention probabilities. A is positive when the intervention lowers turnover ($\beta_s < 0$). For small probabilities, $(1-\lambda)^{12} \approx 1 - (12/T)\, p$, which gives the approximation in (6).

Cost of one separation. In (7), C_{hire} is the recruitment cost per new hire: advertising, agency or referral fees, and staff time spent on screening and onboarding. C_{train} is the direct cost of training: trainer time, wages paid during off-line training, and materials. C_{ramp} is the ramp-up cost, the output lost while a new hire is slower than the worker who left, measured in garment equivalents and valued per piece:

$$C_{ramp} = \sum_{k=1}^{\tau} \left(\bar{e}_L - \bar{e}_{new,k}\right) \frac{60\, H_k}{s_g}\, v, \tag{28}$$

where k indexes weeks since hire, τ is the number of weeks until new hires reach the efficiency of the workers they replace, $\bar{e}_{new,k}$ is the average efficiency of new hires in week k, H_k their hours in that week, and $\bar{e}_L$ the efficiency of workers who would have left. The term $60H_k/s_g$ is the theoretical number of garments the worker's hours represent, so

$(\bar{e}_L - \bar{e}_{new,k})\, 60H_k/s_g$ is the number of garment equivalents lost. The value of a garment equivalent follows the market regime: $v = \bar{\pi}$ if the plant is capacity-constrained, and $v = c_h\, s_g/(60\,\bar{e_0})$ if it is demand-constrained (the cost of the extra hours needed to make up one garment). The ramp-up path can be estimated from rosters (join dates) and daily output (piece-based efficiency by week since joining). Finally, C_{vac} is the vacancy cost, the output lost while the post is empty before the replacement arrives:

$$C_{vac} = d_{vac} \cdot \kappa \cdot \bar{e_0}\, \frac{60\, h_d}{s_g}\, v, \tag{29}$$

where d_{vac} is the number of working days a post stays empty, h_d the hours worked per day, and $\kappa \in [0, 1]$ the share of the absent worker's output that is lost because others cannot cover it ($\kappa = 0$ if the line absorbs the vacancy; κ close to 1 at a bottleneck operation). The term $\bar{e_0} \cdot 60h_d/s_g$ is the number of garment equivalents a worker of average efficiency produces in a day, so the product of the first four terms is the garment equivalents lost during the vacancy.

A.4.4 Avoiding double counting

If team efficiency is computed over all workers, part of β_e already contains the ramp-up saving. Write

$$\beta_e = \beta_e^{alloc} + \beta_e^{comp}, \qquad \beta_e^{comp} \approx \Delta n_{new}\, (\bar{e}_L - \bar{e}_{new}), \tag{30}$$

where β_e^{alloc} is the part of the efficiency effect that comes from better allocation of tasks among the team's workers, and β_e^{comp} the part that comes from a change in the team's composition: fewer hours worked by slow new hires. In the second expression, Δn_{new} is the treatment-induced change in the share of team hours worked by workers still in ramp-up and $\bar{e}_{new}$ their average efficiency, so β_e^{comp} is the shift in hours from new hires to experienced workers times the efficiency gap between them. Two consistent options: (i) use the full β_e in the productivity channel and drop C_{ramp} from C_{sep}; or (ii) use β_e^{alloc} in the productivity channel and keep C_{ramp}. For simplicity, Our application uses option (i), and assumes every worker who quits is replaced one-for-one and immediately with $C_{vac} = 0$.

A.4.5 Scaling from the study sample to the plant.

The effects are estimated on the study teams, so B is the benefit for the N workers in those teams. Applying the effects to all teams assumes the effect is the same in teams outside the sample, and scales B by the ratio of the plant's total workforce, N_{plant}, to the study workforce:

$$B_{plant} = \frac{N_{plant}}{N}\, B. \tag{31}$$

For the productivity channel this is equivalent to setting $Q_0 = Q_{plant}$ in (5), which gives (24).