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Job Rotation and Worker Performance

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Abstract

How should firms allocate workers to jobs? A standard approach is for firms to assign workers to jobs and to train workers for one job. An alternative approach is for firms to rotate workers to different jobs while providing training for multiple jobs. This study investigates the impact of job rotation on worker allocation and efficiency within a garment manufacturing firm in Asia. In this randomized controlled trial, the control group follows the standard, pre-existing practice at the firm, receiving training for and assignment to one job. In the treatment group, workers undergo training for multiple jobs. After training, preferences of both workers and managers guide permanent job assignments using a version of the deferred acceptance algorithm. We examine how the treatment affects performance, employee turnover, work satisfaction, and job preference discovery.

Keywords: Job rotation, cross-skilling, job allocation, worker productivity, job satisfaction
JEL codes: M50, O14, O15
Study pre-registration: This project has been pre-registered under AEARCTR-0012136
Proposed timeline: If accepted based on pre-results review, this study will be completed by July 2025.

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1 Introduction

The allocation (and misallocation) of capital is one potential explanation for the large differences in the level of economic development across countries. The existing literature has provided evidence for misallocation of both physical capital and human capital (Hsieh and Klenow, 2009; Restuccia and Rogerson, 2008; Restuccia et al., 2008; Vollrath, 2009). While much of the existing literature on this topic has focused on the allocation of resources across firms, there is much less work on the potential misallocation of labor within firms.

How should firms allocate workers to jobs? This is an especially important question in the context of low income countries where many positions are low-skilled and semi-skilled and where job applicants often do not have any prior training or experience.[1] In this situation, there are a variety of positions that a worker could be trained for and assigned to.

The most standard approach is for firms to assign each worker to a single job and to train that worker for one job. For job applicants with prior experience, prior experience is often used to guide the job assignment of new hires. However, for job applicants without prior experience, conversations with firms staffing low skilled jobs suggests that these assignments usually do not follow a clearly defined process. Thus, it is unclear whether firms are making the optimal decision regarding how to assign workers across jobs within a firm.

An alternative approach is for firms to rotate workers to different jobs while providing training for multiple jobs.[2] For the firms that have formal job rotation programs for new hires, they usually take into account the workers' preferences over the positions that they have rotated through to make the permanent position assignment.

Job rotation programs can have several benefits.[3] First, it may allow workers and firms to learn about a worker's comparative advantage, and increase productivity through better job matches. Second, it may make workers happier because they get to choose a job that they enjoy more, they feel empowered and appreciate the training provided in multiple skills. Third, the job rotation program may increase organizational agility and flexibility, with workers being able to step into different roles temporarily in response to absences or quits of other workers. Finally, the rotation to a new position may lead a worker to renew their effort (Hakenes and Katolnik, 2017).[4]

[1] In our study sample, about two-thirds of the new hires have never worked in the garment industry prior to starting with our partner firm.

[2] This is a fairly common practice among firms. In a survey of over 6000 firms in different countries in 2014 and 2015, including many countries in Europe as well as the U.S., Australia, China, Indonesia, Brazil, Philippines, and South Africa, Reichel and Kohont (2017) finds about 50% of firms reported that they have practiced job rotation.

[3] This is also referred to as cross-skilling.

[4] A theory paper by Ortega (2001) on job rotation emphasizes effort motivation as a potential benefit in addition to learning about match quality.

At the same time, the job rotation program also has costs. Providing training is costly to firms and a job rotation program increases those costs by providing training for more than one position. Second, providing additional training for workers may increase their marketability in the broader labor market. This can increase turnover rates in a context where the firm, like most manufacturing firms in developing countries, already struggle with high turnover rates. If the job rotation program increases productivity on the job but also increases turnover, it is not clear if the benefits will outweigh the costs.

We collaborate with a leading garment manufacturer in Asia to examine the impacts of job rotation. The randomized experiment will involve all new workers and recently hired existing workers. In the control group, workers receive the standard approach of assignment and training for one job. This is the pre-existing practice at the firm. In the treatment group, workers will receive training for multiple jobs at the firm. In the treatment group, after the training period ends, workers are asked to express their preferences over jobs and managers are asked to evaluate the performance of workers. The assignment of new hires to permanent jobs is made by incorporating the preferences of both sides using a version of the deferred acceptance algorithm. We examine how the treatment affects performance, employee turnover, and work satisfaction.

There have been very recent papers that have examined the rotation of managers across job types (Fenizia, 2022; Minni, 2023; Cullen and Perez-Truglia, forthcoming) using non-random variation of managers across managerial positions. The empirical evidence on the rotation of workers has documented the types of firms that implement job rotation programs using Danish data (Eriksson and Ortega, 2006). To our knowledge, we are first empirical paper to implement a randomized experiment on job rotation and to focus on the outcomes of workers rather than managers.

Our paper also contributes to a literature that uses a mechanism design approach in the assignment of workers to jobs. Sönmez (2013) introduces a bidding mechanism in matching military school graduates to their desired branches where they can bid for additional years of service for higher priority by location. However, this work does not implement a randomized evaluation. The closest related paper is a study that randomizes the use of a deferred acceptance algorithm for the assignment of army offices to units (Davis et al., 2023). In the context of a developing country, randomized evaluations of incentive mechanisms have been used to assign bureaucrats to job locations (Khan et al., 2019). Our research is the first to combine a mechanism design approach with a randomized evaluation in the context of private sector jobs. This distinction may be quite important as turnover rates in private sector jobs are much higher than in the military and bureaucratic jobs, which may change both the costs and benefits of the matching system.

Our paper also relates to work on the returns to training within firms. In personnel economics, there is a small literature on estimating the returns to training within firms using non-experimental personnel data (Bartel, 1995). Turning to RCTs, Adhvaryu et al. (forthcoming) study the return of the soft and life skill training in garment factories in a large Indian firm, showing substantial returns. De Grip and Sauermann (2012) and Espinosa and Stanton (2023) also use RCTs to estimate the returns to training in firms, focusing on general work training (e.g., time management), and they document important spillover effects of training. Our RCT differs in that we focus on training workers in multiple jobs within the firm. Another distinction of our study is that we have the opportunity to change the permanent job assignments of workers after the additional training. That is, our training treatment differs from those in the literature, both in the nature of the training and via its combination with allocation post-training.

Finally, our paper ties into the literature at the intersection of personnel economics and development economics that uses experimental evidence in lower income countries to show that empowering workers and providing them with more voice over their experience as workers within a firm can lead to better outcomes at both the worker level and the firm level. Cai and Wang (2022) show that randomizing the implementation of a program that gives manufacturing workers in China the ability to rate their supervisors reduced turnover and boosted team productivity. The benefits of worker feedback is also shown by Adhvaryu et al. (2022) where Indian garment factory workers were randomly chosen to participate in a feedback survey after a disappointing wage increase; worker voice in this context reduced turnover. Our paper offers a very different form of worker feedback, focusing on feedback on job assignment, than the other existing papers.

2 Research Design

2.1 Methodological Framework and Identification Strategy

The framework employed in this study is a randomized controlled trial. This design solves the two potential biases that could arise from observational studies. First, there is the potential for selection bias where better or worse workers may be selected for a job rotation program. The selection may be positive if this is seen as a reward or incentive for keeping good workers. Alternatively, the selection may be negative if managers prefer to get rid of bad workers by rotating them through other jobs. In addition, managers may strategically choose which types of positions to offer a worker based on their performance or characteristics. The randomization ensures that any observed changes in outcomes can be attributed to the intervention.

2.2 Intervention

The main intervention in this project involves job rotation and matching. After working in the job that they were initially assigned to by the firm's standard job allocation process, participants in the treatment arm will be randomly assigned to one to two additional available jobs where they will work for one to two weeks.[5] At the end of the full rotation cycle, the managers will evaluate the quality of each worker that rotates through their team and workers will express their preferences over jobs. Thus, for the treatment group, the input of both managers and workers will be considered in the allocation of jobs and workers. More specifically, a deferred acceptance algorithm, used in two-sided matching, will be employed to combine workers' and managers' preferences. The surveys are designed to be incentive compatible. Respondents are reminded that their answers will be used in the assignment process for the permanent job assignment of treatment workers. Control workers will continue to work in the jobs that they were initially assigned to by the firm.

There are eight jobs considered core production activities to the production process of the manufacturing firm, collectively representing approximately 90% of production workers. These eight different jobs exist at the same level within the organizational hierarchy. However, treated workers will not be exposed to all eight jobs. To minimize the impact on the firm's performance and costs, treated workers are rotated to the jobs open for staffing at the time of treatment. This approach, which randomly places treatment workers into available slots, addresses their current staffing issues.

Once both performance evaluations by managers and workers' job preferences are collected, a two-sided matching protocol, specifically the deferred acceptance matching protocol used in the National Resident Matching Program, will be employed to suggest jobs that best fit both the firm and the workers. On the worker side, these preferences are ordered in a ranking across all available jobs for that batch. Managers' weekly evaluations contribute to a comprehensive ranking in each job of all treated workers.[6] Consequently, for the manager side, the matching algorithm will prioritize workers who outperformed the average and de-prioritize those who performed below the average during the rotation. The set of available jobs for treated workers is tailored to include all of the jobs that treated workers rotated through.

The outcomes of the matching process for the permanent job assignments will be presented as recommendations to each factory location. However, there is a possibility of the factory locations deviating from the matching recommendations. Given the robust support from higher-level management, we anticipate that the recommendations will be followed approximately 80% of the time

[5] If a treated worker is assigned one additional job (rather than two), he or she will be performing that job for a duration of one to two weeks, depending on job availability.

[6] Treated workers who did not rotate into a manager's supervision receive a score of 3 on a scale from 1 to 5.

across the various locations.

2.3 Hypotheses

In this study, using a combination of surveys and administrative data, we will examine four primary outcomes and two secondary outcomes. The first primary outcome centers on worker performance metrics. To test whether the rotation program improves the allocation of workers to jobs, we will use two sources of data. One source is data that we gather in our endline survey of managers, seeking feedback from immediate supervisors about the overall performance of each of the workers in our sample. We hope to supplement this with administrative data on individual performance. However, there are some positions for which performance is only recorded by the firm at the team-level, and we are not certain at this stage if the administrative data on performance at the station-level collected from monitoring devices can be linked successfully to individuals yet.

The second set of primary outcomes is employee turnover and absenteeism, which poses significant costs to the firm. The firm estimates that it spends approximately 300 USD per employee for recruitment costs and the initial general training.[7] To contextualize any changes in turnover, we plan to use administrative data that come from the firm's exit interviews. The firm asks reasons for quits, including work-related and personal factors, and we will aggregate this information into a binary outcome, distinguishing between work-related and non-work-related reasons.

The third primary outcome is employee satisfaction. We think there are several mechanisms through which employee satisfaction may increase. First, employees may value getting the additional training and appreciate being able to express their preferences over jobs in the final job allocation mechanism. Second, if the mechanism succeeds in better matches between workers and jobs, we would expect their satisfaction to increase.

Our fourth primary outcome is job preference discovery. Exposing workers to different jobs may impact their preferences over the jobs. This may provide evidence of suboptimal job allocation before the intervention. Additionally, we will test the correlation between preferences and performance, drawing on both performance and preference data from our endline survey, to see whether workers prefer jobs that they are good at.

For a secondary outcome, we will examine whether treated workers temporarily substitute into jobs other than their primary assignment more often than control workers. We are unsure whether this is possible yet in the firm's administrative data but if this is not possible in the administrative data, then our endline survey will ask questions about temporary job substitutions.

[7]This represents over one month of salary for an average worker.

For another secondary outcome, we will study workplace violations. This serves as an indirect measure of unobserved worker effort, although violations are rare events. We will utilize administrative data on violation records.

The hypotheses in this study are structured to systematically test the expected impacts of the rotation program on various outcome variables above.

H1 Worker Performance

- H1a: Treatment workers have better individual performance outcomes relative to control workers due to better matching of workers and jobs.
- H1b: Treatment workers may have worse individual performance outcomes relative to control workers initially due to lower tenure in their permanent position but this gap fades with time.

H2 Employee Turnover and Absenteeism

- H2a: Treatment workers are hypothesized to be less likely to be absent at work and less likely to quit, specifically due to work-related reasons, because they are happier with jobs better aligned to their preferences and/or skills.
- H2b: Treatment workers may be more likely to quit if the broader training that they received allows them to get better jobs at other firms.

H3 Employee Satisfaction
Treatment workers are anticipated to express greater satisfaction with their jobs due to better job matching.

H4 Job Preference Discovery
Treatment workers are hypothesized to be more likely to update their job preferences given that they are given the opportunity to learn more about different jobs.

H5 Job Substitutions
Treatment workers are more likely to temporarily substitute into other positions when other workers are absent or quit because they have some training in these other positions.

H6 Workplace Violations
Treatment workers are less likely to violate the firm's regulations given that they are more satisfied at work.

2.4 Sample Selection and Treatment Assignment

Our sample is based on groups of new hires to several different factory locations of a large garment manufacturing firm in Asia that produces clothing for export. Our partner firm will initially screen new workers to identify those suitable for the program and those who are not. They may choose to exclude certain new hires from the job rotation program, particularly if these workers already have extensive experience in the job they were hired for. From this list of qualified candidates, we will then select workers for the sample before the randomization. While workers may be hired continuously over time, we will group new hires into study batches that are defined as intervals of 3 to 5 weeks.[8] Some batches will be excluded from the rotation program. Given that our rotation schedule is designed with fixed job slots, we require sufficient variability in the jobs. For example, if all new hires in one month are for a single position, rotation across jobs is impossible.

For batches that meet the qualification of having a diversity of positions open such that treated workers can be rotated into one or two other jobs in addition to their initial assignment, we will randomly allocate individuals into the treatment and control groups using a computer program.[9] Individuals in the treatment group are then randomly assigned to a job rotation schedule; the timeline for each batch is shown in Table 4).

We expect high but not perfect compliance from the firm both with the job rotation schedule and with permanent job assignment. Our surveys combined with the administrative data will allow us to check the degree of compliance with both. We are aiming for compliance rates of 80% or higher, and we will show and discuss the compliance rates in the paper. The firm has adopted a decentralized management style where each factory has a certain amount of freedom in their management, as long as the target KPIs are met. While this study has been strongly endorsed by top management, the pressure to meet target output means that, at the factory level, managers may have to address immediate, often manpower-related, problems by deviating from our rotation schedule and matching recommendations.

We minimize non-compliance risks in several ways. First, each factory is informed that activities related to this study will not be counted in their KPIs. For instance, one of their main KPIs is the target ratio of sewers to non-sewers. During the study period, the central planning team will not count missing targets against their KPI if it happens as a result of the job rotation program. Second, our field team will maintain close communication with each location and frequently check monitoring data to ensure that each location respects our rotation schedule and matching recom-

[8] The intervals may vary by factory and time, depending on the speed of hiring. We expect each batch to have about 10-15 workers.

[9] The initial job assignment is done by the firm. Given that batches consist of workers who joined within an interval of 3-5 weeks, some workers may have been performing their initial jobs for 1 week, while others for as long as 4 weeks.

mendations.

2.5 Sample and Statistical Power

We conducted a small pilot study in May 2023 involving 16 workers, each completing a total of two to three rotations. The goal of the pilot was primarily to demonstrate proof of concept to the firm and iron out logistical issues for the firm's implementation of the program. After each job, these workers were evaluated by their managers and participated in a preference survey. Since the primary purpose of the pilot was to work out the complex logistical issues of implementation, we did not do a follow-up survey after the rotation.

The unit of analysis in this project is at the individual level. We aim to recruit between 500 and 1000 new hires for this project. We use numbers from the manager evaluations obtained from the pilot work in May 2023 and made assumptions on the expected effect size. 16 workers were evaluated in up to 4 different jobs on a scale of 1 to 4. We use the overall ratings of the worker in 59 observations to do power calculations below. The average score is 2.712 and the standard deviation is 0.696 (σ). We also assume a compliance rate of 80%.

Table 1: Power Calculation

Effect size	Power	Alpha	Sample size
0.20 $\times\sigma$	0.9	0.05	**1000**
0.29 $\times\sigma$	0.9	0.05	**500**

Note: This power calculation was done using manager evaluation scores from the pilot study done in May 2023. We calculate the sample size needed for minimum detectable size denoted in terms of the standard deviation of manager evaluations.

Given our target sample size, the minimum effect size we should be able to detect in the best-case scenario lies around $1/4$ of the standard deviation. Ideally, we would like to have as many observations as possible to enable power for the heterogeneity analyses. In practice, the partner company incurs costs from this project, both explicitly (such as the wage bills of dedicated personnel) and implicitly (such as the opportunity costs of deviating from the default of job assignments). We maintain that a sample size between 500 and 1000 new hires should provide us with confidence in detecting the effect of the program.

In addition, to address the concern of multiple hypothesis testing, we performed power calculations comparing classic, single-outcome inference to inference accounting for multiple hypothesis testing using the free, step-down procedure of Westfall and Young (1993). We divide our main outcomes into two families. The first family is a family of business outcome variables, containing employee performance measured on a Likert scale and turnover. The second family is non-business outcomes. Our power calculation focuses on our main outcome variables, namely, employee performance on a Likert scale.

To perform this calculation, we simulate data on performance and turnover, assuming different levels of correlation. Specifically, we simulate data from two standard normal distributions under a correlation of -0.2 or -0.4, which we refer to as weak or strong correlation.

Under classic, single-outcome inference, for a sample size of 500, standard deviation of 0.696, power 0.9 and significance level of 0.05, the minimum detectable effect is 0.202 points (on a 1-5 Likert scale). We believe that an effect of about 0.2 points is quite reasonable. This is 29% of the standard deviation. Under inference corrected for Westfall-Young multiple hypothesis testing, for the effect of 0.202 points, the power is 0.815 under weaker correlation and 0.847 under stronger correlation.

3 Data

3.1 Data Collection

The data for this study will include a combination of surveys and administrative data. Prior to the commencement of each job rotation cycle, participants will complete a short baseline survey as well as an aptitude test called the General Aptitude Test Battery that has been shown to be correlated with the performance of garment manufacturing workers in both developed and developing countries settings (Dagenais (1990), Dolke (2022)). This survey will include general demographic information, work experience, as well as preferences regarding the various job positions within the firm.[10]

While new workers can join the firm continuously, they will be batched into job rotation groups on the basis of the time that they join the firm where we will target a batch to be defined by arrivals over the course of a month. At the end of each round of rotation, we will survey immediate supervisors of the treated workers. In this survey, managers will be asked to assess the performance of treated workers who have rotated to their teams in an incentive-compatible manner.[11] The

[10]The baseline survey instrument is in Appendix A.2.
[11]The survey is in Appendix A.3.

evaluations collected in these surveys will be used in the algorithm that determines the permanent matches.

Given that treated workers will go through a maximum of three jobs, we will have up to three manager evaluations for each treated worker. Upon the conclusion of the rotation cycle, all study participants (both treatment and control workers) will participate in our midline worker survey.[12] This survey collects information on job preferences from participants after the rotation cycle. In combination with the assessments by managers, this data is used in the algorithm that produces the final job matches.

We anticipate wrapping up the intervention by summer 2024, aiming to achieve a sample size ranging from 500 to 1,000 workers by that time (see Table 5 for the study timeline). The ultimate sample size and end date will depend on the turnover and hiring rates of our partner firm over this period. Thus, there is some uncertainty in the final sample size and when exactly the intervention will end. Our estimates for the sample size and timeline are based on past turnover and hiring rates. In this estimated timeline, the average exposure period will be around seven months, with a range spanning from three to eleven months.[13]

After the conclusion of the last job rotation cycle, we will begin conducting our endline surveys for both workers and managers. The worker survey will focus on gathering feedback regarding their satisfaction with their job, information about substituting into other jobs, and their experience with job rotation. The manager survey will center on collecting information related to worker performance.

With the exception of the short baseline survey and aptitude test, which are administered by the firm, we have trained enumerators collecting the other rounds of surveys on our behalf.

For the administrative data, we will get access to the firm's personnel records, which include demographic information, salary disbursals, jobs, attendance data, quits, and reasons for quitting. Additionally, we will leverage the workplace violation records, which store information regarding the description of the violations, date of the violations, and the corresponding punishments. We will also use performance data, both at the individual level and team level. If they have administrative data on temporary substitutions, then we can use this.

[12] The survey is in Appendix A.4.

[13] As individuals in the rotation program try other positions, there may initially be a decline in performance compared to the control group because their tenure in the position is shorter. Through discussions with our partner organization, we expect a convergence in the gradient of tenure between the treatment and control group by the 3 month mark.

3.2 Initial Data

We launched the project in August 2023. Thus, we have some initial, incoming data to present (up through early November 2023). So far, 17 people were randomized into the control group and 18 into treatment group. Following the rotation for the treatment group, the matching algorithm recommended a job that was different than their initial assignment for 7 out of the 15 workers.[14] Notably, one worker transitioned from a job she least preferred in her initial assignment by the firm to a job she most preferred.

Table 2: Summary Statistics of the Baseline Data

	Control			Treatment			
	Mean	SD	N	Mean	SD	N	p-value
Age	27.76	7.58	17	28.89	7.80	18	0.67
Male	0.35	0.49	17	0.33	0.49	18	0.91
Native Language 1	0.06	0.24	17	0.11	0.32	18	0.59
Native Language 2	0.82	0.39	17	0.72	0.46	18	0.49
Native Language 3	0.12	0.33	17	0.17	0.38	18	0.69
Secondary Schooling or higher	0.76	0.44	17	0.78	0.43	18	0.93
Prior Experience in Garment Industry	0.35	0.49	17	0.28	0.46	18	0.64
Has Family/Friends at Company	0.59	0.51	17	0.78	0.43	18	0.24
Length of Time in City	0.65	0.49	17	0.67	0.49	18	0.91
Skill Level Sewing	1.59	0.87	17	1.44	0.92	18	0.64
Aptitude Score	82.18	17.71	17	88.44	15.07	18	0.27

Notes: The p-value is on the test of whether the treatment and control means are statistically different from each other. *Length of Time in City* is a binary indicator equal to 1 if respondent lived in the city for at least 10 years. *Skill Level Sewing* measures respondent's subjective evaluation of their own skills in Sewing. The response scale is from 1 (no skills) to 4 (advanced skills). *Aptitude Score* measures respondent's performance on a baseline test involving arithmetic reasoning, 3D space, name comparison and object matching problems.

Table 2 presents some information about the incoming data. The average new hire in the sample is in their late-20s. About two-thirds of the workers are female. About three-quarters of the sample have a secondary school degree or higher. While most new workers speak the native language (Native Language 2), there are also new hires who are migrants as indicated by their native language being other languages. Over 70% of the workers have never worked in the garment industry at all before starting at this firm. About half of the sample does have a friend or family member also working at the firm. Most workers have lived in the local city for over 10 years. The most common job is as a sewer. However, the average incoming sewing skill level is quite low (averaging between no skills and basic sewing skills).

At baseline, the new workers are also asked to express their preferences over each of the 8 core jobs of the production process at the garment manufacturing firm on a scale from 1 to 5 where

[14] Three treatment workers quit so we are left with 15 workers in the treatment group.

1 means they strongly dislike the job and 5 means they strongly prefer the job. A value of 3 means neutral or no preference. Given that most of these workers have no prior experience in garment manufacturing and are answering the survey during the orientation period prior to actually working in specific job, it is not surprising that most people are stating that they have no preference and there is little variation across positions. In addition to their preferences, we are interested in whether people like jobs that they are good at or if there may be a trade-off in terms of jobs that are liked versus what they are good at. We also ask them about how well they predict they will perform in each of the 8 core jobs on a scale from 1 to 5 where 1 corresponds to unsatisfactory and 5 corresponds to well ahead of standard. The middle value of 3 corresponds to them predicting that they would be satisfactory in the position.

Table 3: Summary Statistics for Baseline Preferences and Expected Ability

	Preference			Expected Ability		
	Control Mean	Treatment Mean	p-value	Control Mean	Treatment Mean	p-value
Cutting	3.18	3.11	0.83	2.59	3.00	0.26
	(0.73)	(1.08)		(1.00)	(1.14)	
Sewing	3.53	3.39	0.71	3.00	3.06	0.89
	(0.94)	(1.24)		(1.22)	(1.21)	
Embroidery	2.94	3.06	0.73	2.53	2.72	0.57
	(1.03)	(0.94)		(1.18)	(0.75)	
Heat Transfer	3.06	3.06	0.99	2.47	2.89	0.24
	(0.97)	(0.73)		(1.12)	(0.90)	
Quality Control	3.00	3.00	1.00	3.06	2.89	0.63
	(0.94)	(0.91)		(1.09)	(0.96)	
Warehouse/Logistic	2.94	3.00	0.85	2.59	2.83	0.51
	(0.97)	(0.84)		(1.12)	(1.04)	
Material/Stock	3.53	3.06	0.20	3.24	2.94	0.51
	(1.12)	(1.00)		(1.25)	(1.30)	
Ironing/Packing	3.06	3.33	0.46	2.82	2.78	0.90
	(1.09)	(1.08)		(1.19)	(1.00)	
N	17	18	35	17	18	35

Notes: The p-value is on the test of whether the treatment and control means are statistically different from each other. In columns 1 and 2, each outcome is based on a question about the respondent's preference for different jobs. The response scale ranges from 1 (strongly dislike this job) to 5 (strongly prefer this job). In columns 4 and 5, each outcome is based on a question about the respondent's expectation of performance in each job. The response scale ranges from 1 (unsatisfactory) to 5 (well ahead of standard). Columns 3 and 6 report the p-values of a t-test comparing the means of treatment and control group.

4 Analysis

4.1 Statistical Model

The main analysis will include regressions that take the following form:

$$Y_i = \alpha + \beta Treat_i + X_i'\gamma + e_i, \tag{1}$$

where Y_i is an outcome of worker i, $Treat_i$ is whether worker i was assigned to treatment and X_i includes control variables such as fixed effects for each hiring batch. If there is imbalance in any baseline characteristics between the treatment and control groups, we can include those variables as additional controls. For outcomes that are collected via survey, we will also include enumerator fixed effects. e_i is an error term. The standard errors will be clustered at the batch level. The key individual-level outcome variables include worker performance metrics, total number of absences, employee satisfaction, worker preference changes across various jobs, substitution rates, violations and quits.

While the treatment occurs at the individual level, we are also interested in looking at team-level outcomes, including team performance metrics. This is especially important given that some positions do not have performance metrics at the individual level but only at the team level. At the team-level, we can look at the impact of the entry of a new team member who is in the treatment versus control group. We can then use the administrative data to look at the periods before the entry of the new worker and the periods after by treatment and control.

If we are able to get a panel of the administrative data, we can also examine the time series dimension of the data. For example, we could estimate hazard rates of quits for the treatment and control groups over time rather than just estimating equation 1.

We will address concerns about multiple hypothesis testing using Westafall-Young corrected p-values. We will do this both to address multiplicity of outcomes and for examining treatment effect heterogeneity.

4.2 Potential Threats

One potential threat to the analysis is if there are spillover effects on the control group workers. For example, one possibility is that the control group workers are upset that they were not given the opportunity to participate in the rotation program or to express preferences over their job assignment if they learn that others are getting the opportunity to do this. We can test this by looking at the relationship between the outcomes of control group workers and the presence of treated workers on their team.

Alternatively, we can look at whether there is evidence that the control group is losing motivation as a result of the intervention by looking at whether absences in the control group increase after the treatment group begins the rotation program. If this shows up in the data, we can compare that to earlier waves of new hires and their time pattern over the same weeks of tenure.

We expect attrition from the sample as workers quit and intend to measure quits as a key outcome of interest.

While managers are motivated to evaluate workers based on their performance as the manager's incentive pay is linked to team-level productivity, we may still be concerned that there is strategic behavior on the part of workers and/or managers in the rankings that they report for the deferred acceptance algorithm. In particular, people may try to create assignments based on pre-existing social networks. The firm does not allow us to use or ask for information about full names, so we cannot ask who exactly they knew prior to joining the firm. However, we can look at whether having similar characteristics to the manager, including sharing the same hometown and gender, matters for rankings used in the matching algorithm.

4.3 Heterogeneous Effects

We will conduct heterogeneity analysis along the following dimensions related to work and experience: prior experience in the garment industry and the number of jobs in the rotation batch. The primary goal of the intervention is to expose workers to various job roles. This will facilitate learning about their preferences and comparative advantage. Workers with limited experience in the garment industry may experience greater gains from rotation. Additionally, the diversity of job options within a rotation batch is a key factor. Batches with a greater number of job options provide workers with increased chances of being matched to a role that aligns with their preferences. Consequently, we anticipate that the number of jobs within a batch could play an important role in the treatment effects, influencing the outcomes based on the availability of diverse job options in each rotation group.

We will also look at heterogenous treatment effects by demographic characteristics including age, education, gender, migrant status, whether they have a friend or family member already working at the firm and their test score. These are all variables that we collect in our baseline survey. We expect that the job rotation may have stronger effects for people who come into this new job knowing less about their comparative advantage. Thus, we might expect stronger effects for young people, people with lower education, and recent migrants to the city.[15]

[15]This builds on the idea that migrants may have fewer social networks to provide information about specific jobs, and

At baseline, we also ask survey questions about people's preferences over the core jobs and their predictions about how well they will perform in them. We would like to use these data to look at heterogeneous effects. For example, are there larger effects for people who update their preferences the most and predictions about their performance the most after the rotation program? Or for workers for whom the gap after job rotation between their initial job assignment by the firm and their job assignment after the algorithm is the largest?

Finally, we would like to look at heterogeniety by performance in their initial job assignment (using the standard assignment process by the firm). Does job rotation help those workers more who were initially doing poorly or well more?

To guide our heterogeneity analysis, we will also implement a data-driven approach such as machine-learning methods from Chernozhukov et al. (2020).

5 Interpreting Results

5.1 Individual-Level Outcomes

There are two primary channels through which we hypothesize that the job rotation program can change worker and firm outcomes. First, we expect the job rotation and permanent job assignment program to lead to better matches between treated workers and within firm job. Second, we expect that the cross-skilling of workers will allow the firm additional flexibility in response to quits and absences in assigning treated workers to substitute into jobs that they have some experience in during the job rotation.

Given that we hypothesize our treatment will lead to better job matches, we expect higher rates of job satisfaction, and lower turnover rates for treated workers relative to control workers. We are also interested in whether treated workers have higher outcomes in terms of employee performance, including absences rates and earnings.[16] We will have individual performance assessments reported by managers in the endline survey.[17]

For the impacts on cross-skilling, we will examine whether the treated workers are more likely to substitute temporarily into other jobs relative to the control workers.

We can also characterize what defines better job matches. For example, are workers happier on

that more schooling, including vocational education, may expose workers to some of these jobs.

[16]Variation month-to-month in earnings can reflect absence rates and over-time hours. Depending on the position, it can also reflect team-level performance bonuses.

[17]For some factories and positions, we may also have administrative data at the individual level. There is station-level administrative perofrmance data, but it is unclear to us right now if we will be able to match the station to the individual.

teams where other workers are more similar to them in terms of gender, age and ethnicity. Or are the characteristics of the managers more important than those of the co-workers? On that vein, is it better for new hires to have managers who are more similar to them?

To explore the possibility that the jobs may have non-pecuniary differences, in the endline survey, we will ask workers to assess these non-pecuniary aspects of their current job including questions about their relationship with their teammates and supervisor, and their assessment of safety, health hazards, opportunities for promotion, and how much their work relies on others.

5.2 Team-Level Outcomes

In addition to the individual-level outcomes, our analysis will also look at outcomes at the team-level. The individual-level randomization will produce variation across teams in whether a team has any treated workers or any control workers. Thus, we can look at whether teams are getting new workers and whether that new worker is a treated worker or a control worker.

This will allow us to see whether better job matches at the individual-level map into team-level outcomes. For example, if there is lower turnover among treated workers, does that mean that teams that received new treated workers have better outcomes in terms of productivity than teams that received new control workers? Similarly, if there are lower absence rates or higher individual-level productivity outcomes among treated workers, do we see better productivity outcomes manifest at the team-level as well?

If the channel at the individual level is not about better job matches but about cross-skilling effects, it is actually possible that the team-level outcomes for teams that have treated workers are actually lower than teams with control workers if the treated workers are more likely to be pulled out of their team to help substitute into other teams in response to quits and absences on other teams. Thus, the aggregate productivity of the firm may be higher but at a cost to the teams of treated workers.

5.3 Understanding Labor Misallocation

If we are able to show substantial labor misallocation by the firm in the absence of our intervention, there are several key follow-up questions that will guide the policy implications of this research. First, we would like to quantify the amount of misallocation of labor and compare it to other estimates that exist on the misallocation of resources across firms in developing and developed countries. We can first measure the impact of job rotation on misallocation by looking at the impact of the treatment on individual performance. However, given that there may be sorting into jobs that are more or less individualized, we would also want to look at having more or less treated workers on team-level production outcomes. The mechanism for team-level effects may operate through individual productivity or through differences in quit rates between treated and control workers.

Second, does the within-firm misallocation of labor naturally solve itself (in the absence of our intervention) over time? The hypothesis here is that the firm can make mistakes in the initial assignment of workers to positions within the firm, but this may or may not be consequential over time if managers and workers shift workers across jobs over time. Thus, the sorting mechanism that finds the best matches between workers and jobs may not occur at the initial job assignment stage but over time in job switches that occur later. We can examine over time whether job matches for the control group improve through subsequent switches.

Third, if we find labor misallocation by the firm, it would be useful to quantify the costs of the job rotation program relative to the benefits, and compare that to the costs and benefits of their existing assignment system. This depends on our ability to get profit data from the firm, and it is not yet clear if they will be willing to share this data. This acknowledges that while our job rotation program may create better matches and positive benefits, there are also costs to the firm of the job rotation. Specifically, workers spend time training on multiple jobs and may have lower rates of productivity during their various training periods.

Finally, if we find that the answer to the second question is that the misallocation does not resolve over time in the absence of our intervention and the answer to the third question is that the cost-benefit analysis suggests that job rotation program produces positive returns to the firm, why does this misallocation of labor occur and persist given that the firm's incentives are to maximize profits and should be doing their best to assign workers to the correct jobs? Can we say something about the barriers that exist that prevent the firm itself from realizing the best outcomes? This is a difficult question to answer. One potential approach is to talk to managers and human resources about their reactions to our findings and whether they have an explanation for why their existing allocation of workers to positions appears suboptimal to us.[18]

[18] For example, while Mas and Moretti (2009) find that there are strong productivity spillovers to having highly productivity workers on a shift, the firm was unwilling to optimize the assignment of shifts because shift choice is a highly valued benefit of workers.

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A Appendix - Study Instruments

A.1 Timeline

TABLE 4 Timeline for each batch

Week (t - n) to Week (t - 1)	New workers join, and baseline surveys along with AP tests are collected.
Monday of Week 0	Rotation schedules are created and communicated to each factory.
Friday of Week 0	1st Manager Survey (for job 1)
Week 1	Rotation 1 begins
Friday of Week 1	2nd Manager Survey (for job 2)
Week 2	Rotation 2 begins
Friday of Week 2	3rd Manager Survey (for job 3) and Midline Worker survey
Week 3	Matching recommendations are communicated
Week 4	Permanent matching starts

This table provides a timeline for each batch, outlining key events and activities. The process repeats after Week 4.

TABLE 5 Study Timeline

May 2023	Exploratory pilot.
August 12, 2023 - August 13, 2023	Kick off meeting with the management.
October 2023	First Rotation starts
Aug 2023 - April 2024	Data Collection on AP-test, baseline, midline
June 2024	Intervention ends
October 2024	Endline Survey begins
December 2024	Data collection concludes

This table provides a timeline for this study, outlining key events and activities.

A.2 Baseline Survey

SURVEY

DATE (D/M/Y): ____ / ____ / ____

1 WHAT IS YOUR FULL NAME? ______________________________

2 WHAT IS YOUR EMPLOYEE ID? ______________________________

3 WHAT IS YOUR PHONE NUMBER? (Optional) ______________________________

BELOW ARE SHORT DESCRIPTIONS OF SOME OF THE MAIN JOBS AT [redacted].

CUTTING: Precisely cutting fabrics into garment pieces
SEWER: Stitching together garment pieces on an assembly line
EMBROIDERY: Adding logos or embellishments on garments using automated machines
HEAT TRANSFER: Applying decorative designs onto garments using a heat machine
QUALITY CONTROL: Ensuring garments meet the set standard for quality through careful checks
IRONER/PACKER: Ironing, folding, and packing finished goods in bags for delivery
WAREHOUSE/LOGISTIC: Delivering and arranging raw materials and finished goods in the factory
MATERIAL/STOCK: Receiving inventory, shelfing raw materials and finished goods

4 WE WOULD LIKE TO ASK YOU ABOUT YOUR PREFERENCES OVER A SET OF JOB FUNCTIONS. HOW WOULD YOU RATE YOUR PREFERENCES OVER DOING EACH OF THE FOLLOWING JOBS?
<u>For each row</u>, please place an X in the correct box.

	STRONGLY PREFER THIS JOB	SOMEWHAT PREFER THIS JOB	NEUTRAL OR NO PREFERENCE	SOMEWHAT DISLIKE THIS JOB	STRONGLY DISLIKE THIS JOB
CUTTING	☐	☐	☐	☐	☐
SEWER	☐	☐	☐	☐	☐
EMBROIDERY	☐	☐	☐	☐	☐
HEAT TRANSFER	☐	☐	☐	☐	☐
QUALITY CONTROL	☐	☐	☐	☐	☐
IRONER/PACKER	☐	☐	☐	☐	☐
WAREHOUSE/LOGISTIC	☐	☐	☐	☐	☐
MATERIAL/STOCK	☐	☐	☐	☐	☐

5 NOW WE WANT TO UNDERSTAND HOW GOOD YOU THINK YOU WILL BE FOR THE SET OF JOB FUNCTIONS. HOW WOULD YOU PREDICT YOUR PERFORMANCE OVER EACH OF THE FOLLOWING?
<u>For each row</u>, please place an X in the correct box.

	UN-SATISFACTORY	LESS THAN SATISFACTORY	SATISFACTORY	MORE THAN SATISFACTORY	WELL AHEAD OF STANDARD
CUTTING	☐	☐	☐	☐	☐
SEWER	☐	☐	☐	☐	☐
EMBROIDERY	☐	☐	☐	☐	☐
HEAT TRANSFER	☐	☐	☐	☐	☐
QUALITY CONTROL	☐	☐	☐	☐	☐
IRONER/PACKER	☐	☐	☐	☐	☐
WAREHOUSE/LOGISTIC	☐	☐	☐	☐	☐
MATERIAL/STOCK	☐	☐	☐	☐	☐

SURVEY

6 WHAT IS YOUR AGE IN YEARS? ☐☐ years

7 WHAT IS YOUR GENDER?
Please place an X in the correct box.
☐ MALE
☐ FEMALE

8 WHAT IS THE HIGHEST LEVEL OF SCHOOLING YOU HAVE COMPLETED?
Please place an X in the correct box below.
☐ LESS THAN PRIMARY SCHOOL
☐ PRIMARY SCHOOL
☐ SECONDARY SCHOOL
☐ POST-SECONDARY SCHOOL

9 WHAT IS YOUR NATIVE LANGUAGE?
Please place an X in the correct box below.
☐ THAI ☐ VIETNAMESE
☐ KHMER ☐ LAO
☐ BURMESE ☐ OTHER
(Please Specify:________________)

10 PLEASE SELECT ANY OTHER LANGUAGES YOU CAN SPEAK SOMEWHAT WELL.
Please place an X in all the boxes that apply.
☐ THAI ☐ VIETNAMESE
☐ KHMER ☐ LAO
☐ BURMESE ☐ OTHER
(Please Specify:________________)

11 DO YOU HAVE PRIOR EXPERIENCE WORKING IN A GARMENT FACTORY?
Please place an X in the correct box.
☐ YES
☐ NO

11a IF YES TO #11, PLEASE INDICATE IF YOU HAVE EXPERIENCE IN THE FOLLOWING POSITIONS:
Please place an X in all the boxes that apply.
☐ SEWER ☐ CUTTING ☐ HEAT TRANSFER ☐ EMBROIDERY
☐ IRONER/PACKER ☐ QUALITY CONTROL ☐ MATERIAL/STOCK ☐ WAREHOUSE/LOGISTIC

12 HOW MANY YEARS OF PRIOR EXPERIENCE DO YOU HAVE WORKING IN A GARMENT FACTORY? ☐☐ years

13 WHAT IS YOUR CURRENT SKILL LEVEL IN SEWING?
Please place an X in the correct box.
☐ NONE
☐ BASIC
☐ INTERMEDIATE
☐ ADVANCED

14 WHAT JOB FUNCTION DO YOU EXPECT TO BE ASSIGNED TO AT [redacted]?
Please place an X in the correct box.
☐ SEWER ☐ CUTTING ☐ HEAT TRANSFER ☐ EMBROIDERY ☐ DO NOT KNOW
☐ IRONER/PACKER ☐ QUALITY CONTROL ☐ MATERIAL/STOCK ☐ WAREHOUSE/LOGISTIC ☐ OTHER
(Please Specify: ________________)

15 HOW LONG HAVE YOU LIVED IN THIS CITY?
Please place an X in the correct box.
☐ 1 YEAR OR LESS
☐ 2-4 YEARS
☐ 5-10 YEARS
☐ OVER 10 YEARS
☐ WHOLE LIFE

16 DO YOU HAVE ANY FRIENDS OR FAMILY MEMBERS WHO ARE CURRENTLY WORKING AT [redacted]?
Please place an X in the correct box.
☐ YES
☐ NO

A.3 Manager Weekly Evaluation Survey

Weekly Manager Survey
(Use only in case of Software issues)

DATE (D/M/Y)…………………………………………………..

Location …………………………………………………………………………..

Issue with ………………………………………………………………

0. [For enumerator only:] Enumerator Name ………………………………………………………..

1. [For enumerator only:] What factory are these calls coordinated with? (please circle the correct factory)
   [redacted]

Hello, my name is ____, and I am here on behalf of a research team. The purpose of this meeting is to collect information on the performance of individuals on your team. The interview will take about 10 minutes. All information obtained will remain confidential.

2. [For enumerator only:] What department are these calls coordinated with?
   a. Sewing
   b. Heat Transfer
   c. Embroidery
   d. Cutting
   e. Quality Control
   f. Ironing/Packing
   g. Warehouse/Logistic
   h. Material/Stock

3. [For enumerator only:] Batch ID ……………………………………………

Weekly Manager Survey
(Use only in case of Software issues)

4. [For enumerator only:] Week ....

5. [For enumerator only:] What is your employee ID?

   ....

6. What is your name?

....

7. What are the name of new hires who joined your team last week?

Worker Names	EID

8. We are collecting your assessments of workers after each rotation while their work is fresh in your mind and may use these in permanent assignments of new workers after all the rotations are finished. You are more likely to get workers that you evaluate higher, and less likely to get workers that you evaluate lower.

   For each of the following workers, please evaluate the overall performance of each worker. You can tell us by giving them one of 5 ratings: Well ahead of standard, more than satisfactory, satisfactory, less than satisfactory, and unsatisfactory.

Worker names	Ratings (enter only the numbers)

Weekly Manager Survey
(Use only in case of Software issues)

Well ahead of standard (1)
More than satisfactory (2)
Satisfactory (3)
Less than satisfactory (4)
Unsatisfactory (5)
Don't Know

9. [For enumerator only] Was the survey successfully completed?

    Yes No

10. [For enumerator only] [If no to 8]Why was the survey not completed?
    a. Technical problem
    b. Could not reach respondent.
    c. Respondent did not have time.
    d. Respondent does not manage the relevant teams anymore
    e. Other → please specify

    ....

11. [For enumerator only] This is the end of the survey. Do you have any final comments or notes about the interview?
    ....
    ....
    ....
    ....
    ....
    ....
    ....
    ....

A.4 Worker Post-Rotation Survey

WORKER MIDLINE SURVEY
(Use only in case of Software issues)

DATE (D/M/Y)..

Location ....

Issue with ....

0. [For enumerator only:] Enumerator Name ....

1. [For enumerator only:] What factory are these calls coordinated with? (please circle the correct factory)

[redacted]

2. [For enumerator only:]Batch ID ....

Hello, my name is .... and I am here on behalf of a research team. The purpose of this meeting is to learn about your preferences over different job functions. The interview will take about 5-10 minutes. All information obtained will remain confidential.

3. What is your name? ....
4. What is your employee ID number? ....
5. Have you worked in more than one job function since starting at [redacted]? (Please circle the correct answer)

   YES NO (If NO -> SKIP TO Q.7)

6. Please tell us all of the job functions you have tried since you began working at [redacted]?

WORKER MIDLINE SURVEY

(Use only in case of Software issues)

JOB 1	
JOB 2	
JOB 3	
JOB 4	
JOB 5	

[For Enumerator at question 7: you can use or give the following descriptions if the respondent needs
Cutting: Precisely cutting fabrics into garment pieces
Sewer: Stitching together garments pieces on an assembly line
Embroidery: Adding logos or embellishments on garments using automated machines
Heat Transfer: Applying decorative designs onto garments using a heat machine
Quality Control: Ensuring garments meet the set standard for quality through careful checks
Ironer/Packer: Ironing, folding and packing finished goods in bags for delivery
Warehouse/Logistic: Delivering and arranging raw materials and finished goods in the factory
Material/Stock: Receiving, inventory, shelfing raw materials and finished goods.]

7. We will ask you about your preferences regarding the 8 different job types that are available at [redacted]. This may affect the position that you are permanently assigned to at [redacted] in the near future. You are more likely to get assigned to a permanent job that you rank closest to 1, and less likely to get a job that you rank closer to 8. So it's in your best interest to be honest in your evaluation.

Sewer ....

Ironer/Packer ....

Quality Control ....

Heat transfer ....

Cutting ....

Embroidery ....

Warehouse/Logistic ....

Material/Stock ....

WORKER MIDLINE SURVEY
(Use only in case of Software issues)

7. [For enumerator only] Was the survey successfully completed?

    Yes No

8. [For enumerator only] [If no to 8]Why was the survey not completed?
    a. Technical problems
    b. Could not reach respondent.
    c. Respondent did not have time.
    d. Respondent does not work at company any more
    e. Other → please specify

    ………………………………………………………………………………………………

9. [For enumerator only] This is the end of the survey. Do you have any final comments or notes about the interview?
    ……
    ……
    ……
    ……
    ……
    ……
    ……
    ……

B Administrative information

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