The Skills to Pay the Bills:

Returns to On-the-job Soft Skills Training*

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Abstract

In imperfectly competitive labor markets, the rents from general training are divided between workers and firms, making training investments potentially profitable for firms. We test this theory via a randomized evaluation of soft skills training for female garment workers in India. The program increased women's extraversion and communication, and spurred skill upgrading. Treated workers were 12 percent more productive than controls post-program, yet wages rise very modestly with treatment (by 0.5 percent), with no differential turnover, suggesting substantial labor market frictions. Consistent with these results, the net return to the firm was 256 percent nine months after the program.

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1 Introduction

Does it pay for firms to provide general training to workers? Firms' returns depend on the productivity gains from training as well as the likelihood that workers, once they achieve greater productivity, choose to stay at the firm. In perfectly competitive markets, workers' wages would need to increase commensurate to their marginal products; any firm that paid below marginal product would lose the newly trained workers as they received higher wage offers at other firms. As Becker (1964) famously noted, this implies that with perfect labor markets, even general training programs that generate large productivity returns may not be appealing investments for firms.

This result, however, appears at odds with the fact that in many markets, firms do indeed provide general training to workers without accompanying wage reductions (e.g., Bassanini et al. (2007)). Theoretical work has emphasized the potential role of asymmetric information and search frictions in generating a wedge between workers? marginal products and their wages in labor market equilibrium (Acemoglu, 1997; Acemoglu and Pischke, 1998, 1999; Autor, 2001; Chang and Wang, 1996; Katz and Ziderman, 1990). If this situation, the productivity rents from general training are divided between workers and firm, creating value to investments in training for firms.

Quantifying the impacts of general training – on productivity, wages, and turnover – is thus of paramount importance to understanding the structure of labor markets. In this paper, we evaluate the workplace impacts of a general training program focusing on soft skills development. There is emerging consensus on the importance of soft skills – for example, the effective allocation of time and money, teamwork, leadership, relationship management, and acquiring and assimilating information – for labor market success (Bassi et al., 2017; Deming, 2015; Groh et al., 2015; Guerra et al., 2014; Heckman and Kautz, 2012; Heckman et al., 2006). Global surveys of employers corroborate that these skills are in high demand (Cunningham and Villaseñor, 2016).

Recent studies demonstrate that it is possible to inculcate soft skills in early childhood and, to an extent, in early adolescence (Attanasio et al., 2014; Gertler et al., 2014; Grantham-McGregor et al., 1991; Ibarrarán et al., 2015). But how malleable these skills are in adulthood is an open question. Structural estimates of dynamic human capital accumulation models suggest that it may be very difficult to affect the stock of skills at later ages, particularly for those with low baseline stocks, due to dynamic complementarities (Aizer and Cunha, 2012; Cunha et al., 2010; Heckman and Mosso, 2014).

Yet the need for trained workers – in terms of both hard (technical) and soft skills – has never been greater, especially in low-income countries, where industrial growth has far outstripped growth in the supply of skilled labor (Cunningham and Villaseñor, 2016; Hanushek, 2013). On-the-job training has always played a crucial role in building workforce human capital (Becker, 1964). In countries with dysfunctional education systems and low public sector capacity, it is all the more true that skilling takes place within the firm (Tan et al., 2016).

The questions that motivate the present study, then, are threefold. First, is it possible to improve soft skills meaningfully for adults with low stocks of these skills? Second, if skills do improve, what are the workplace consequences, including impacts on productivity, wages, and retention? Finally does it pay for firms to impart this form of general training to their workers, and what can we learn about the extent of labor market frictions in this context?

To answer these questions, we partnered with the largest ready-made garment export firm in India to evaluate an intensive, workplace-based soft skills training program. The initiative, the Personal Advancement and Career Enhancement (P.A.C.E.) program, aims to empower female garment workers (FGWs) via training in a broad variety of life skills, including modules on communication, time management, financial literacy, successful task execution, and problem-solving. We conducted a randomized controlled trial (RCT) in five garment factories in Bengaluru, a large city in southern India. We assessed the impacts of soft skills training on 1) measures of the stock of these skills via surveys; and 2) administrative data on retention, productivity, wages, task complexity, and other workplace outcomes. Finally, we compute the firm's returns, combining our point estimates with data on the program's costs and the firm's accounting profits.

We used a two-stage randomization procedure. We enrolled female garment workers (FGWs) in a lottery for the chance to take part in the P.A.C.E. program. In the first stage, we randomized production lines to treatment. In the second stage, within treatment lines, we randomized workers who had enrolled in the lottery to either direct P.A.C.E. training or spillover treatment. We thus estimate treatment effects by comparing trained workers (on treatment lines) to control workers on control lines (who enrolled in the lottery but whose lines were assigned to control). We estimate spillovers by comparing untrained workers on treatment lines to control workers on control lines.

Direct impacts on workplace outcomes, measured using the firm's administrative data, are consistent with the acquisition of soft skills by workers. Treated workers are more 11-12 percent productive and more likely to be assigned to complex tasks. Impacts last up to 9 months after program completion (when we ceased data collection), suggesting that learned skills translated into persistent improvements in workplace outcomes. Workers on treatment lines who did not receive the program are also more productive and are assigned to more complex operations, generating team-level (production line) impacts on productivity post-program completion. Wages went up very slightly as a result of treatment: an increase of about 0.5 percent. The program had no sustained impact on turnover. Retention was actually higher in the treatment group relative to control during the program period; this effect diminished after program completion.[1]

Finally, we combine our point estimates of impacts on workplace outcomes with program cost and accounting profit data to calculate the costs and benefits of the program to the firm. The net rate of return was positive by the end of the program period (12%). Nine months after program completion, fueled by post-program increases in productivity, the return climbed to nearly 250%. These large returns are rationalized by the relatively low costs of the program combined with the accumulated effects on productivity and person days, and are consistent with other recent interventions in garment factories in South Asia (Menzel, 2015).

The weight of the evidence we present suggests that the primary mechanism for improvements in workplace outcomes was improvement in the stock of soft skills. In particular, treated women showed a pronounced increase in extraversion, which impacts productivity via improvements in women's ability to communicate and collaboratively solve issues with their peers and supervisors. These women were also more likely to request and complete skill upgrading trainings, generating complementary improvements in "hard" skills. Survey results indicate greater self-assessment of workplace quality (relative to production line peers), consistent with an increase in self-regard. Finally, pre/post data from assessment tools designed to measure learning in each of the program's modules show that treated workers significantly improved their stocks of knowledge in each one of the program's target areas. Taken in sum, we interpret the results to indicate that the program increased workers' stocks of soft skills, which in turn led to productivity improvements.[2]

Our study serves as a proving ground for influential theories regarding firms' provision of general training to workers. Broadly consistent with the predictions of Acemoglu (1997); Acemoglu and Pischke (1998, 1999); Autor (2001) (and against the hypothesis of Becker (1964)), we find that 1) general training does not increase worker attrition, despite high overall turnover (indeed, during the program,

[1]We use a dynamic inverse probability weighting procedure, described in detail in section 4, throughout our analysis to correct for potential changes in the size and composition of the treatment and control groups over time.

[2]We deal with several other possible mechanisms in section 6, including potential changes mental and physical health, reciprocity motive, and social capital.

workers are *more* likely to remain with the firm); and 2) the increase in wages for treated workers is not commensurate with the productivity response (wage increases by 0.5 percent, while productivity increases by 11-12 percent). This set of results suggests that labor market frictions – search costs or information asymmetry or both – are likely substantial in our setting.

Previous work quantifying the productivity impacts of on-the-job training generally uses observational data on firms and workers in the United States and Western Europe (Barrett and O'Connell, 2001; Barron et al., 1999; Dearden et al., 2006; Konings and Vanormelingen, 2015; Mincer, 1962). These studies tend to find that training increases productivity, but there is disagreement on the magnitude of this increase (Blundell et al., 1999). Specifically, when endogeneity of training is credibly accounted for (e.g., using matching methods), productivity returns become quite small (Goux and Maurin, 2000; Leuven and Oosterbeek, 2008). We add to this literature in several ways. First, we estimate causal effects by exploiting randomized assignment to training, which overcomes potential self-selection bias (Altonji and Spletzer, 1991; Bartel and Sicherman, 1998). Second, we estimate impacts on retention in addition to productivity; retention is crucial to understanding firms' overall returns to training but has not been examined thus far. Third, we carry out our experiment in a low-income country setting – where training frontline workers might have large potential given low levels of baseline skills – while existing studies use data from firms in high-income countries.

We also contribute to the literature examining the labor market impacts of soft skills (Bassi et al., 2017; Deming, 2015; Groh et al., 2015; Guerra et al., 2014; Heckman and Kautz, 2012; Heckman et al., 2006; Riordan and Rosas, 2003). Growing interest in active labor market policies (Heckman et al., 1999) in low-income countries has spurred high-quality research on the impacts of vocational training programs, which often include a soft skills training component (Betcherman et al., 2004). In general, evidence on the labor market benefits of training alone (as opposed to training plus asset or cash transfers) does not yield consistent evidence of impact.[3] One intervention focused on young women finds positive impacts on some outcomes (Buvinić and Furst-Nichols, 2016).

Only two studies to our knowledge evaluate (via randomized assignment) the impacts of soft skills separately from other types of training: Groh et al. (2012) and Ashraf et al. (2017). Groh et al. (2012) examine the impacts of soft skills training (and separately, wage subsidies) for female community college graduates in Jordan. Treatment effects on the probability of employment, work hours, and in-

[3] There is growing consensus that training, when delivered alongside asset or cash transfers, can have lasting impacts on poverty and wellbeing, especially for women (Bandiera et al., 2016, 2014; Banerjee et al., 2015).

come appear to be quite small. Ashraf et al. (2017) demonstrates that negotiations training for women in Zambia did not affect labor market outcomes, but did impact the wellbeing of children via more greater household bargaining power. Our study takes a complementary approach by targeting a population of young workers who are already employed and for whom high frequency observation of workplace outcomes is possible.

Finally, we contribute to the literature on female labor force participation and workplace outcomes. Female participation in the labor force has stagnated globally and has recently been falling (Morton et al., 2014). In India, it is not only unusually low (India ranks 120th out of 131 countries (Chatterjee et al., 2015)), but has experienced a substantial reduction in the share of women working in rural areas between 1987 and 2009, despite a fertility transition and relatively robust economic growth (Afridi et al., 2016). Studying improvements in career prospects for women, via managerial training and promotion as Macchiavello et al. (2015) do, or via soft-skills training and resulting productivity enhancements as we do, can contribute to understanding determinants of female labor force participation that are amenable to policy intervention.

The rest of the paper is organized as follows. Section 2 discusses the garment production context and reviews the details of the training program and the experimental design. Section 3 discusses the data sources and the construction of key variables, and section 4 describes the estimation strategy. Section 5 describes the results of the estimation. Section 6 discusses and evaluates possible mechanisms, and section 7 concludes with an analysis of the costs and benefits to the firm.

2 Context, Program Details, and Experiment Design

2.1 Context

2.1.1 Ready-made Garments in India

Apparel is one of the largest export sectors in the world, and vitally important for the economies of several large developing countries (Staritz, 2010). India is one of the world's largest producers of textile and garments, with export value totaling $10.7 billion in 2009-2010. The size of the sector and the labor-intensity of the garment production process make the sector well-suited to absorb the influx of young, unskilled and semi-skilled labor migrating from rural self-employment to wage labor in urban areas, especially women (World Bank, 2012). Women comprise the majority of the workforce in garment factories, and new labor force entrants tend to be disproportionately female, particularly in countries like India where the baseline female labor force participation rate is low (Staritz, 2010). Shahi Exports, Private Limited, the firm with which we partnered to do this study, is the largest private garment exporter in India, and the single largest employer of unskilled and semi-skilled female labor in the country.

2.1.2 The Garment Production Process

There are three broad stages of garment production: cutting, sewing, and finishing. In this study, we estimate program impacts on workers from all departments, except for impacts on productivity and task complexity, which are only available for sewing workers (who make up about 80% of the factory's total employment).[4]

In the sewing department of the study factories (as in most medium and large garment factories), garments are sewn in production lines consisting of around 70-100 workers arranged in sequence. Most of the workers on the line are assigned to machines completing sewing tasks (one person to machine). The remaining workers perform complementary tasks to sewing, such as folding or aligning the garment to feed it into a machine. Each line produces a single style of garment at a time (the color and size of the garment might vary but the design and style will be the same for every garment produced by that line until the ordered quantity for that garment is met).

Completed sections of garments pass between machine operators, are attached to each other in additional operations along the way, and emerge at the end of the line as a completed garment. These completed garments are then transferred to the finishing floor. In the finishing department, garments are checked, ironed, and packed for shipping. Most quality checking is done on the sewing floor during production, but final checks are done in the finishing stage. Any garments with quality issues are sent back to the sewing floor for rework or, if irreparably ruined, are discarded before packing.[5] Orders are then packed and sent to ports for export.

[4]This is because a standardized measure of output is recorded for each worker in each hour on the sewing floor, but such a measure is not recorded for workers in other departments.

[5]Completed quantities of garments recorded in the production data reflect only pieces which have passed quality checks, so quantity produced reflects both quantity and minimum quality combined.

2.2 Program Details

The Personal Advancement and Career Enhancement (P.A.C.E.) program was designed and first implemented by GAP Inc. specifically for female garment workers in developing countries. Shahi Exports participated in the original design and piloting of the program as one of the largest suppliers to GAP. The intervention we study involved the implementation of the P.A.C.E. program in five factories in the Bengaluru area which had not yet adopted the program. The goal of this 80-hour program was to improve life skills such as time management, effective communication, problem-solving, and financial literacy for its trainees. The program began with an introductory ceremony for participants, trainers, and firm management. The core modules were: Communication (9.5 hours); Problem Solving and Decision-Making (13 hours); Time and Stress Management (12 hours); Financial Literacy (4.5 hours); Legal Literacy and Social Entitlements (8.5 hours); and Execution Excellence (5 hours).[6] Appendix Table A1 provides an overview of the topics covered in each module. After all modules had been completed, there were two review sessions of about 3 hours in total to review the experience and discuss how participants would apply their learnings to personal and professional life situations. At the close of the program, there was a graduation ceremony.

Each worker attended training for two hours per week. Management allocated one hour of workers' production time a week to the program, and workers contributed one hour of their own time. Training sessions were conducted at the beginning of the production day in designated classroom spaces in the factories, with workers assigned to groups corresponding to different days of the work week. That is, a worker assigned to the Monday group would be expected to attend training starting one hour before production starts on each Monday and ending after the first production hour of the day is completed (i.e., two hours in total). Production constraints required that each day's group be composed of workers from across production lines so as not to produce large, unbalanced absences from any one line in the first hour of any production day. Accordingly, the training groups were balanced in size with roughly 50 trainees per class. Due to holidays and festivals (which are times of high absenteeism), sessions were conducted in practice somewhat more flexibly. Catch-up sessions were conducted for workers who were unable to attend a session. With these adjustments, overall program implementation took slightly over 11 months: the introductory ceremony was in July 2013, training

[6] Additional modules on Water, Sanitation and Hygiene (6 hours) and General and Reproductive Health (10 hours) were also included, but were not considered core modules. Pre/post assessments were not conducted for these ancillary modules and the content in these modules has been reduced in subsequent implementations.

was conducted between July 2013 and May 2014, and the closing ceremony in June 2014.

2.3 Experimental Design

Participants were chosen from a pool of workers who expressed interest and committed to enroll in the program. The workers were informed that the training was over-subscribed and that a subset of workers would be chosen at random from a lottery to actually receive the training, with untreated workers granted the right to enroll in a later lottery for the next training batch.[7] Randomization was conducted at two levels: line level (stratified by unit(factory), above- and below-median efficiency and above- and below-median attendance at baseline, as well as above- and below-median enrollment in the lottery), and then at the individual level within treatment lines. The five factory units had 112 production lines in total. In the first stage of randomization, a proportion of production lines (roughly 2/3) within each factory were randomized to treatment, yielding 80 treatment lines and 32 control lines across units. In the second stage of randomization, within lines randomized to treatment, a fixed number of workers from each treatment line were randomly chosen to take part in the P.A.C.E. program from the total set of workers who expressed interest by enrolling in the treatment lottery.[8]

Approximately 2,700 workers signed up for the treatment lottery, from which 1,087 were chosen for treatment. Out of the 1,616 untrained workers, 779 workers were in control lines, and the remainder, 837 workers, were in treatment lines. The former group (untrained workers in control lines) serves as our primary control. The latter group (untrained workers in treatment lines) is used to estimate treatment spillovers. Summary statistics and balance checks are discussed in Section 3.4.[9]

3 Data

3.1 Production Data

Productivity data was collected using tablet computers assigned to each production line on the sewing floor. The employee in charge of collecting the data (called "production writer"), who was traditionally charged with recording by hand on paper each machine operator's completed operations each hour

[7] Importantly, losers of the lottery were told that they would not necessarily receive the training in the next batch, nor would they be able to earn the right to be trained in any way, but rather subsequent training batches would also be chosen at random via lottery.

[8] The decision to allocate a fixed number of workers to treatment per treatment line was due primarily to production constraints requiring a minimum manpower be present at all times during production hours.

[9] For the sake of brevity, we present balance checks for treatment versus control workers, but of course performed balance checks (available upon request) for spillover versus control workers as well.

for the line, was trained to input production data directly in the tablet computer. These data then wirelessly synced to the server obviating the need for tabulating and hand inputting aggregate production numbers at the end of each day. Importantly, from the perspective of the garment workers, production data were being recorded identically before and after the intervention, with the only difference being the medium by which the data were recorded (and consequently, the accuracy of the resulting data).

3.1.1 Productivity

The key measures of production we study are pieces (garments) produced and efficiency. At the worker-hour level, pieces produced are simply the number of garments that passed a worker's station by the end of that production hour. For example, if a worker was assigned to sew plackets onto shirt fronts, the number of shirt fronts at that worker's station that had completed placket attachment by the end of a given production hour would be recorded as that worker's "pieces produced." In order to calculate the worker-level daily mean of production from these observations, we average the pieces produced by each worker over the course of the day (8 production hours).[10]

Efficiency is calculated as pieces produced divided by the target quantity of pieces per unit time. The target quantity for a given operation is calculated using a measure of garment and operation complexity called the "standard allowable minute" (SAM). SAM is defined as the number of minutes that should be required for a single garment of a particular style to be produced. That is, a garment style with a SAM of 30 is deemed to take a half an hour to produce one complete garment. This measure at the line level is then decomposed into worker or task specific increments. A line with 60 machine operators then would have an average worker-hourly SAM of 0.5 SAM.[11] As the name suggests, it is standardized across the global garment industry and is drawn from an industrial engineering database.[12] The target quantity for a given unit of time for a worker completing a particular operation is then calculated as the unit of time in minutes divided by the SAM. That is, the target quantity of pieces to be produced by a worker in an hour for an operation with a SAM of 0.5 will be $60/.5 = 120$.

Note that though productivity was being recorded prior to the program implementation, the worker-hourly level data was not kept prior to the introduction of the tablet computers for production writing

[10]As noted above, pieces are recorded only if the garment is complete and passes minimum quality standards during in-line and end-line quality checking. In averaging across hourly quantities within the day, we expect that any mismeasurement of productivity arising from re-worked defective pieces is minimized.

[11]Mean SAM across worker hourly observations is 0.61 with a standard deviation of 0.20.

[12]This measure may be amended to account for stylistic variations from the representative garment style in the database. Any amendments are explored and suggested by the sampling department, in which master tailors make samples of each specific style to be produced by lines on the sewing floor (for costing purposes).

but rather discarded after line-daily level aggregate measures were input into the data server. Accordingly, line-daily level aggregate data was all that was available at the time of treatment assignment, and as mentioned above, the first stage randomization of lines to treatment was stratified by line-level baseline efficiency and so is balanced across treatment and control by construction.[13] During the month of treatment announcement (June 2013) the tablets were introduced onto the production floors. Accordingly, June 2013 represents the pre-program baseline for all productivity analysis below.

3.2 Human Resources Data: Attendance and Salary

Data on demographic characteristics, attendance, tenure and salary of workers are kept in a firm-managed database. The data linked to worker ID numbers were shared with us. The variables available in demographic data include age, date on which the worker joined the firm, gender, native language, home town, and education. We combined these with daily attendance data at the worker level indexed by worker ID number and date, which records whether a worker attended work on a given date, whether absence was authorized or not, and whether a worker was late to work on a given day (worker tardiness). We also combined these with monthly salary data which also indicates current skill grade level.

3.3 Survey Data

In addition to measuring workplace outcomes, a survey of 1,000 randomly chosen treated and control workers (comprising 538 treated workers, and the remainder control workers) was conducted in June 2014, the month following program completion. The survey covered, among other things, questions related to financial decisions (including savings and debt) and awareness of and participation in welfare programs (government or employer sponsored). It also covered personality characteristics (conscientiousness, extraversion, locus of control, perseverance, and self-sufficiency), mental health (hope/optimism, self-esteem, and the Kessler 10 module, which can be used to diagnose moderate to severe psychological distress (Kessler et al., 2003)), and risk and time preferences elicited using lottery choices.[14] Finally, the survey covered worker's self-assessments relative to peers (by asking them to imagine a six-step ladder with the lowest productivity workers on the lowest steps, and then asking

[13]Hence, productivity measures are not included in the balance check presented in Table 1. However, as shown in section 5, during the month of treatment announcement (when no training had started), treated and control workers do not have differential productivity.

[14]Risk and time preference measures were taken from the Indonesian Family Life Survey (IFLS).

Table 1: Summary Statistics

	(1)		(2)		(3)	
	Control		Treated		Difference	
P.A.C.E. Treatment (Whole Sample)	**Control Workers in Control Lines**		**Treated Workers in Treatment Lines**			
Number of workers	1,365		1,341			
	Mean	SD	Mean	SD	Mean Difference	p value
Attendance Rate (Jan-May 2013)	0.882	0.235	0.895	0.202	-0.012	0.143
High School	0.626	0.486	0.607	0.486	0.019	0.310
Years of Tenure	1.399	2.405	1.368	2.114	0.031	0.721
Age	27.638	15.052	27.651	13.376	-0.014	0.980
1(Speaks Kannada)	0.676	1.256	0.674	1.073	0.002	0.967
Log(Salary) (May 2013)	8.736	0.238	8.733	0.207	0.003	0.712
Efficiency (Announcement Month)	0.580	0.546	0.555	0.418	0.025	0.290
SAM (Announcement Month)	0.613	0.642	0.612	0.500	0.001	0.974
P.A.C.E. Treatment (Sewing Department)	**Control Workers in Control Lines**		**Treated Workers in Treatment Lines**			
Number of workers	779		1,087			
	Mean	SD	Mean	SD	Mean Difference	p value
Attendance Rate (Jan-May 2013)	0.898	0.117	0.903	0.103	-0.005	0.380
High School	0.602	0.489	0.604	0.489	-0.003	0.901
Years of Tenure	1.432	2.709	1.353	2.119	0.079	0.500
Age	27.712	14.087	27.420	11.638	0.292	0.637
1(Speaks Kannada)	0.657	1.560	0.671	1.156	-0.014	0.834
High Skill Grade	0.616	0.843	0.642	0.688	-0.026	0.473
log(Salary) (May 2013)	8.746	0.188	8.737	0.156	0.009	0.258
Efficiency (Announcement Month)	0.586	0.587	0.556	0.426	0.030	0.268
SAM (Announcement Month)	0.618	0.726	0.615	0.535	0.003	0.928
Spillover Treatment (Sewing Department)	**Control Workers in Control Lines**		**Control Workers in Treatment Lines**			
Number of workers	779		837			

Notes: Tests of differences calculated using errors clustered at the line level according to the experimental design.

them which step they would place themselves on), and participation in skill development programs, production awards, or incentive programs on the job.

3.4 Summary Statistics and Balance Checks

Table 1 presents summary statistics of the main variables of interest, as well as balance checks for baseline values of attendance rate, high school completion, years of tenure with the firm, age, median or higher skill grade (for sewing workers only), and an indicator for speaking the local language (Kannada). Tests of differences in means are presented for the whole sample as well as for the subsample of sewing department workers only. We fail to reject that the difference between treated and control workers for any of these outcome means at baseline is statistically significantly different from zero. Average attendance rates are about 90%, and average tenure with the firm is about 1.4 years. The average worker is about 27-28 years old. Over 60% of both samples are high school educated and speak Kannada.

The summary statistics and differences presented in Table 1 apply to the direct treatment comparison. Analogous balance checks for spillover comparisons in the sewing department subsample were performed as well. Similarly, we find no significant differences. We do not present them here for the sake of brevity.

4 Empirical Strategy

4.1 Overview

The empirical analysis proceeds in several steps, beginning with testing the impact of the program on retention. This is important as a first step because impacts on retention would necessitate a weighting procedure to account for the differential attrition across treatment and control groups. Following this, we test for differences in workplace outcomes, then in survey measures of self-reported personal and professional outcomes, and finally estimate treatment spillovers.

4.2 Retention, Working, and Cumulative Person Days

We estimate the following regression specification to test whether P.A.C.E. treatment impacts retention:

$$\begin{aligned} R_{wdmy} = \alpha_0 + \zeta_1 1[T_w] * 1[\text{Treatment Announced}]_{my} + \zeta_2 1[T_w] * 1[\text{During Treatment}]_{my} + \\ \zeta_3 1[T_w] * 1[\text{After Treatment}]_{my} + \psi_{uym} + \eta_w + \varepsilon_{wdmy} \end{aligned} \tag{1}$$

where the outcome is an indicator variable that takes the value 1 if worker w was retained on day d in month m and year y and 0 otherwise, $1[T_w]$ is a dummy variable that takes the value 1 if the worker is a trained worker on a treatment line and 0 if she is a control worker on a control line, and it is interacted with dummies that take the value 1 for the month that the assignment to treatment was announced, the months during the treatment and the months post-treatment, respectively, thus allowing comparison relative to the pre-announcement period. Each regression includes unit x year x month fixed effects ψ_{uym} (which absorb the main effects of the time dummies) and worker fixed effects η_w (which absorb the main effect of the treatment indicator).

We estimate equation 1 separately for retention dummy variables constructed using both daily attendance data and monthly payroll data. The difference between the two is that with the daily data we can see whether the worker stopped coming to work within the month, even before they are removed from the payroll. Standard errors are clustered at the production line level - while we did a two level randomized treatment assignment with the lower level of treatment at the worker level, we report line level clustering to be as conservative as possible. This is particularly important since we designed the experiment to measure spillover effects, and in fact find evidence, as discussed below, of significant spillovers within a production line.

To estimate the impact of treatment on the additional number of days the firm receives from the worker, we consider two outcomes: the first is a binary working variable that is 1 if the worker was retained *and* is present in the the factory on a given day and 0 otherwise. It is thus a combination of retention and attendance. The second is the number of cumulative person days as measured by the cumulative sum of the first variable. Both are defined at the daily level for each worker. They are estimated as in Equation 1 using these variables instead of retention on the left-hand side. These variables can once again be calculated from two sources of raw data: attendance and production rosters. The first is available for the whole sample of workers from all departments and the second for workers in the sewing department alone.

4.3 Dealing with Potential Bias from Selective Attrition

When examining conditionally observed outcomes such as productivity (which are only observed if the worker is still at the firm), there is a potential for selective attrition based on treatment, which would generate bias in the impact estimates. To test and account for this potential bias, we follow several approaches, outlined below.

1. *Testing directly for treatment-induced changes in the relative size of treatment v. control groups:* We test directly for differential retention by estimating the regression specification in Equation 1 shown above. We present the results in Section 5.1. The results indicate there was no differential retention at the end point of the program period (June 2014) and as well as afterward.

2. *Balance tests by baseline characteristics at different points during and post-program completion:* To test whether the retention across treatment and control is correlated with baseline characteristics, we present the results of balance tests by treatment and control one month after treatment (June

2014) as well as during the last month of data collection (February 2015). Results are presented in Table A10; the analysis shown here demonstrates that all baseline characteristics are balanced on means at both points in time. Tests conducted for other points in time are also balanced and omitted here for brevity. In addition, there is no heterogeneity in retention impacts across distributions of baseline characteristics (which provides a more stringent test than balance checks based on means), as shown in Figures A1-A5 in the Appendix.

3. *Dynamic weighting of conditionally observed outcomes:* As mentioned above, we do not find any differential retention at the end point of the program period, nor do we find any evidence of heterogeneity in retention across treatment and control groups for any baseline characteristics. Despite this, in order to recover population average treatment effects on conditionally observed outcomes throughout the observation period, we weight treatment and control groups by the probability of being observed at any intermediate point in the data. For example, if there exists differential attrition across treatment and control at 6 months into program implementation, even if this difference later equalizes, to recover the population average treatment effect on any conditionally observed outcome (e.g., productivity or salary) at all subsequent points of observation, we must weight all observations prior to that time by the probability of being able to measure the outcome at each point in time. Accordingly, we adapt the approach proposed in Wooldridge (2010) to accommodate any potential heterogeneous impacts of treatment by baseline characteristics of the workers and any differential dynamics in the onset or decay of treatment effects across time, to produce the following method:

   (a) Estimate a probit specification for the probability of being observed, which is a dummy variable that takes the value 1 if the worker is in the sample on any given month and 0 otherwise (i.e., the retained dummy if studying impacts from the attendance or salary data and the working dummy if studying impacts from the production data), on the treatment indicator interacted with month by year fixed effects and baseline characteristics (attendance, education, tenure, age, skill grade, productivity and task complexity).[15]

   (b) We then estimate equation 1 using the conditionally observed outcome variables on the left-hand side and the inverse of the predicted probabilities from step as probability weights. Note that because in the intermediate data (after the announcement but before the endline)

[15] Since workers salaries are homogenous within skill grade level, grade proxies for skill level as well as salary.

the control group is less likely to be working (as shown in the results), this amounts to overweighting a subset of control observations at most points along the timeline.

In practice, once worker fixed effects are included in all regressions, the weighting procedure has negligible effect on the results. We explored robustness to different weights, as well as the absence of weights altogether, but do not present these results for the sake of brevity as they are generally quite similar.

4. *Production line-level estimates:* Finally, we present results for productivity and task complexity at the line level that includes all workers on the production lines, rather than at the individual level. Results are discussed in detail in Section 6.5, and are quite consistent with individual-level results. (Note that we would expect smaller effects at the production line level, given that only a fraction of workers on each line were treated.)

4.4 Productivity and Task Complexity

We estimate treatment impacts on three outcomes from the productivity data: pieces produced, efficiency, and SAM. As discussed above, SAM measures task complexity, pieces produced are number of garments passing the worker's station, and efficiency is actual pieces produced divided by target pieces (calculated from SAM). All of these variables are only measured if a worker is retained by the factory, and present in the factory that day (or more specifically hour).[16]. Accordingly, these conditionally observed outcomes must be weighted in the analysis as discussed above. The weights are obtained from probit regressions of the working dummy on treatment and its interaction with month by year fixed effects and baseline characteristics.

In the SAM regressions, we follow the above specification exactly. However, in the efficiency regression, we replace the worker fixed effects with worker by garment style fixed effects. These are to account for any treatment impacts on the task complexity as identified in the SAM regression. That is, if treatment workers are more likely to be allocated to more complex tasks, a regression of efficiency on treatment that does not restrict the comparison to be within worker by garment style observations would conflate harder task assignment with potentially lower resulting productivity.[17] We also include as additional controls days that the style has been running on the production line and total order size

[16] As mentioned in the previous section, productivity data is available only for members of the sewing department

[17] Indeed, results from regressions omitting garment style fixed effects show weakly negative impacts on productivity.

to account for learning dynamics at the line level that might impact worker productivity across the life of the order. Finally, when regressing pieces produced on treatment, we include target pieces as a control.

4.5 Career Advancement and Career Expectations

To study the impact of the program on career advancement, we measure impacts on gross salary. We first estimate the retention probability weights as detailed in section 3, and then estimate equation 1 using those inverse probability weights, with the log of gross salary as the outcome.[18]

We use five variables from the cross-sectional survey data to cover self-reported performance, subjective expectations of promotion, self-assessment, and initiative in requesting skill development. The subjective expectations of promotion were measured by a binary variable for whether the worker expects to be promoted in the next six months. The request for skill development was measured by asking workers whether they have undergone skill development training in the last six months. Self-reported performance was measured by asking whether workers have received production awards or incentives in the last 6 months. Finally, we measured two kinds of self-assessment. Both asked the worker to imagine a ladder with six steps representing the worst to best workers on their production line (6 being the best). The first self-assessment asked workers where they would place themselves relative to all the workers on their line, and the second where they would place themselves relative to workers of their skill level in their production line. Since the variation in the survey variables is only cross-sectional, we regress these outcomes on a binary variable for treatment or control, and include factory unit fixed effects. In survey outcome regressions, we employ weights obtained from the retention probit using attendance data matched to the date of survey.

4.6 Attendance, Unauthorized Leave and Tardiness

We also analyze attendance outcomes, once again weighting these data by the inverse retention probabilities estimated from the probit specification discussed above. We focus the analysis on three outcome variables: whether the worker is present at work, whether the worker is absent without leave (unauthorized) if absent, and whether the worker was tardy in coming to work.

[18] Note that the administrative salary data is at the monthly level for each worker rather than the daily-level.

4.7 Other Survey Outcomes

Finally, we consider the impact of the program on survey outcomes that might plausibly reflect the skills taught by P.A.C.E. For instance, since the program targets the stock of non-cognitive skills such as the ability to acquire and use information more effectively, we consider outcome variables regarding whether workers avail themselves of government and firm welfare programs like pension schemes and subsidized health-care. Similarly, since the program aims to make workers more forward-looking, we test whether there is an increase in workers' savings, especially for important future considerations like education (their own or their childrens'), and risk and time preferences. Furthermore, we test whether the program impacted personality characteristics (conscientiousness, locus of control, perseverance, extraversion and self-sufficiency) and mental health (self-esteem, hope/optimism, and mental distress.). As mentioned previously, the survey measures are cross-sectional. The regression specification is thus the same as for the survey outcomes in the previous section: we regress the outcome on the binary treatment variable and include factory unit fixed effects and retention weights from the attendance data matched by survey date.

4.8 Figures

We create figures illustrating the month-by-month treatment impacts by re-estimating all the outcome regressions with the treatment binary interacted with monthly dummies from June 2013 onwards (rather than the announcement, during, and after dummies presented in equation 1 above). All regression analogs are reported in tables in the Appendix, with figures presented and discussed in section 5. Dummies for months prior to June 2013 are excluded to make treatment effects relative to the pre-announcement period in all figures, except for those depicting monthly treatment impacts on productivity outcomes for which the announcement month (June 2013) is the first month of observation and the excluded base month.

4.9 Spillover Effects/Production Complementarity Effects

To estimate the effects on untrained workers who interact with trained workers, we re-run all of the specifications mentioned above, replacing the binary treatment variable with the binary spillover treatment variable. This variable compares untrained workers in treatment lines (workers who enrolled in the lottery but did not receive the program and who work in production lines with workers who received the training) with control workers in control lines (workers who enrolled in the lottery but did not receive the program and who work in production lines without any trained workers). Thus, it takes the value 1 if the individual is an untrained worker in a treated line, and 0 if the worker is a control worker in a control line. We expect that both due to ease of communication with treated workers and production complementarities within the line, workers who work alongside treated workers in the same line are most likely to exhibit externalities to treatment. We supplement this analysis with partial correlations between productivity measures of workers on the same line in the announcement month prior to program start. These partial correlations help to indicate the magnitude of the role of technical complementarities in coincident effects on directly treated and spillover workers.

5 Results

5.1 Retention and Daily Working Status

Figure 2A: Raw Retention (Sewing)

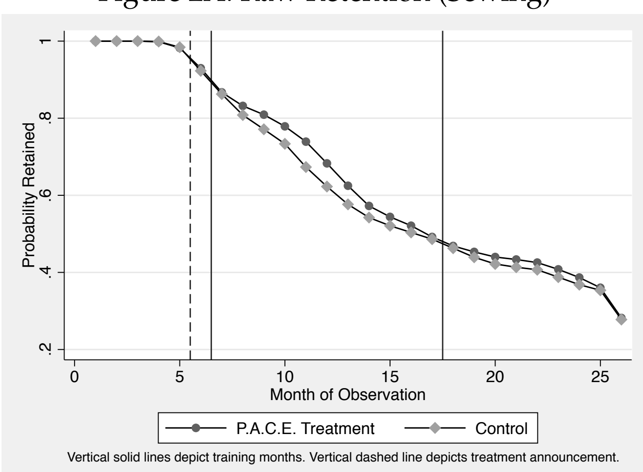
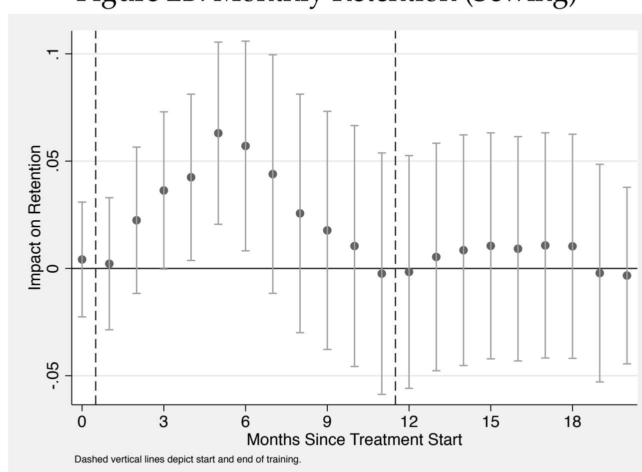


Figure 2B: Monthly Retention (Sewing)



Figures 2A and 2B depict impacts of P.A.C.E treatment on retention. Figure 2A depicts raw retention data from the attendance roster across P.A.C.E treatment and control groups over the full observation period. Figure 2B depicts coefficients of monthly impacts from the preferred regression specification. The corresponding full results are reported in Table A2 in the Appendix. These figures depict data from sewing department workers only for consistency with later results for which we only have data from sewing department workers (i.e., productivity). Analogous figures for the whole sample are available upon request. Figures using payroll roster data instead of attendance data look nearly identical. Accordingly, these are not presented, but are also available upon request. Table 2, however, does present analogous regression results from all of these alternative samples.

We begin by measuring the impacts of P.A.C.E. on retention and the probability that a worker is on the job on a given day. Figure 2A shows raw retention data for both treatment and control groups over the observation period with training months denoted. The dashed vertical line in Figure 2A denotes the announcement of assignment to treatment and the vertical solid lines depict the program window. Since the sampling of retention data started in month 4 of the denoted timeline, retention is mechanically equal to 1 in the first four months. Figure 2B shows analogous regression coefficients to those from Table 2, but with treatment effects estimated month by month. This figure shows that there is a statistically significant impact of treatment on retention early in the program period, which dissipates by the end of the program (the program training window is denoted by dashed vertical lines). The figures shown here are from the sample of sewing workers using the attendance data (to ensure consistency with other outcomes like productivity for which we only have data for the sewing department). Using the entire sample or the payroll data yields nearly identical figures and so these additional figures are omitted for brevity. We do report estimates using these alternate samples in Table 2.

Figure 3A: Raw Working (Sewing)

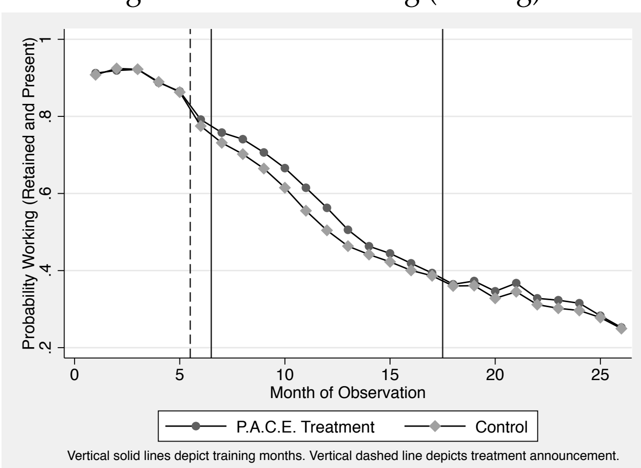
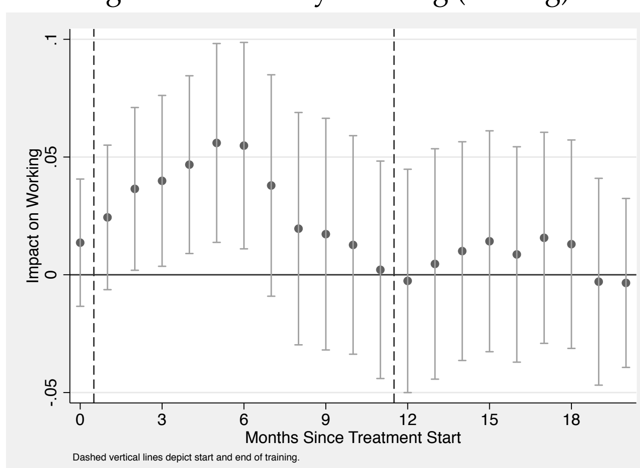


Figure 3B: Monthly Working (Sewing)



Figures 3A and 3B depict impacts of P.A.C.E treatment on working (retained and present) in the factory. Figure 3A depicts raw presence data from the attendance roster across P.A.C.E treatment and control groups over the full observation period. Figure 3B depicts coefficients of monthly impacts from the preferred regression specification. The corresponding full results are reported in Table A2 in the Appendix. These figures depict data from the sample of sewing department workers only, with analogous whole sample figures showing similar patterns and available upon request

The second outcome of interest is the probability that a worker is retained *and* present at work on a given day. This variable, which we refer to as "working" status, is therefore equal to 0 on a given day if the worker has permanently left the factory, or she is still working for the firm but is not present on

Table 2: Impacts of P.A.C.E. Treatment on Retention and Working Status

	(1)	(2)	(3)	(4)	(5)	(6)
	Retained		Retained		Working	
	1(Worker Still on ***Attendance*** Roster)		1(Worker Still on ***Payroll*** Roster)		1(Worker Retained and Present in Factory Today)	
					Attendance Roster	***Production Data***
	(Whole Sample)	*(Sewing Dept Only)*	*(Whole Sample)*	*(Sewing Dept Only)*	*(Whole Sample)*	*(Sewing Dept Only)*
After X P.A.C.E. Treatment	0.0337	0.00534	0.0382	0.00685	0.0170	0.0740**
	(0.0230)	(0.0257)	(0.0265)	-0.0274	(0.0190)	(0.0364)
During X P.A.C.E. Treatment	0.0575**	0.0289	0.0595**	0.0283	0.0431**	0.0897***
	(0.0228)	(0.0212)	(0.0255)	(0.0216)	(0.0180)	(0.0318)
Announced X P.A.C.E.. Treatment	0.0406*	0.00416	0.0438*	0.00476	0.0303*	
	(0.0214)	(0.0136)	(0.0236)	(0.0153)	(0.0171)	
Fixed Effects			Unit X Month X Year, Worker			
Observations	2,078,400	1,433,981	62,585	43,141	1,848,003	778,916
Control Mean of Dependent Variable	0.589	0.628	0.619	0.656	0.480	0.367

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Standard errors are clustered at the treatment line level. Retained dummy and Working dummy are both defined for every worker date observation in the data and therfore the regressions do not require any weighting.

a given day, and is 1 otherwise. Figure 3A shows raw data on the binary variable for working for both treatment and control groups over the observation period (with the treatment announcement period indicated again by the vertical dashed line and the program training window by vertical solid lines). Figure 3B once again shows analogous regression coefficients to those from Table 2, but with month-by-month treatment effects. Figure 3A shows that the probability of working (being retained and present in the factory) is greater for the treatment group throughout much of the treatment period, and Figure 3B confirms that treatment impacts are statistically significant for most of the treatment period but not afterward (the program period is once again denoted by dashed vertical lines in Figure 3B).

Table 2 presents the results for retention and working status. The first two columns present results from the attendance data and the third and fourth column from the payroll data. As in the figures, there is a statistically significant impact of nearly 6 percentage points (pp) during the treatment, and roughly 4pp when the treatment is announced; the pattern is consistent across both sources of data, but is statistically stronger when both sewing and non-sewing workers are considered. We conclude from these results that the program had positive impacts on retention during program announcement and implementation that are quite large relative to mean retention (nearly 10% of the mean), although the impacts dissipate after treatment. The results presented in Table A2 in the Appendix (showing impacts for treatment announcement and each month during and after treatment) exhibit a similar pattern - treatment workers are more likely to be retained during the month of treatment announcement and during treatment, though the impact dissipates towards the end of the program, and disappears altogether post-treatment.

Table 2 also shows the impacts on the working binary during and after the program. We present the results from the attendance data for the entire sample and from the production data for the subsample of sewing department workers. The production data is a precise way to test whether the worker is actually present on the production line on a given day, and thus a more precise measure of attendance for sewing workers - however, it is only available starting June 2013 (the month of treatment announcement), and so that month is the excluded category for the productivity data source.

We find that P.A.C.E. treatment affects both outcomes positively (with statistical precision). Treatment workers are about 3pp more likely to be working during treatment than control workers relative to before treatment, and about 4pp more likely after treatment, a 6-8% increase relative to the mean probability of working. For the sewing department the impacts relative to the control mean are qualitatively similar but larger in magnitude - a 9pp increase during the treatment, and about 7.7pp after the treatment relative to the treatment assignment announcement period. Appendix Table A2 presents the results of the regressions that estimate the impact of treatment in each month separately, and as shown in the Figures 3A and 3B, indicate that the treatment significantly increases the probability that the worker is retained and present. Thus, the treatment has a strong positive impact on the likelihood of working.

As discussed in section 3 above, impacts on retention and worker presence also have implications for the estimation of impacts on outcomes that are conditional on being retained or present (e.g., productivity and salary). We will therefore use a weighting procedure when measuring impacts on these outcomes.

5.2 Productivity and Task Complexity

If P.A.C.E. impacted the stock of soft skills (e.g., time management, intrinsic motivation, communication, extraversion), then it should follow that marginal productivity rises, both through direct channels, to the extent that soft skills are used in production, and indirect channels, if workers were more likely to ask for and receive additional training in hard skills.

To test this hypothesis, we consider three outcomes: 1) efficiency (pieces produced divided by target pieces); 2) average hourly pieces produced each day (controlling for target pieces); and 3) the complexity of the task to which workers are assigned, as measured by SAM (number of minutes in which a task is expected to be completed – a higher SAM thus denotes a more complex task).

Figure 4A: Monthly Efficiency (Sewing)

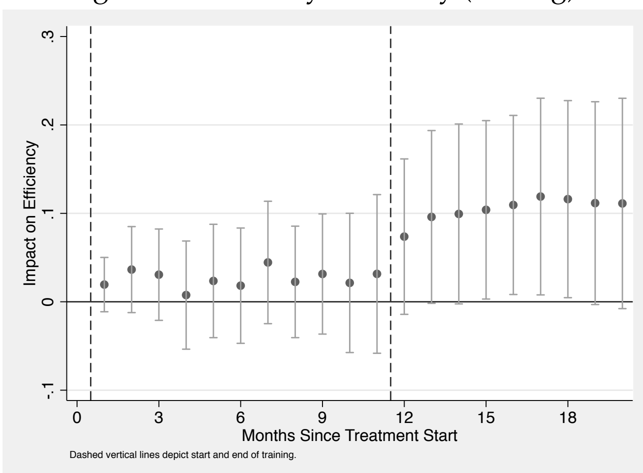
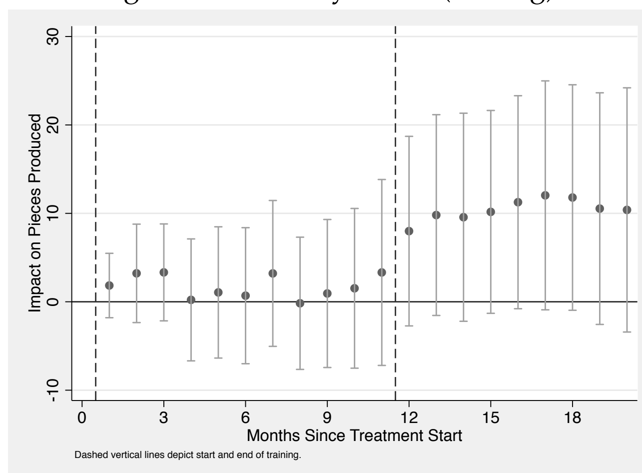


Figure 4B: Monthly Pieces (Sewing)



Figures 4A and 4B depict impacts of P.A.C.E treatment on productivity in the factory. Figure 4A depicts coefficients of monthly impacts on efficiency (actual pieces produced / target pieces) from the preferred regression specification (including worker by item (style) fixed effects and controls for the number of days the worker has been producing that style on that line and the total order quantity). Figure 4B presents the analogous figure for impacts on pieces produced (controlling additionally for target quantity). The corresponding full results are reported in Table A3 in the Appendix. These figures depict data from sewing department workers only as production data exists only for sewing department workers. Note we do not present raw data figures for production since raw data comparisons do not depict clear, easily interpreted patterns without properly accounting for style and operation complexity. However, we explicitly present figures of raw data on operation complexity (SAM) over time along with monthly impacts on the complexity of the operation assigned to each worker in Figures 5A and 5B below.

Figures 4A and 4B show regression coefficients of the impacts of treatment on efficiency and produced quantity analogous to those reported in Table 3, with treatment effects estimated by month. Figure 5A shows raw operation complexity data for both treatment and control groups over the observation period with training months denoted. Figure 5B shows analogous regression coefficients to those from Table 3 for the complexity of the operation the worker is performing as measured by SAM, but with impacts split up monthly. Figures 4A and 4B indicate that treatment increases efficiency and the total production of the workers (controlling for target production) after the program concludes, with impacts a bit more precisely measured for efficiency. Figure 5A and 5B illustrate that both during and after the program, there is evidence that treated workers are assigned to more complex tasks (tasks with higher SAM).

These patterns are confirmed in Table 3, which reports the results of analogous regressions in which impacts are grouped into during and after P.A.C.E. program implementation. Treated workers are

Figure 5A: Raw SAM (Sewing)

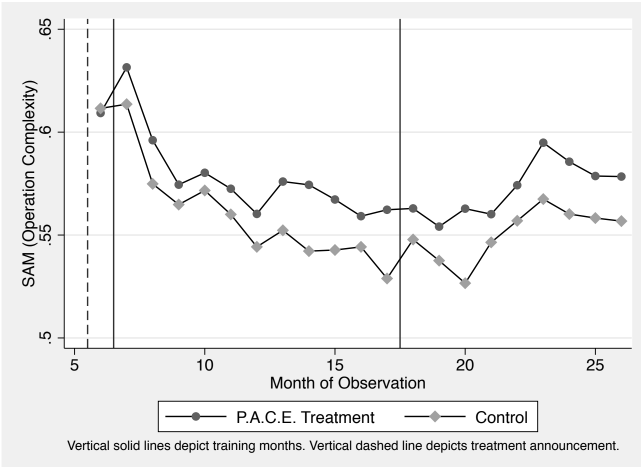
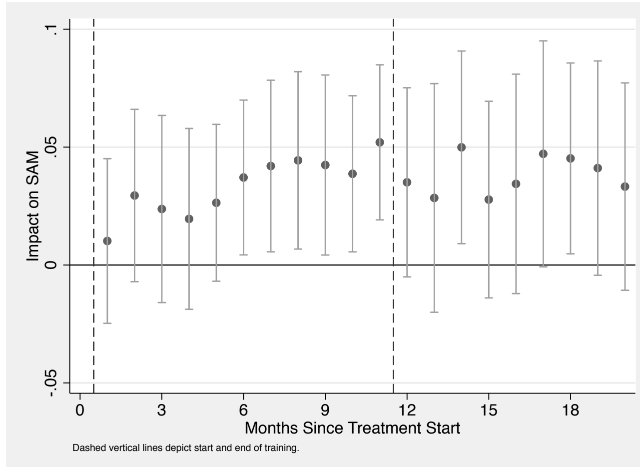


Figure 5B: Monthly SAM (Sewing)



Figures 5A and 5B depict impacts of P.A.C.E treatment on operation complexity (SAM, or standard allowable minute per operation-piece). Figure 5A depicts raw SAM from the production data across P.A.C.E treatment and control groups over the full observation period (June 1, 2013 onwards in the production data). Figure 5B depicts coefficients of monthly impacts from the preferred regression specification. The corresponding full results are reported in Table A3 in the Appendix.

more efficient after the program (relative to the month of treatment assignment announcement) by nearly 7 percentage points, about 12% relative the control group mean. They also produce about 6 garments more each hour on average relative to the control group after the treatment, about 10% of the control group mean. Again, consistent with the evidence presented above, we see that most of the impacts on productivity accrue *after* program completion.

On the other hand, we see fairly consistent impacts on task complexity (SAM) throughout the program, and they are sustained and remain statistically significant after the program period. That is, treated workers are assigned to more complex tasks both during and after treatment (tasks to which they are assigned are expected to take about 2 seconds (0.03 minutes) more, roughly 5% of the control group mean). Thus, not only are workers in the treatment group assigned to more complex tasks during and after the program, they are more productive even at these harder tasks once treatment ends. The non-cognitive skills that the program covers (like time management, goal setting, and team work) enhance worker productivity and the ability to perform complex tasks.

The time pattern of impacts on productivity – insignificant increases during the program period followed by large, significant increases afterward – is striking and deserves additional consideration. We reason that the "incubation period" for productivity impacts in the context of this program, through

Table 3: Impacts of P.A.C.E. Treatment on Productivity

	(1)	(2)	(3)
	Efficiency	Pieces Produced	SAM (Operation Complexity)
	Mean(Produced/Target)	Mean(Pieces per Hour)	Mean(Standard Allowable Minute)
After X P.A.C.E. Treatment	0.0681**	7.272**	0.0334*
	(0.0301)	(3.286)	(0.0180)
During X P.A.C.E. Treatment	0.0203	1.926	0.0320**
	(0.0153)	(1.812)	(0.0146)
Additional Controls	Days on Same Line-Garment, Total Order Size	Days on Same Line-Garment, Total Order Size, Target Pieces	None
Fixed Effects	Unit X Month X Year, Worker X Garment		Unit X Month X Year, Worker
Weights	Inverse Predicted Probability from Probit of Working on Treatments X Mo-Yr X Baseline Characteristics		
Observations	290,763	290,763	290,763
Control Mean of Dependent Variable	0.542	62.250	0.565

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Standard errors are clustered at the treatment line level. Observations are weighted in regressions by the inverse of the predicted probability of working (i.e., not yet attrited and present in the factory with non-missing data) in the sample that day from a probit regression of the working dummy on month by year FE and their interaction with individual and line treatment dummies and baseline variables reported in Table 1. Sample is trimmed in these regressions to omit days in which the worker is observed for only a half a production day or less or days in which the worker is observed for more than 2 overtime hours as these are anomalous observations with imprecise production measures. These outliers make up only around 5% of the work-day observations

both direct and indirect channels mentioned above, is likely long. First, truly learning soft skills to the point that they can be applied in the workplace may take time. Second, sets of soft skills may be complementary, so that the incremental learnings in a given module have a greater impact later in the program. Third, from anecdotal observation, women took several months to become true participants in the group sessions; at the beginning of the program the level of participation, fitting with the cultural context in which these women live, was quite low. Fourth, speaking to the indirect channel of requesting and acquiring hard skills, perfecting basic sewing techniques and learning additional ones likely takes time. Finally, fifth, the more complex tasks to which women were assigned likely generated a lag in the increase of efficiency.

For all of the above reasons, we might see productivity impacts rising only toward the end of the program period. We cannot say with statistical power whether the rise in productivity was sudden (occurring exactly at the end of the program) or more gradual, but the results on quantity, and to a lesser extent the results on efficiency, seem to indicate a gradual pattern that begins several months before program completion.

5.3 Person Days and Career Advancement

In addition to worker presence and productivity, we consider the total number of working days accrued to the firm, and career advancement within the firm. The measure of the first outcome is the cumulative number of working days that accrue to the firm. This is the running sum of the worker presence, which is the cumulative number of days that the worker was present in the firm. Since this variable is not conditional on retention (not missing if the worker has left the firm), no re-weighting of the treatment and control groups are required. To estimate the impacts of treatment on career advancement, we consider both whether the worker was given a raise using monthly payroll data as well as worker-reported measures of expectations of promotion, whether they recently asked for (and received) skill development training and production incentives, and finally, how they assess their own ability relative to all workers on their production line, and relative to workers in their production line that are the same skill level as them. Except for the salary data which is at the monthly level for each worker, the self-reported measures are from a worker-level survey and vary only cross-sectionally.

Figure 6A: Raw Person Days (Sewing)

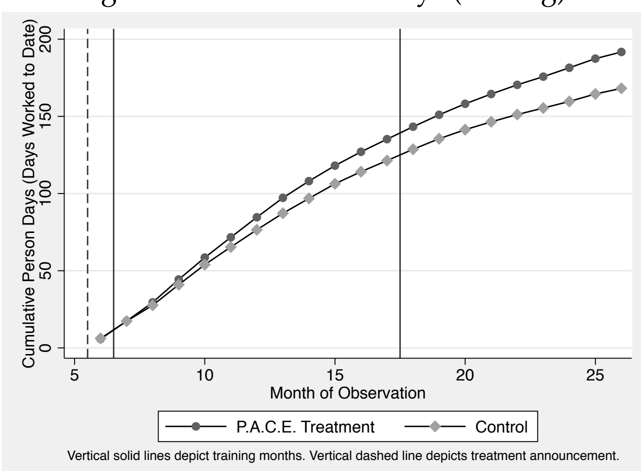
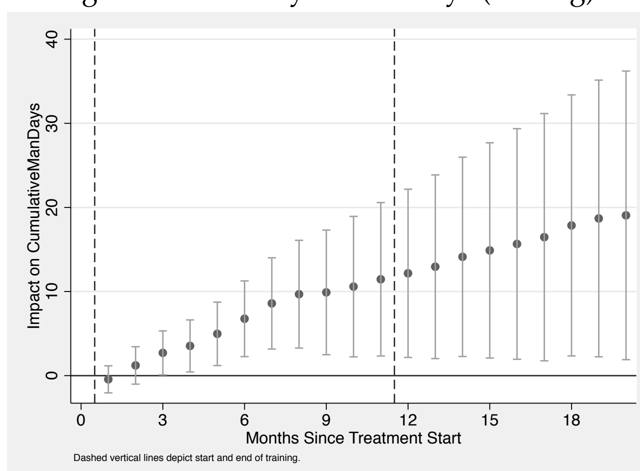


Figure 6B: Monthly Person Days (Sewing)



Figures 6A and 6B depict impacts of P.A.C.E treatment on cumulative person days in the factory from the start of the observation period (January 1, 2013) to date. Figure 6A depicts raw person days data from the production data across P.A.C.E treatment and control groups over the full observation period. Figure 6B depicts coefficients of monthly impacts from the preferred regression specification. The corresponding full results are reported in Table A4 in the Appendix. These figures depict data from sewing department workers only as the production data only exists for these workers. Analogous figures for the whole sample of workers using attendance roster data are available upon request.

Figures 6A and 6B show that the treatment impact on cumulative person days is positive and statistically significant by roughly 3 months into the program period. The impacts continue to grow quickly through month 8 of the training period, after which the growth slows somewhat but remains positive through the remainder of the observation period. Columns 1 and 2 of Table 4 show the impacts on cumulative person days during and after the program, using attendance and production data, respectively. We present the results from the attendance data for the entire sample and from the production data for the subsample of sewing department workers. The treatment increases the cumulative person days per treated worker by 8.5 days during treatment and 19 days after treatment when the entire sample is considered, which is about 4.25% and 9% of the mean cumulative number of days of the control group.

Appendix Table A4 presents the results of the regressions that estimate the impact of treatment in each month separately, and as shown in the Figures 6A and 6B, indicate that the treatment significantly increases the cumulative person days during and after the program. Thus, the treatment has a strong positive impact of the number of person days for the firm, which is an important consideration in the cost-benefit analysis conducted in section 7.

Figure 7: Monthly Salary (Sewing)

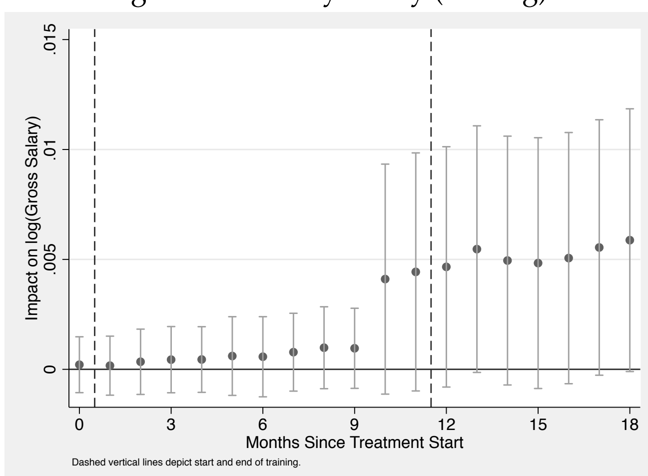


Figure 7 depicts coefficients of monthly impacts of P.A.C.E treatment on log(gross salary) from the preferred regression specification. This figure depicts data for sewing department workers only as aforementioned productivity impacts and profitability calculations discussed below pertain only to this sample. The corresponding full results are available upon request.

Figure 7 shows analogous regression coefficients to those from Table 4, but with impacts split up monthly and training months denoted. We see in Figure 7 that PACE workers are paid negligibly more (roughly half a percent), with the gap showing up towards the end of the program period. Column 3 of Table 4 illustrates the results of the estimation comparing treatment workers to control workers during the treatment assignment announcement month, and during and after the treatment (relative to before the treatment assignment announcement month). Treatment workers receive less than half a percent more wages in the period after the program completion, which translates to roughly 30 INR or less than .5 USD a month. Thus, despite being assigned to more complex tasks and being more productive, treated workers are not paid significantly higher wages.

Columns 4-8 of Table 4 presents the results from analysis of related survey outcomes. Treatment workers are nearly 7.7 percentage points more likely to report that they expect a promotion within the next six months (roughly 13% of the control group mean), and are nearly 15 percentage points more likely to request skill development training (59% of the control group mean). They are not significantly more likely to report having received a production incentive or award, but rate themselves higher relative to relative to all co-workers in their production line. Specifically, when asked to rank themselves relative to workers in their production line of the same skill grade level, they are significantly more likely to rate themselves at a higher level (as shown in column 8). Finally, Table A4 in the Appendix presents the month by month estimation results for cumulative person days and salary. As shown in the figures, salaries are marginally higher for treatment workers in the last 2 months of treatment and after program completion.

5.4 Attendance

Related outcomes of interest are attendance (a binary variable that is 1 if the worker is at work today and 0 if not), unauthorized leave (a binary variable that is 1 if the worker is not at work today and did not inform the employer and 0 if she is either at work or absent and took prior formal leave from the employer), and tardiness (a binary variable that is 1 if the worker was late relative to the modal arrival time of co-workers on the line that day and 0 if not). Appendix Table A5 present the impacts of treatment on these outcomes. There are no precisely measured impacts on any of the outcomes if the grouping is done by these milestones rather than a month by month comparison. Appendix Table A6 presents the regression results of the month by month estimation. The results indicate that treatment workers are more likely to attend work in the first two months of the program, and absences are more likely to be authorized during the same months. Worker tardiness does not appear to be impacted during or after treatment.

Table 4: Impacts of P.A.C.E. Treatment on Cumulative Person Days and Career Advancement

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Cumulative Person Days		log(Gross Salary)	Expect Promotion Next 6 Mos	Skill Development Training	Production Award or Incentive	Line Co-Worker Self-Assessment	Skill Peer Self-Assessment
	Sum of Days Working for Each Worker to Date			Self-Reported Binary from Survey				
	Attendance Roster	***Production Data***	***Salary Data***	***Survey Data***				
	(Whole Sample)	*(Sewing Dept Only)*	*(Sewing Dept Only)*	*(Sewing Dept Only)*				
After X P.A.C.E. Treatment	19.44**	15.74**	0.00493*					
	(8.495)	(6.888)	(0.00274)					
During X P.A.C.E. Treatment	8.410*	6.339***	0.00114					
	(4.281)	(2.393)	(0.000835)					
Announced X P.A.C.E. Treatment	-1.058		0.000218					
	(4.938)		(0.000648)					
P.A.C.E. Treatment				0.0767*	0.148***	0.0281	0.0784	0.130**
				(0.0429)	(0.0484)	(0.0184)	(0.0688)	(0.0645)
Fixed Effects	Unit X Month X Year, Worker			Unit				
Weights	None		Inverse Predicted Probability from Probit of Retention on Treatments X Mo-Yr X Baseline Characteristics					
Observations	1,848,003	778,916	28,692	621	621	621	621	621
Control Mean of Dependent Variable	201.408	103.220	8.909	0.562	0.251	0.032	5.276	5.321

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Standard errors are clustered at the line level. Observations are weighted in regressions by the inverse of the predicted probability of being retained (i.e., not yet attrited with non-missing data) in the sample that day from a probit regression of the retained dummy on month by year FE and their interaction with individual and line treatment dummies and baseline variables reported in Table 1.

6 Mechanisms

The results discussed in the previous section indicate that soft-skills training, which increases worker productivity and ability to handle complex tasks, does not lead to (differential) attrition of trained workers from the firm. While treated workers' wages do increase, they do so at a much lower proportion than their productivity. These findings are consistent with studies using observational data on worker training (Autor, 2001), theoretical models that predict employers' ability to retain rents from general worker training make it profitable (Acemoglu, 1997; Acemoglu and Pischke, 1998, 1999).

Our interpretation of the productivity and task complexity results is that skills like time and stress management; communication; problem solving and decision-making; and effective teamwork are "soft" inputs into production. Reinforcing these skills through the P.A.C.E. program should thus directly affect workplace outcomes. Across the categories of results presented below, impacts are consistent with a direct treatment effect on the stock of soft skills. In particular, the narrative that emerges is one that is consistent with the P.A.C.E. program increasing the stock of soft skills. This is indicated in part that treated women are more likely to proactively increase their stock of hard skills by requesting technical training, are more extraverted, more likely to seek out and avail themselves of government and employer benefits to which they are entitled, and more likely to exhibit forward looking behavior via savings and aspirations for their children's future. Finally, these women share learnings with their co-workers, and these spillovers contribute to more productive co-workers over and above direct production complementarities.

Below, we support this interpretation using evidence from a survey of treatment and control workers; from assessments of the treatment group's knowledge before and after the completion of the program's core modules; and from the size and nature of treatment spillovers. We also present several alternative interpretations and discuss the plausibility of each in turn.

6.1 Survey Results

The first piece of evidence supporting the interpretation that the stock of soft skills changed comes from a survey we administered to treatment and control workers in the month after program completion. Table 5 evaluates the impact of P.A.C.E. treatment on financial behaviors and attitudes (Panel A); availing of firm and government programs (Panel B); personality (Panel C); and mental well-being and aspirations (Panel D).

Table 5: Impacts of P.A.C.E. Treatment on Women's Outcomes

	(1)	(2)	(3)	(4)	(5)
Panel A: Financial Behaviors and Attitudes	Saving for Education	Saving for Other Reasons	Risk and Time Preference Index	Insurance	Informal Borrow or Lend
P.A.C.E. Treatment	0.0607*	-0.0332	-0.154*	0.00742	0.0235
	(0.0349)	(0.0364)	(0.0866)	(0.0395)	(0.0444)
Control Group Mean of Dependent Variable	0.263	0.273	0.054	0.368	0.429
Panel B: Government and Firm Entitlements	Gov. Pension	Gov. Subsidized Healthcare	Other Gov. Subsidy	Firm Entitlements	Community Self Help Group
P.A.C.E. Treatment	0.0232*	0.0221**	0.00746	-0.0303	-0.0346
	(0.0137)	(0.00982)	(0.0299)	(0.0332)	(0.0313)
Control Group Mean of Dependent Variable	0.038	0.006	0.117	0.140	0.149
Panel C: Personality	Conscientiousness	Locus of Control	Perserverance	Extraversion	Self-Sufficiency
P.A.C.E. Treatment	0.0530	0.0264	-0.105	0.159**	0.0383
	(0.0776)	(0.0787)	(0.0902)	(0.0678)	(0.0872)
Control Group Mean of Dependent Variable	-0.041	-0.023	0.037	-0.064	-0.067
Panel D: Mental Health and Aspirations	Self-Esteem	Hope/Optimism	Moderate Distress	Child's Expected Age at Marriage	Child Educated Beyond College
P.A.C.E. Treatment	-0.158	-0.0634	-0.0419	0.0793	0.0808***
	(0.113)	(0.0837)	(0.0384)	(0.167)	(0.0280)
Control Group Mean of Dependent Variable	0.049	0.027	0.095	23.416	0.114
Fixed Effects	Unit	Unit	Unit	Unit	Unit
Weighted	Yes	Yes	Yes	Yes	Yes
Observations	621	621	621	621	621

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Standard errors are clustered at the treatment line level. Obersvations are weighted in regressions by the inverse of the predicted probability of being retained (i.e., not yet attrited with non-missing data) in the sample that day from a probit regression in the attendance roster of the retained dummy on month by year FE and their interaction with individual and line treatment dummies and baseline variables reported in Table 1.

We discuss results in support of each category in turn to lay out our reasoning. The first category is meant to evaluate whether P.A.C.E. treatment changes women's financial behaviors and attitudes. This change would be consistent with a shift in forward-looking behavior, an important dimension of soft skills. The results from Panel A indicate that there is a positive impact on saving for own and children's education, and the impacts are quite large relative to the control group mean (about a quarter of the control group mean). Savings for other purposes show no significant impacts; self-reported measures of participating in insurance and informal lending show positive but insignificant impacts. Finally, we construct a survey-based measure of combined risk-aversion and patience with higher scores corresponding to extreme, seemingly irrational, levels of risk aversion and patience (i.e., dominated selections between tradeoffs of certain and risky payouts and current and future payouts). The estimate suggests that treatment reduces this baseline extreme risk aversion and patience, although this estimate is measured imprecisely.

The second category, availing oneself of government and employer-based entitlement programs, is meant to evaluate changes in the effectiveness of information acquisition, another important soft skill. The results in Panel B show that treated workers are substantially more likely to seek out welfare programs. Impacts on binary indicators for self-reported seeking out of government pension and government subsidized healthcare indicate that treated workers are more likely to avail themselves of these programs. The magnitude of these impacts are quite large relative to control group means, which are around 0 for both outcomes. Impacts on other government subsidies and firm entitlements are negligible. Finally, we find the intriguing result that membership in community self-help groups (a common and often powerful feature of social networks for Indian women) goes down, though this estimate is imprecise. This result might suggest a substitution occurring due to the P.A.C.E. program's impact on social connections and empowerment-related agenda in the workplace.

The third category, personality, is meant to assess differences in key traits that are associated with the stock of soft skills, namely conscientiousness, locus of control, perseverance, extraversion, and self-sufficiency. In general, the impact estimates (shown in Panel C) suffer from lack of precision, but P.A.C.E. treatment does have a large positive and statistically significant impact on extraversion. This result on extraversion is consistent with the results above related to seeking out information and resources, as well as results on self-reported comparisons to co-workers, which show that P.A.C.E. training increased self-regard with respect to workplace performance relative to peers.

The final category of the survey, mental health and aspirations, is meant to assess impacts on psychological well-being and the extent to which future aspirations are affected by treatment. The results reported in Panel D show that, in general, outcomes associated with psychological well-being (self-esteem, optimism, and mental distress) are unaffected by P.A.C.E. treatment, but aspirations for children's education rise dramatically in relation to the control group mean. This is consistent with the result on saving for education presented in Panel A.

6.2 Pre- and Post-Module Assessments

Figure 8A: Post - Pre % Change in Score

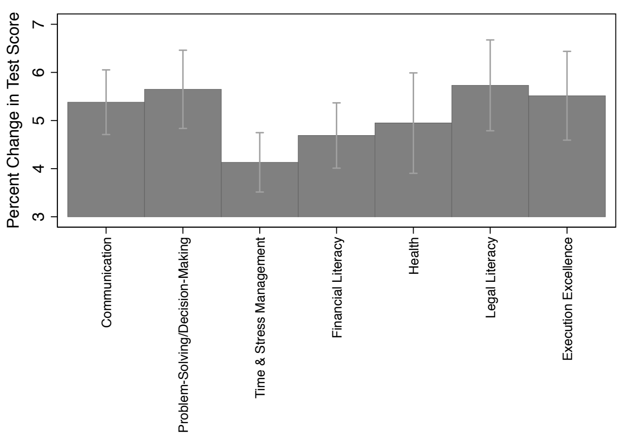
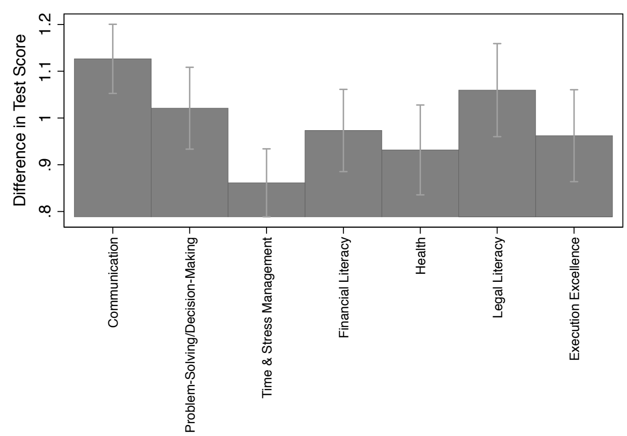


Figure 8B: Post - Pre Difference in Score



Figures 8A and 8B depict normalized (percent change, 8A) and raw (8B) differences in the pre- and post-module assessments administered for all core P.A.C.E. modules. Raw scores for each assessment are out of 100. These assessments were, of course, not given to control workers and accordingly cannot be analyzed in the preferred specification.

The second source of evidence on the direct impacts of P.A.C.E. on the stock of soft skills is pre- and post-module assessments built into the program. These assessments were designed to test the specific value added from each core program module. They were only administered to program participants, and thus we cannot compute a treatment v. control difference, rather only a post- v. pre-module difference.

We present two sets of results, shown in Figures 8A and 8B. Figure 8A shows the difference between (identical) assessments taken pre- and post-module completion normalized by the baseline score, creating a percent change indicator from pre to post for each core P.A.C.E. module. Figure 8B shows the raw difference between the pre- and post-module assessments, again for the set of core modules.

The results from both analyses show that P.A.C.E. participants retained, and were able to communicate, the soft skills acquired in each of the program's core modules. The changes shown in Figure 8A are all in the neighborhood of 5-6 percent, with the largest impacts (in percent terms) for Communication, Execution Excellence, Legal Literacy, and Problem-solving/Decision-making.. The largest raw difference, shown in Figure 8B, is from the Communication module.

These results fit well with the notion that workers absorbed the skills taught in each of the core modules, and that the stock of skills increased, at least in the short run. There are several caveats in interpreting these changes. First, as described above, control workers were not given the assessments, so we are not able to estimate impacts by comparing across randomized participants (treatment v. control). Second, we are measuring skill retention directly after module completion; this does not necessarily reflect true (long-term) skill absorption. Nevertheless, these assessments offer some supporting evidence that is consistent with our hypothesis that P.A.C.E. acted on workplace outcomes by increasing the stock of soft skills.

6.3 Treatment Spillovers

Finally, we consider evidence on spillovers. Recall that the experiment was designed to capture spillovers within production lines through a two-stage randomization procedure, in which lines were first randomized to treatment or control, and then within treatment lines, workers who had enrolled in the P.A.C.E. lottery were randomized to treatment or to the spillover group. In this section we evaluate spillovers by comparing the outcomes of this latter group to control workers on control lines.

Spillovers are important from both a program evaluation standpoint as well as from the perspective of understanding mechanisms of impact. Regarding program evaluation, if spillovers are large, then we know the program's benefits extend to non-participants through communication amongst co-workers, providing greater justification for employer investment in soft skills training. Regarding mechanisms, if spillovers exist, we know that it is likely that participants actually increased their stock of soft skills enough to share some of those gains with their co-workers. We evaluate these hypotheses in Table 6, which presents the spillover results for workplace outcomes of interest. (Note that probability weights, when necessary, are calculated exactly as they are in the treatment effect estimation, using spillover treatment indicators in place of direct P.A.C.E. training.)

Panel A presents the results for person days as well as productivity and task complexity. There are some weakly statistically significant impacts on the binary for working during treatment for the entire sample, and a stronger result for cumulative person days - untrained workers who work with treated workers work for about 7.8 more days during program months relative to control workers. Moreover, efficiency, pieces produced, and SAM show spillover impacts nearly as large as the main impacts of P.A.C.E. treatment. Panel B presents the results for career advancement variables. As in the productivity and task complexity outcomes, the spillover impacts on salary are of the same magnitude as direct effects (though more precisely estimated). We interpret wage impacts on spillovers as precisely estimated 0s (point estimates indicate raises of less than 1%). The worker survey outcomes on expected probability of promotion, requesting skill development training, receiving a production incentive or self-assessment relative to co-workers are not precisely measured, but again have coefficients of the same sign as the main treatment impacts.

Table 7 presents the results for the non-workplace outcomes of interest for spillovers. The only strongly statistically significant impacts are that workers who work with treated workers are more likely to avail of government subsidized healthcare. On the whole, impacts on non-workplace outcomes do not show any robust spillover impacts.

In sum, then, for workplace outcomes we see large spillover impacts on cumulative person days accrued to the firm, efficiency and pieces produced and task complexity, with many of these impacts being nearly as large as those from direct training. We see some evidence for spillovers on outcomes outside the workplace, but the results are imprecise in general. Overall, the presence of spillovers suggests that knowledge transfer happened as a direct result of the program – i.e., that program participants imbibed soft skills, which they then communicated to co-workers on their production lines, and that transfer helped improve outcomes of non-participants, as well.

It should be noted that it is possible that spillover impacts are observed in our setting, even absent any true transfer of soft skills, by way of technical complementarities in production. While these certainly exist given the team production environment in our context, we do not think the evidence supports the hypothesis that spillovers are entirely, or even mostly, due to technical complementarity. We estimate partial correlations between contemporaneous productivity measures of co-workers on the line using pre-program data (during the announcement month of June 2013) and find that the size of the complementarities are substantially smaller than the spillover effect estimates on productivity presented here. According to our estimates, technical complementarities produce partial correlations of less than 30% in within line efficiency across co-workers, roughly 20% in pieces produced contem-

Table 6: Spillovers on Co-Workers (Attendance, Productivity, and Career Advancement)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Production	Working		Cumulative Man Days		Efficiency	Pieces Produced	SAM (Operation Complexity)
	Attendance	***Production***	***Attendance***	***Production***			
After X Spillover	-0.0163	0.0350	8.867	8.034	0.0648*	7.383*	0.0396**
	(0.0207)	(0.0434)	(9.079)	(7.626)	(0.0349)	(3.812)	(0.0174)
During X Spillover	0.0297	0.0663*	7.780**	4.489	0.00787	1.072	0.0111
	(0.0211)	(0.0385)	(3.568)	(2.820)	(0.0162)	(2.137)	(0.0132)
Announced X Spillover	0.0317*		2.151				
	(0.0172)		(1.372)				
Fixed Effects	Unit X Month X Year, Worker				Unit X Month X Year, Worker X Garment		Unit X Month X Year, Worker
Weights	None				Inverse Predicted Probability from Probit of Working on Treatments X Mo-Yr X Baseline Characteristics		
Observations	1,102,880	673,407	562,478	673,407	241,322	241,322	241,322
Control Mean of Dependent Variable	0.519	0.382	0.390	107.437	0.548	63.011	0.565
Panel B: Retention and Career Advancement	Retained		log(Gross Salary)	Skill Development Training	Production Award or Incentive	Line Co-Worker Self-Assessment	Skill Peer Self-Assessment
	Attendance	***Payroll***					
After X Spillover	-0.0177	-0.0155	0.00907***				
	(0.0246)	(0.0257)	(0.00300)				
During X Spillover	0.0361	0.0386	0.00198**				
	(0.0234)	(0.0239)	(0.000813)				
Announced X Spillover	0.0173	0.0197	0.000313				
	(0.0161)	(0.0177)	(0.000544)				
Spillover				0.0168	0.0116	0.132*	0.0933
				(0.0584)	(0.0226)	(0.0717)	(0.0704)
Fixed Effects	Unit X Month X Year, Worker						
Weights	None		Inverse Predicted Probability from Probit of Retention on Treatments X Mo-Yr X Baseline Characteristics				
Observations	1,241,328	37,357	24,508	527	527	527	527
Control Mean of Dependent Variable	0.628	0.656	8.909	0.247	0.030	5.243	5.270

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Standard errors are clustered at the treatment line level. All regressions are for sewing department workers only as spillover sample is not defined for non-sewing workers. Retained and working dummies and cumulative man days are defined for every worker date observation in the data and therfore regressions do not require any weighting. Observations in attendance and advancement regressions are weighted in regressions by the inverse of the predicted probability of being retained (i.e., not yet attrited with non-missing data) in the sample that day from a probit regression of the retained dummy on month by year FE and their interaction with individual and line treatment dummies and baseline variables reported in Table 1.

poraneously by co-workers, and less than 5% in complexity of task assignments within garment style. See Appendix section A.3 for more detail on the estimation of complementarities.

6.4 Alternative Mechanisms

Having presented evidence on the salience of direct skilling as a result of the P.A.C.E. program, we now discuss several alternative interpretations of the results and any supporting evidence of each.

First, we evaluate the possibility that the effects were due to sheepskin effects, i.e., taking part in P.A.C.E. "certified" workers as high quality from the perspective of management, and this led to the improvements in workplace outcomes we observe. We reason that sheepskin effects are unlikely to explain the majority of the program's impacts given the presence of large spillovers. That is, non-participants (who should have no gains through sheepskin effects) who worked with P.A.C.E.-trained workers showed large impacts on workplace outcomes, as well, but did not earn a certificate for program completion. Our reasoning is that if sheepskin effects are driving the changes we see, the spillover group would not have shown such large impacts.

Second, we evaluate reciprocity (an impulse to give back to the employer as a result of access to the program) as the predominant mechanism driving workplace impacts. While it is plausible that some part of the impacts observed is due to reciprocity, we find it unlikely that the majority of impacts are due to this mechanism. This is for two reasons. First, again, spillovers in treatment would be difficult to explain if reciprocity were the main driving force behind workplace impacts, since non-participants should not be driven by this motive. Second, productivity impacts accumulate slowly during the program period and persist for at least 9 months after program completion, with the largest productivity impacts occurring during this post-training period. This does not fit well with a reciprocity motive as a primary mechanism, since we would expect the reciprocity motive to be strongest while the program is offered. This is in line with recent evidence on the limited role for reciprocity in the workplace (DellaVigna et al., 2016).

Third, it is possible that workers found the classes enjoyable and they improved workers' mental well-being, which in turn made workers more productive. The results reported in Panel D in Table 5 show that levels of psychological distress are unaffected by treatment, which contradict changes in worker well-being and happiness being the mechanism for productivity impacts.[19]

Finally, we evaluate the idea that increased social capital drives the results on workplace impacts.

[19]Results are unchanged if severe mental distress is used as an outcome instead of moderate mental distress.

Table 7: Spillovers on Co-Workers (Financial Behaviors, Personality, and Mental Health)

	(1)	(2)	(3)	(4)	(5)
Panel A: Financial Behaviors and Attitudes	Saving for Education	Saving for Other Reasons	Risk and Time Preference Index	Insurance	Informal Borrow or Lend
Spillover	0.0547	0.00840	-0.111	0.0428	-0.0320
	(0.0419)	(0.0431)	(0.106)	(0.0469)	(0.0470)
Control Group Mean of Dependent Variable	0.270	0.270	0.061	0.376	0.418
Panel B: Government and Firm Entitlements	Gov. Pension	Gov. Subsidized Healthcare	Other Gov. Subsidy	Firm Entitlements	Community Self Help Group
Spillover	0.0107	0.0327**	0.0213	-0.0166	0.0114
	(0.0178)	(0.0142)	(0.0320)	(0.0238)	(0.0323)
Control Group Mean of Dependent Variable	0.027	0.008	0.106	0.137	0.152
Panel C: Personality	Conscientiousness	Locus of Control	Perserverance	Extraversion	Self-Sufficiency
Spillover	-0.00153	0.122	-0.152	0.0903	0.0861
	(0.0838)	(0.0889)	(0.0958)	(0.0863)	(0.0979)
Control Group Mean of Dependent Variable	-0.027	-0.044	0.033	-0.079	-0.056
Panel D: Mental Health and Aspirations	Self-Esteem	Hope/Optimism	Moderate Distress	Child's Expected Age at Marriage	Child Educated Beyond College
Spillover	-0.169*	-0.0949	-0.00407	-0.0830	0.0399
	(0.0974)	(0.0956)	(0.0311)	(0.210)	(0.0335)
Control Group Mean of Dependent Variable	0.057	0.051	0.099	23.424	0.099
Fixed Effects	Unit	Unit	Unit	Unit	Unit
Weighted	Yes	Yes	Yes	Yes	Yes
Observations	527	527	527	527	527

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Standard errors are clustered at the treatment line level. Observations are weighted in regressions by the inverse of the predicted probability of being retained (i.e., not yet attrited with non-missing data) in the sample that day from a probit regression in the attendance roster of the retained dummy on month by year FE and their interaction with individual and line treatment dummies and baseline variables reported in Table 1.

The argument here is that it is possible that P.A.C.E. group sessions encouraged social interaction, which in turn has productive benefits through knowledge and skill transfer. Indeed, recent work by Menzel (2015), who carried out an experiment shifting the extent of social connectivity in garment factories in Bangladesh, affirms this idea.

While it is quite likely (from casual observation of the program sessions) that P.A.C.E. did increase social capital among workers, we contend that it probably did not account for the majority of program impacts. First, language-based and cultural barriers are quite salient in the workplace in our context, likely limiting the extent of the importance of social connectivity in productivity. Nearly half the workers in the factories under study are migrants, many of whom do not speak Kannada, the indigenous language of Karnataka (the Indian state where Bengaluru is located). Second, due to throughput constraints which dictated the number of workers from the same production line who could leave at the same time for a P.A.C.E. session, co-workers on the same line were usually placed in different sessions. Again, this likely limited the increase in within-line social connectivity. These explanations do not preclude social ties from being a salient mechanism; they simply lower the likelihood that this channel generated workplace impacts.

6.5 Line-Level Productivity and Task Complexity Results

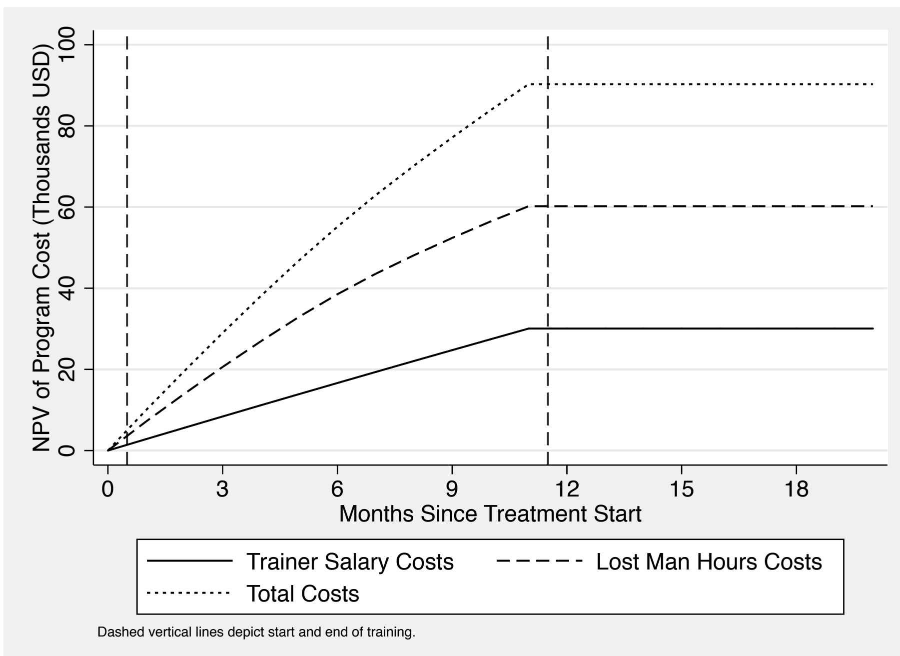
As a further test of robustness of our main results, we present regression results using daily productivity and task complexity at the production-line level instead of the individual-level.[20] Results are presented in Table A9. They are less precise since they include all workers on the line, not just treated workers, but very consistent with the individual-level results - the treatment effects for total garments produced and SAM are statistically significant at the 10% level after treatment, and the treatment effects for efficiency are positive with a p-value of 0.15. Treated lines produce about 45 garments These results provide further evidence that the main results are not driven by differential attrition rates by treatment. Furthermore, they indicate that the firm gains not only higher individual-level productivity from training the treated workers, but that these workers enable the entire production lines on which they produce to become more productive.

[20]Note that these results include all workers on the production line, not just those that signed up for the program.

7 Conclusion

In this paper, we evaluate the workplace impacts of general training in soft skills development. Consistent with theories of labor market imperfections and firms' gains from general training, we find that workers are substantially more productive after soft skills training, and that treated workers are no more likely to leave after training than controls. Wages rise by a very small amount after the program period, suggesting that the firm captures *most* of the gains from the increased marginal productivity of labor.

Figure 8: Total Program Costs Over Time



To quantify the total returns in terms of profit to the firm, we combine our treatment effect estimates on retention (person-days) and productivity with costing data obtained from the program administrators. We report in Table 8 calculations of the net-present-value costs and benefits. Benefits are calculated in terms of additional person days and incremental productivity from treated workers using estimates from the randomized evaluation.[21] We ignore spillover impacts and focus on direct treatment benefits only. This is primarily because the costs and benefits for individual trainees scale

[21] We ignore wage increases in these calculations as treatment impacts on salaries were negligible. In addition, we implicitly assume in calculating lost productivity due to reduced person days that the rate of hiring or worker replacement is common across treatment and control workers such that differential attrition produces truly lost person days. This is largely true as hiring is centralized for each factory unit, and even across factories with respect to migrant workers and new trainees. Accordingly, we are told that it is impossible for the rate of recruitment, hiring, and training to respond to differential turnover across lines within a factory and even to a degree across factories.

linearly, while spillover impacts are likely non-linear in the proportion of a line treated. (In addition to this, production complementarities are also likely non-linear and more salient when a larger fraction of the line is treated.) For this reason, to remain as conservative regarding benefits of the program as possible, we omit spillover impacts from the calculations that follow.

Figure 9: Cumulative Program Benefits Over Time

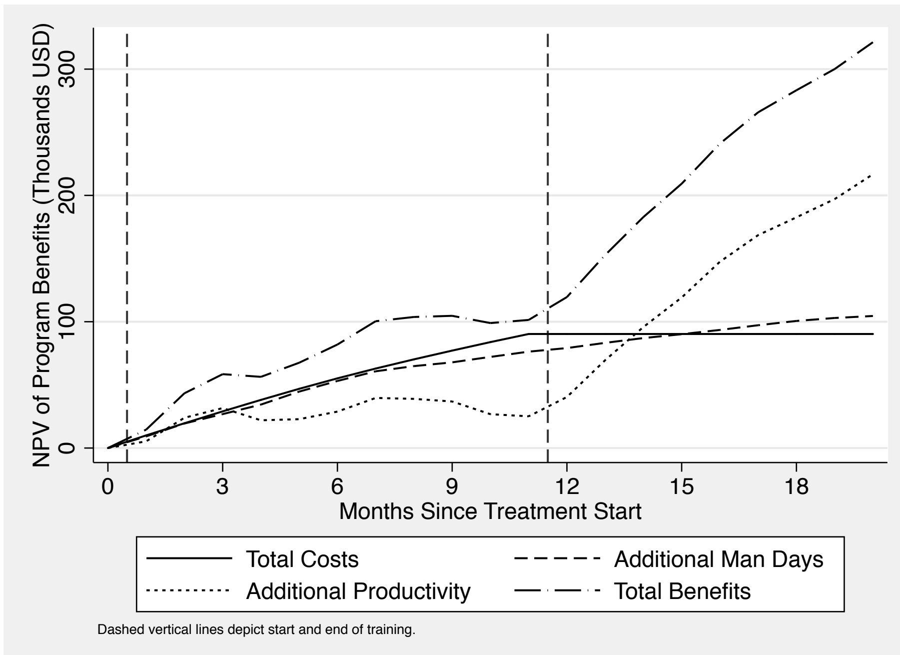


Table 8 first outlines costs of the program, both overhead costs and variable costs. The overhead costs are given entirely by the costs of hiring two full-time trainers per factory for the 11 months of the program. The variable costs are from lost production hours. For the 1,087 treated workers, total program costs are approximately $90,285, about $30,000 of which are overhead costs, and the remainder variable costs associated with lost hours. The time path of total costs in net present value (NPV) are shown in Figure 8. This figure shows that both components of total costs rise linearly during the program period, peaking at program completion.

Details on profit margins on additional revenue both from an additional person day and additional productivity, as well as additional revenue per garment were obtained from the firm. The benefits of the program accrue from the higher number of cumulative person days accrued to the firm and higher productivity. At the end of the program period, the NPV of these benefits is just over $100,000, about three-quarters of which is the result of person days gained due to differential attrition during the program. At the end of our tracking period (9 months after program completion), total benefits are

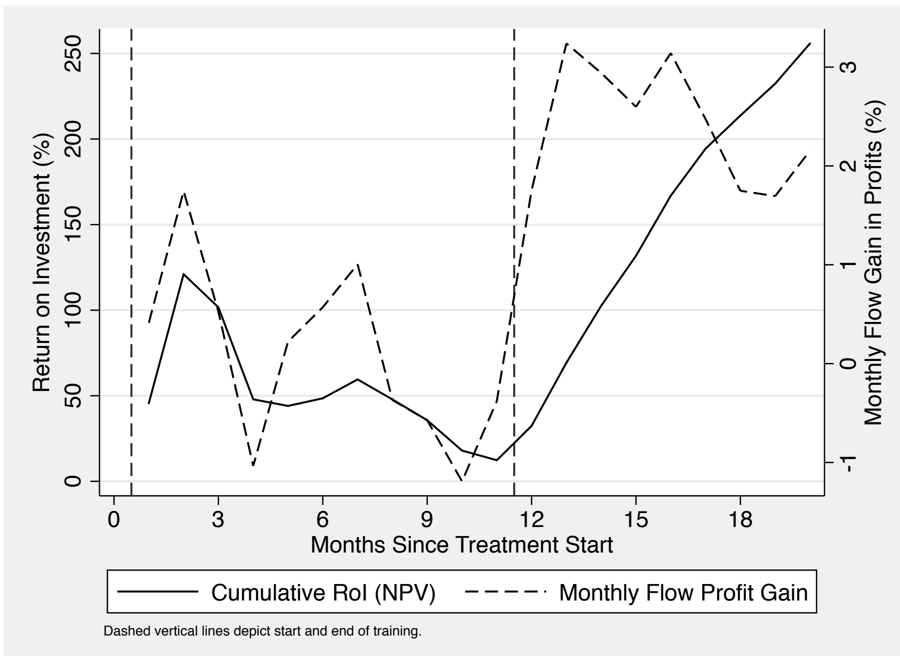
Table 8: Return on Investment Calculations (Costs and Benefits to Firm)

Sewing Department Only (1087 Treated Workers)	
P.A.C.E. Training Overhead Cost (2 Trainers per Factory for 11 Mos)	-$30,065.26
P.A.C.E. Training Variable Cost (Lost Garments from Lost Man Hours)	-$60,219.47
Total Cost *(All numbers in present value)*	-$90,284.73
1 Year After Program Announcement	
Additional Man Days	$76,308.65
Additional Productivity	$25,095.73
Net Present Value of Subtotal	$101,404.40
Net Rate of Return	**12%**
20 Mos After Program Announcement	
Additional Man Days (End of Observation)	$104,520.70
Additional Productivity (Garments per 8 hr day)	$216,767.10
Net Present Value of Subtotal	$321,287.80
Net Rate of Return	**256%**
Assumptions	
Additional Garments per Additional Man Day	8.3
Additional Revenue per Garment	$7.00
Labor Contribution to Cost ("Cut to Make")	30%
Profit Margin on Additional Revenue from Additional Productivity	24%
Profit Margin on Additional Revenue from Additional Man Day	6%
Interest Rate	7.5%
INR per 1 USD	60

Notes: Trainer salaries were 17,000 INR per year for each trainer. There were 2 trainers for each of the 5 factories; 10 trainers in total. Additional garments per additional man day is calculated by dividing the average worker level SAM (minutes to complete the operation on a single garment) by the line level SAM (minutes to complete a full garment for the line) and multiplying by 480 minutes in a work day. Additional revenue per garment is taken from the accounting department of the firm, as is the "Cut to Make" or labor percent contribution to total production cost. Profit margin on additional revenue generated through improved efficiency is calculated as 80% of the "Cut to Make" cost as instructed by the accounting office of the firm and the profit margin on additional revenue from an additional man day is equivalent to the average profit margin of the firm. The monthly interest rate is the average interest rate that prevailed during the study time period. Similarly, the exchange rate is the average from the study period.

substantially higher, more than $320,000. In the post-program period, returns via productivity gains dominate, accounting for more than two-thirds of the total benefits. Figure 9 plots the time path of cumulative benefits to the firm. Note that these returns accrued *net of* attrition – that is, we only count person days gained and productivity increases accruing to workers who were still present at each point in time.

Figure 10: Cumulative and Flow Return Over Time



The net rate of return *at the end of the program period* is thus 12%. That is, by the time the program ended, program costs had been recouped by the firm, plus 12 percent additional returns. Twenty months after program completion, flow benefits from post-program productivity impacts help generate a net rate of return of 256%. Figure 10 shows the time path of the cumulative and flow net rate of return. As depicted by the dashed line corresponding to the right axis, flow returns are roughly 2% at the end of the observation period, indicating that this cumulative return of 256% will likely continue to grow.

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Appendix: Not for publication.

A Additional Results

A.1 P.A.C.E. Program Details

Table A1 presents an overview of the modules included in the P.A.C.E. training program. The program spanned roughly 80 hours of training, but involved additional meetings for refresher sessions as well as introduction and conclusion sections. The core content sessions covered content regarding communication, problem-solving and decision-making, time and stress management, sanitation and hygiene, financial literacy, general and reproductive health, legal literacy and social entitlements, and execution excellence or intrinsic motivation.

A.2 Monthly Treatment Impacts and Additional Results

Tables A2 through A4 and A6 present month by month treatment effects on the main outcomes of interest analyzed in the paper. Tables A2, A3 and A4 present monthly treatment impacts for outcomes presented in Tables 2, 3, and 4 in the main results of the paper, respectively. Table A5 presents estimates of treatment impacts on additional outcomes from the attendance dataset in specifications similar to impacts shown in the main tables. We find no evidence of strong impacts of treatment on presence, unauthorized absence, or tardiness in any of the announcement, during, or after periods. Table A6 presents the monthly treatment effect analogues. We find that there are indeed significant positive impacts on workers being present in the factory and negative impacts on unauthorized absence in the first two months of the training. These effects dissipate quickly though, perhaps reflecting initial excitement more than long-lasting behavioral changes.

Table A1: P.A.C.E. Training Modules and Duration

Module Name	(Non-Exhaustive) Overview of Topics Covered	Aproximate Duration (hours)
Introductory Session	Ice-breaking games, overview of program topics and importance, program background and importance.	5
Communication	Basics and importance of communication, gender dynamics and bairriers in communication, communication in the workplace, home, and community.	9.5
Problem Solving and Decision Making (PSDM)	Basic concepts in PSDM, problem analysis and solution finding, creative thinking for solutions,, problem-solving in groups and accountability, consensus-building at work, home, and in the community.	13
Time and Stress Management	Time management, stress management (including some exercises for stress management), positive thinking	12
Water, Sanitation, and Hygiene (WASH)	Sanitary practices, the importance of clean water to health, rights of access to water	6
Financial Literacy	Importance of savings, financial planning tools, savings options	4.5
General and Reproductive Health	Nutrition, reproductive health, mental and emotional health	10
Legal Literacy and Social Entitlements	Basics of the legal system and structure, womens' legal rights	8.5
Execution Excellence	Important aspects of workplace excellence like attention to quality, teamwork, and timeliness.	5
Two Consolidation Sessions of 90 minutes each	Review sessions	3
Closing Session	Celebratory conclusion of the program	5

Table A2: Monthly Impacts of P.A.C.E. Treatment on Retention

	(1)	(2)	(3)	(4)	(5)	(6)
	Retained		Retained		Working	
	1(Worker Still on ***Attendance*** Roster)		1(Worker Still on ***Payroll*** Roster)		1(Worker Retained and Present in Factory Today)	
	(Whole Sample)	*(Sewing Dept Only)*	*(Whole Sample)*	*(Sewing Dept Only)*	*(Whole Sample)*	*(Sewing Dept Only)*
					Attendance Roster	***Production Data***
Announcement Month X Treatment	0.0406*	0.00416	0.0438*	0.00476	0.0303*	
	(0.0214)	(0.0136)	(0.0236)	(0.0153)	(0.0171)	
Treatment Month 1 X Treatment	0.0375	0.00218	0.0376	-0.00171	0.0372**	0.0936**
	(0.0229)	(0.0157)	(0.0254)	(0.0171)	(0.0182)	(0.0383)
Treatment Month 2 X Treatment	0.0500**	0.0224	0.0492*	0.0182	0.0467**	0.109***
	(0.0229)	(0.0174)	(0.0257)	(0.0184)	(0.0205)	(0.0339)
Treatment Month 3 X Treatment	0.0605**	0.0363*	0.0618**	0.0354*	0.0490**	0.0819**
	(0.0235)	(0.0187)	(0.0263)	(0.0192)	(0.0201)	(0.0365)
Treatment Month 4 X Treatment	0.0682***	0.0425**	0.0652**	0.0366*	0.0603***	0.0867***
	(0.0242)	(0.0198)	(0.0268)	(0.0205)	(0.0201)	(0.0316)
Treatment Month 5 X Treatment	0.0876***	0.0630***	0.0909***	0.0633***	0.0666***	0.124***
	(0.0242)	(0.0217)	(0.0274)	(0.0219)	(0.0207)	(0.0359)
Treatment Month 6 X Treatment	0.0806***	0.0571**	0.0840***	0.0587**	0.0660***	0.110***
	(0.0248)	(0.0249)	(0.0276)	(0.0253)	(0.0199)	(0.0347)
Treatment Month 7 X Treatment	0.0727***	0.0440	0.0768***	0.0465	0.0470**	0.111***
	(0.0259)	(0.0284)	(0.0284)	(0.0294)	(0.0202)	(0.0377)
Treatment Month 8 X Treatment	0.0591**	0.0256	0.0635**	0.0264	0.0360*	0.0654
	(0.0259)	(0.0284)	(0.0289)	(0.0295)	(0.0213)	(0.0408)
Treatment Month 9 X Treatment	0.0469*	0.0177	0.0496*	0.0182	0.0284	0.0505
	(0.0253)	(0.0283)	(0.0280)	(0.0292)	(0.0207)	(0.0379)
Treatment Month 10 X Treatment	0.0413	0.0104	0.0459	0.0123	0.0280	0.0779**
	(0.0255)	(0.0286)	(0.0283)	(0.0295)	(0.0195)	(0.0372)
Treatment Month 11 X Treatment	0.0286	-0.00244	0.0302	-0.00297	0.0122	0.0799**
	(0.0254)	(0.0287)	(0.0281)	(0.0299)	(0.0198)	(0.0388)
Post Treatment Month 1 X Treatment	0.0317	-0.00164	0.0327	-0.00395	0.0118	0.0577
	(0.0250)	(0.0277)	(0.0282)	(0.0289)	(0.0203)	(0.0369)
Post Treatment Month 2 X Treatment	0.0358	0.00534	0.0374	0.00418	0.0180	0.0884**
	(0.0242)	(0.0270)	(0.0269)	(0.0279)	(0.0208)	(0.0377)
Post Treatment Month 3 X Treatment	0.0377	0.00849	0.0399	0.00712	0.0209	0.0834**
	(0.0238)	(0.0274)	(0.0271)	(0.0285)	(0.0199)	(0.0408)
Post Treatment Month 4 X Treatment	0.0364	0.0105	0.0387	0.0101	0.0236	0.0691*
	(0.0234)	(0.0269)	(0.0262)	(0.0277)	(0.0203)	(0.0388)
Post Treatment Month 5 X Treatment	0.0356	0.00916	0.0365	0.00836	0.0202	0.0777**
	(0.0233)	(0.0267)	(0.0262)	(0.0276)	(0.0199)	(0.0382)
Post Treatment Month 6 X Treatment	0.0392*	0.0107	0.0413	0.00955	0.0232	0.0895**
	(0.0236)	(0.0268)	(0.0266)	(0.0277)	(0.0191)	(0.0396)
Post Treatment Month 7 X Treatment	0.0372	0.0103	0.0405	0.0125	0.0231	0.0872**
	(0.0236)	(0.0266)	(0.0263)	(0.0274)	(0.0191)	(0.0395)
Post Treatment Month 8 X Treatment	0.0293	-0.00220			0.0117	0.0655*
	(0.0235)	(0.0259)			(0.0196)	(0.0394)
Post Treatment Month 9 X Treatment	0.0191	-0.00333			0.000313	0.0458
	(0.0206)	(0.0210)			(0.0175)	(0.0401)
Fixed Effects			Unit X Month X Year, Worker			
Observations	2,078,400	1,433,981	62,585	43,141	1,848,003	778,916
Control Mean of Dependent Variable	0.589	0.628	0.619	0.656	0.480	0.367

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Standard errors are clustered at the treatment line level. Retained dummy is defined for every worker date observation in the data and therfore regressions do not require any weighting.

Table A3: Monthly Impacts of P.A.C.E. Treatment on Productivity and Task Complexity

	(1)	(2)	(3)
	Efficiency Mean(Produced/Target)	Pieces Produced Mean(Pieces per Hour)	SAM (Operation Complexity) Mean(Standard Allowable Minute)
Treatment Month 1 X Treatment	0.0194	1.839	0.0102
	(0.0157)	(1.858)	(0.0178)
Treatment Month 2 X Treatment	0.0364	3.213	0.0295
	(0.0248)	(2.840)	(0.0186)
Treatment Month 3 X Treatment	0.0306	3.321	0.0238
	(0.0264)	(2.796)	(0.0202)
Treatment Month 4 X Treatment	0.00756	0.211	0.0196
	(0.0312)	(3.519)	(0.0196)
Treatment Month 5 X Treatment	0.0235	1.056	0.0264
	(0.0327)	(3.787)	(0.0170)
Treatment Month 6 X Treatment	0.0182	0.686	0.0371**
	(0.0333)	(3.925)	(0.0167)
Treatment Month 7 X Treatment	0.0445	3.204	0.0420**
	(0.0353)	(4.209)	(0.0186)
Treatment Month 8 X Treatment	0.0225	-0.172	0.0444**
	(0.0322)	(3.816)	(0.0192)
Treatment Month 9 X Treatment	0.0314	0.935	0.0424**
	(0.0347)	(4.271)	(0.0195)
Treatment Month 10 X Treatment	0.0213	1.520	0.0387**
	(0.0402)	(4.608)	(0.0169)
Treatment Month 11 X Treatment	0.0315	3.313	0.0520***
	(0.0458)	(5.365)	(0.0168)
Post Treatment Month 1 X Treatment	0.0737	7.989	0.0351*
	(0.0448)	(5.469)	(0.0205)
Post Treatment Month 2 X Treatment	0.0959*	9.805*	0.0285
	(0.0499)	(5.794)	(0.0247)
Post Treatment Month 3 X Treatment	0.0993*	9.560	0.0499**
	(0.0519)	(6.004)	(0.0208)
Post Treatment Month 4 X Treatment	0.104**	10.16*	0.0278
	(0.0515)	(5.853)	(0.0213)
Post Treatment Month 5 X Treatment	0.109**	11.26*	0.0344
	(0.0517)	(6.145)	(0.0237)
Post Treatment Month 6 X Treatment	0.119**	12.03*	0.0472*
	(0.0567)	(6.605)	(0.0244)
Post Treatment Month 7 X Treatment	0.116**	11.79*	0.0452**
	(0.0569)	(6.507)	(0.0207)
Post Treatment Month 8 X Treatment	0.112*	10.54	0.0411*
	(0.0585)	(6.683)	(0.0232)
Post Treatment Month 9 X Treatment	0.111*	10.39	0.0333
	(0.0607)	(7.046)	(0.0224)
Additional Controls	Days on Same Line-Garment, Total Order Size	Days on Same Line-Garment, Total Order Size, Target Pieces	None
Fixed Effects	Unit X Month X Year, Worker X Garment		Unit X Month X Year, Worker
Weights	Inverse Predicted Probability from Probit of Working on Treatments X Mo-Yr X Baseline Characteristics		
Observations	290,763	290,763	290,763
Control Mean of Dependent Variable	0.542	62.250	0.565

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Standard errors are clustered at the treatment line level. Productivity, promotion, and are weighted in regressions by the inverse of the predicted probability of working (i.e., not yet attrited and present in the factory with non-missing data) in the sample that day from a probit regression of the working dummy on month by year FE and their interaction with individual and line treatment dummies and baseline variables reported in Table 1.

Table A4: Monthly Impacts of P.A.C.E. Treatment on Cumulative Person Days and Salary

	(1)	(2)	(3)
	Cumulative Person Days Sum of Days Working for Each Worker to Date		log(Gross Salary)
	Attendance Roster	***Production Data***	
	(Whole Sample)	*(Sewing Dept Only)*	*(Sewing Dept Only)*
Announcement Month X Treatment	-1.063		0.000210
	(4.946)		(0.000648)
Treatment Month 1 X Treatment	0.266	-0.439	0.000167
	(5.045)	(0.820)	(0.000685)
Treatment Month 2 X Treatment	1.346	1.215	0.000343
	(4.919)	(1.136)	(0.000758)
Treatment Month 3 X Treatment	2.991	2.706**	0.000440
	(4.626)	(1.334)	(0.000768)
Treatment Month 4 X Treatment	4.449	3.531**	0.000446
	(4.477)	(1.578)	(0.000762)
Treatment Month 5 X Treatment	6.581	4.971**	0.000604
	(4.571)	(1.923)	(0.000913)
Treatment Month 6 X Treatment	8.755*	6.762***	0.000572
	(4.467)	(2.295)	(0.000930)
Treatment Month 7 X Treatment	10.84**	8.589***	0.000780
	(4.523)	(2.767)	(0.000903)
Treatment Month 8 X Treatment	12.37**	9.682***	0.000983
	(4.754)	(3.268)	(0.000950)
Treatment Month 9 X Treatment	13.44***	9.898**	0.000956
	(4.998)	(3.781)	(0.000930)
Treatment Month 10 X Treatment	14.65***	10.58**	0.00410
	(5.381)	(4.261)	(0.00267)
Treatment Month 11 X Treatment	15.47***	11.45**	0.00443
	(5.849)	(4.655)	(0.00276)
Post Treatment Month 1 X Treatment	16.11**	12.16**	0.00466*
	(6.356)	(5.102)	(0.00279)
Post Treatment Month 2 X Treatment	16.74**	12.95**	0.00547*
	(6.911)	(5.570)	(0.00286)
Post Treatment Month 3 X Treatment	17.64**	14.12**	0.00495*
	(7.431)	(6.045)	(0.00289)
Post Treatment Month 4 X Treatment	18.70**	14.89**	0.00483*
	(8.096)	(6.529)	(0.00291)
Post Treatment Month 5 X Treatment	19.30**	15.66**	0.00506*
	(8.603)	(6.995)	(0.00291)
Post Treatment Month 6 X Treatment	20.25**	16.46**	0.00554*
	(9.064)	(7.499)	(0.00296)
Post Treatment Month 7 X Treatment	21.19**	17.86**	0.00588*
	(9.585)	(7.916)	(0.00305)
Post Treatment Month 8 X Treatment	22.15**	18.69**	
	(10.04)	(8.392)	
Post Treatment Month 9 X Treatment	22.74**	19.06**	
	(10.76)	(8.754)	
Fixed Effects	Unit X Month X Year, Worker		
Weights	None		Inverse Predicted Probability from Probit of Working on Treatments X Mo-Yr X Baseline Characteristics
Observations	1,848,003	778,916	28,692
Control Mean of Dependent Variable	201.408	103.220	8.771

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Standard errors are clustered at the treatment line level. Cumulative man days are both defined for every worker date observation in the data and therfore regressions do not require any weighting. Probability of promotion is weighted in regressions by the inverse of the predicted probability of working (i.e., not yet attrited and present in the factory with non-missing data) in the sample that day from a probit regression of the working dummy on month by year FE and their interaction with individual and line treatment dummies and baseline variables reported in Table 1.

Table A5: Impacts of P.A.C.E. Treatment on Presence, Unauthorized Absence, and Tardiness

	(1)	(2)	(3)	(4)	(5)	(6)
	Present		Unauthorized Absent		Tardy	
	1(Worker Present in Factory Today if Stilll on Attendance Roster)		1(Worker Absent without Leave Today if Still on Attendance Roster)		1(Worker Arrived Late Today Relative to Other Workers on Line)	
	(Whole Sample)	*(Sewing Dept Only)*	*(Whole Sample)*	*(Sewing Dept Only)*	*(Whole Sample)*	*(Sewing Dept Only)*
After X P.A.C.E. Treatment	0.00465	0.00485	-0.00925	-0.00940	-0.0208	-0.0207
	(0.00820)	(0.00819)	(0.00717)	(0.00716)	(0.0145)	(0.0145)
During X P.A.C.E. Treatment	0.00770	0.00791	-0.00701	-0.00717	0.00138	0.00145
	(0.00596)	(0.00592)	(0.00582)	(0.00580)	(0.0123)	(0.0123)
Announced X P.A.C.E.. Treatment	0.00971	0.00999	-0.0107	-0.0109	0.00421	0.00428
	(0.0106)	(0.0106)	(0.0107)	(0.0106)	(0.00971)	(0.00969)
Fixed Effects	Unit X Month X Year, Worker					
Weights	Inverse Predicted Probability from Probit of Retention on Treatments X Mo-Yr X Baseline Characteristics					
Observations	822,488	736,439	822,488	736,439	668,489	602,178
Control Mean of Dependent Variable	0.889	0.893	0.100	0.097	0.385	0.394

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Standard errors are clustered at the treatment line level. Observations are weighted in regressions by the inverse of the predicted probability of being retained (i.e., not yet attrited with non-missing data) in the sample that day from a probit regression of the retained dummy on month by year FE and their interaction with individual and line treatment dummies and baseline variables reported in Table 1.

Table A6: Monthly Impacts of P.A.C.E. Treatment on Presence, Unauthorized Absence, and Tardiness

	(1)	(2)	(3)	(4)	(5)	(6)
	Present		Unauthorized Absent		Tardy	
	1(Worker Present in Factory Today if Stilll on Attendance Roster)		1(Worker Absent without Leave Today if Still on Attendance Roster)		1(Worker Arrived Late Today Relative to Other Workers on Line)	
	(Whole Sample)	*(Sewing Dept Only)*	*(Whole Sample)*	*(Sewing Dept Only)*	*(Whole Sample)*	*(Sewing Dept Only)*
Announcement Month X Treatment	0.00983	0.0101	-0.0108	-0.0111	0.00268	0.00245
	(0.0106)	(0.0106)	(0.0107)	(0.0106)	(0.00968)	(0.00973)
Treatment Month 1 X Treatment	0.0240**	0.0242**	-0.0214*	-0.0216*	-0.00772	-0.00792
	(0.0110)	(0.0109)	(0.0114)	(0.0113)	(0.0121)	(0.0121)
Treatment Month 2 X Treatment	0.0176**	0.0179**	-0.0195**	-0.0197**	0.00588	0.00569
	(0.00871)	(0.00871)	(0.00831)	(0.00832)	(0.0129)	(0.0129)
Treatment Month 3 X Treatment	0.00893	0.00915	-0.00518	-0.00535	0.00916	0.00897
	(0.00831)	(0.00825)	(0.00822)	(0.00818)	(0.0137)	(0.0137)
Treatment Month 4 X Treatment	0.0142	0.0145	-0.0157	-0.0158	-0.00196	-0.00212
	(0.0108)	(0.0108)	(0.0108)	(0.0109)	(0.0150)	(0.0151)
Treatment Month 5 X Treatment	-0.00181	-0.00160	0.00227	0.00210	-0.000873	-0.00106
	(0.0110)	(0.0110)	(0.0112)	(0.0112)	(0.0173)	(0.0172)
Treatment Month 6 X Treatment	0.0141	0.0143	-0.0131	-0.0132	-0.00187	-0.00205
	(0.0138)	(0.0138)	(0.0139)	(0.0139)	(0.0165)	(0.0165)
Treatment Month 7 X Treatment	-0.000808	-0.000614	-0.00131	-0.00145	-0.0143	-0.0144
	(0.0157)	(0.0156)	(0.0139)	(0.0140)	(0.0179)	(0.0179)
Treatment Month 8 X Treatment	-0.0144	-0.0142	0.0198*	0.0197	-0.00901	-0.00922
	(0.0130)	(0.0130)	(0.0119)	(0.0119)	(0.0196)	(0.0196)
Treatment Month 9 X Treatment	0.00108	0.00128	0.00417	0.00402	-0.00583	-0.00602
	(0.0135)	(0.0135)	(0.0115)	(0.0115)	(0.0198)	(0.0197)
Treatment Month 10 X Treatment	0.00156	0.00175	-0.00739	-0.00753	0.00277	0.00261
	(0.0124)	(0.0124)	(0.0104)	(0.0104)	(0.0213)	(0.0213)
Treatment Month 11 X Treatment	0.00735	0.00755	-0.00681	-0.00695	-0.00573	-0.00588
	(0.0121)	(0.0121)	(0.0113)	(0.0113)	(0.0228)	(0.0228)
Post Treatment Month 1 X Treatment	-0.000112	8.24e-05	-0.00477	-0.00491	-0.00941	-0.00954
	(0.0161)	(0.0161)	(0.0147)	(0.0147)	(0.0190)	(0.0190)
Post Treatment Month 2 X Treatment	-0.00318	-0.00298	-0.00266	-0.00281	-0.0179	-0.0181
	(0.0151)	(0.0151)	(0.0130)	(0.0130)	(0.0175)	(0.0175)
Post Treatment Month 3 X Treatment	0.00704	0.00724	-0.00848	-0.00863	-0.0241	-0.0242
	(0.0109)	(0.0109)	(0.0104)	(0.0104)	(0.0193)	(0.0193)
Post Treatment Month 4 X Treatment	0.00848	0.00868	-0.0128	-0.0129	-0.0193	-0.0194
	(0.00981)	(0.00983)	(0.00796)	(0.00797)	(0.0204)	(0.0204)
Post Treatment Month 5 X Treatment	0.00637	0.00656	-0.00519	-0.00533	-0.00651	-0.00664
	(0.0146)	(0.0146)	(0.0127)	(0.0127)	(0.0217)	(0.0217)
Post Treatment Month 6 X Treatment	0.0210	0.0212	-0.0250	-0.0251	-0.0172	-0.0174
	(0.0155)	(0.0155)	(0.0153)	(0.0153)	(0.0175)	(0.0175)
Post Treatment Month 7 X Treatment	0.000175	0.000388	-0.00387	-0.00403	-0.0303	-0.0304
	(0.0135)	(0.0135)	(0.0117)	(0.0116)	(0.0214)	(0.0214)
Post Treatment Month 8 X Treatment	-0.0121	-0.0119	-0.00405	-0.00420	-0.0285	-0.0286
	(0.0161)	(0.0161)	(0.0116)	(0.0116)	(0.0230)	(0.0230)
Post Treatment Month 9 X Treatment	-0.000300	-9.74e-05	-0.00218	-0.00233	-0.0117	-0.0119
	(0.0174)	(0.0175)	(0.0144)	(0.0144)	(0.0249)	(0.0249)
Fixed Effects	Unit X Month X Year, Worker					
Weights	Inverse Predicted Probability from Probit of Retention on Treatments X Mo-Yr X Baseline Characteristics					
Observations	822,488	736,439	822,488	736,439	624,622	563,624
Control Mean of Dependent Variable	0.889	0.893	0.100	0.097	0.342	0.367

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Standard errors are clustered at the treatment line level. Observations are weighted in regressions by the inverse of the predicted probability of being retained (i.e., not yet attrited with non-missing data) in the sample that day from a probit regression of the retained dummy on month by year FE and their interaction with individual and line treatment dummies and baseline variables reported in Table 1.

Table A7: Production Complementarities Within Line in Pre-Training Period (June 2013)

	(1)	(2)	(3)
	Efficiency	Pieces Produced	SAM (Operation Complexity)
	Mean(Produced/Target)	Mean(Pieces per Hour)	Mean(Standard Allowable Minute)
Co-Worker Efficiency	0.286***		
	(0.0309)		
Co-Worker Pieces Produced		0.207***	
		(0.0259)	
Co-Worker SAM			0.0339***
			(0.0114)
Fixed Effects	Worker X Garment		Worker
Observations	705,523	705,523	705,523
Raw Correlation	0.561	0.466	0.266

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Standard errors are clustered at the treatment line level. Sample reflects only the first month of production data prior to the start of the program. Data matches all workers productivity in a day to all other workers in the line that day.

A.3 Production Complementarities

Table A7 presents evidence of the empirical magnitudes of production complementarities in the pre-training period to help to interpret the large spillover effects estimated in the paper. We find that technical complementarities can explain at most 29 percent of coincident variation in productivity of co-workers on a line in a given day. That is, regressing efficiency of a worker on the contemporaneous efficiency of her co-workers on the line, after controlling for the same controls used in the production regressions estimated in the main results (namely, worker by garment fixed effects), produces a partial correlation of .286. That is, a change in efficiency of co-workers of 10 percentage points would increase a worker's own efficiency by 2.6 percentage points through technical complementarities alone. Analogous regressions for pieces produced reveals an even smaller partial correlation of .207. This means that if a worker produces an extra garment per hour, her co-workers would be enabled to produce only .2 extra garments per hour by way of complementarities.

While this is indeed evidence of a significant technical complementarity in production across workers within a line, the spillover impacts we find are on the order of 90% of the magnitude of the direct effects on production outcomes. This leads us to interpret the spillover impacts of evidence of true skill diffusion above and beyond any technical complementarities. Furthermore, similar analysis of operation complexity (SAM) across co-workers indicates that co-workers on a line can indeed be assigned to operations of vastly different complexity contemporaneously on the same garment. This supports the interpretation that even spillover impacts on complexity of the assigned task are not simply due to common line assignment or garment orders, but rather reflect coincident evolution in capabilities across P.A.C.E. workers and their untreated co-workers.

A.4 Correction for Multiple Hypothesis Testing

In Table A8, we re-estimate the direct impacts of the P.A.C.E program on the main outcomes, correcting for multiple hypothesis testing. The regression specifications are identical to the analogous regressions in the main tables; however, in place of standard errors, we report (corrected) q-values (false discovery rates) in parentheses in this table. Each panel of the table corresponds to a set of hypothesis - for instance, we test all the productivity outcomes (efficiency, pieces produced, and operation complexity) as one set of hypotheses, all workplace survey outcomes as another set of hypotheses, and so on. To correct the p-values for multiple hypothesis testing, we follow Anderson (2008) who recommends using the methodology of Benjamini and Hochberg (1995). This method controls the False Discovery Rate (FDR) at level q when there are M hypothesis to be tested (say $H_1, ...H_M$), by sorting the corresponding p-values in increasing order ($p_1 < ...p_M$), and rejecting c hypotheses such that c is the largest w where $pw < (qw/M)$.[22].

Overall, the significance of the main results is preserved for the set of workplace outcomes, albeit less so with the non-workplace survey outcomes. The retention and productivity impacts exhibit almost no differences in significance in Panels A and B, respectively, when the corrections for multiple hypothesis are done.[23] Workplace survey outcomes in Panel C and government and firm entitlements in Panel E also show very similar significance to the main results. Outcomes in Panels D, E and F show small increases in p-values (or q-values) to levels slightly higher than conventional levels of significance. For example, in the set of behaviors related to financial behaviors and attitudes, the positive impact on savings for children's education is significant at the 10% level in the main results, and the p-value increases to 0.21 after the multiple hypothesis testing correction; while, the set of personality outcomes produces a marginally insignificant positive impact of P.A.C.E. on extraversion with p-value of .103 after the correction is applied, as compared to an estimate that was significant at the 5% level in the main results. As in the uncorrected regressions, there are no statistically significant impacts on

[22]To implement this procedure, we use the Stata code available here: `https://are.berkeley.edu/~mlanderson/ARE_Website/Research.html`

[23]We report working and person day outcomes from the attendance dataset only for brevity, but similar equivalence is obtained when analyzing production data analogues

Table A8: Robustness to Corrections for Multiple Hypothesis Testing (Anderson, 2008)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Retention and Worker Presence	Retained		Working		Cumulative Man Days	
	(Whole Sample)	*(Sewing Dept Only)*	*(Whole Sample)*	*(Sewing Dept Only)*	*(Whole Sample)*	*(Sewing Dept Only)*
After X P.A.C.E. Treatment	0.0337	0.0053	0.0170	0.00635	19.44*	9.1712
	(0.219)	(0.836)	(0.373)	(0.836)	(0.071)	(0.835)
During X P.A.C.E. Treatment	0.0575**	0.0289	0.0431**	0.0313	8.410*	5.0686
	(0.02)	(0.175)	(0.02)	(0.156)	(0.052)	(0.156)
Announced X P.A.C.E.. Treatment	0.0406	0.004	0.0303	0.0136	-1.058	0.5013
	(0.119)	(0.762)	(0.119)	(0.762)	(0.831)	(0.762)
Panel B: Productivity	Efficiency	Pieces Produced	SAM (Operation Complexity)			
After X P.A.C.E. Treatment	0.0681**	7.272**	0.0334*			
	(0.045)	(0.045)	(0.067)			
During X P.A.C.E. Treatment	0.0203	1.926	0.0320*			
	(0.282)	(0.288)	(0.093)			
Panel C: Workplace Survey Outcomes	Expect Promotion Next 6 Mos	Skill Development Training	Production Award or Incentive	Peer Self-Assessment	Line Co-Worker Self-Assessment	
P.A.C.E. Treatment	0.0767	0.148**	0.0281	0.0784	0.13	
	(0.129)	(0.015)	(0.162)	(0.258)	(0.115)	
Panel D: Financial Behaviors and Attitudes	Saving for Education	Saving for Other Reasons	Risk and Time Preference Index	Insurance	Informal Borrow or Lend	
P.A.C.E. Treatment	0.0607	-0.0332	-0.154	0.00742	0.0235	
	(0.212)	(0.608)	(0.212)	(0.852)	(0.747)	
Panel B: Government and Firm Entitlements	Gov. Pension	Gov. Subsidized Healthcare	Other Gov. Subsidy	Firm Entitlements	Community Self Help Group	
P.A.C.E. Treatment	0.0232	0.0221	0.00746	-0.0303	-0.0346	
	(0.235)	(0.132)	(0.804)	(0.455)	(0.453)	
Panel F: Personality	Conscientiousness	Locus of Control	Perserverance	Extraversion	Self-Sufficiency	
P.A.C.E. Treatment	0.0530	0.0264	-0.105	0.159	0.0383	
	(0.739)	(0.739)	(0.619)	(0.103)	(0.739)	
Panel D: Mental Health and Aspirations	Self-Esteem	Hope/Optimism	Moderate Distress	Child's Expected Age at Marriage	Child Educated Beyond College	
P.A.C.E. Treatment	-0.158	-0.0634	-0.0419	0.0793	0.0808**	
	(0.415)	(0.563)	(0.463)	(0.637)	(0.024)	

Notes: p-values adjusted for multiple hypothesis testing, q-values (false discovery rates) in parentheses (*** q<0.01, ** q<0.05, * q<0.1). Standard errors are clustered at the treatment line level. The methodology from Anderson (2008) was used to correct for multple hypothesis testing. Specifications are otherwise identical to analogous regressions in main results tables. For conciseness, weights, fixed effects, and controls are not mentioned here, but are included in regressions where noted in analogous main tables. Similarly, observations and control means of dependent variables are omitted as well, but identical to those from main tables.

mental health, but the impact on aspirations for one's children remains positive and strongly statistically significant.

A.5 Productivity and Task Complexity Results at the Production-Line Level

Table A9: Impact of P.A.C.E. Treatment on Daily Productivity and Task Complexity at the Production-Line Level

	(1)	(2)	(3)
	Total Pieces Produced	Efficiency	SAM (Operation Complexity)
	Mean(Pieces per Hour)	Mean(Produced/Target)	Mean(Standard Allowable Minute)
After X P.A.C.E. Treatment	44.92*	0.0318	0.0312*
	(26.71)	(0.0255)	(0.0177)
During X P.A.C.E. Treatment	31.02	0.00739	0.0217
	(23.27)	(0.0152)	(0.0143)
Additional Controls	Days on Same Line-Garment, Total Order Size, Target Quantity	Days on Same Line-Garment, Total Order Size	None
Fixed Effects		Unit X Month X Year, Line	
Weights		None	
Observations	84,335	84,335	84,335
Control Mean of Dependent Variable	320.3	0.510	0.572

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Standard errors are clustered at the treatment line level.

A.6 Balance Tests by Baseline Characteristics at Different Points During and Post-Treatment

Table A10: Summary Statistics: Balance Checks for Baseline Characteristics at Different Points in Time

	(1)		(2)		(3)	
	One Month Post Treatment (June 2014)					
	Control		Treated		Difference	
P.A.C.E. Treatment (Sewing Department)	**Control Workers in Control Lines**		**Treated Workers in Treatment Lines**			
Number of workers	364		512			
	Mean	SD	Mean	SD	Mean Difference	p value
Attendance Rate (Jan-May 2013)	0.916	0.070	0.918	0.073	-0.002	0.732
1(High Education)	0.577	0.495	0.581	0.494	-0.004	0.912
Years of Tenure	1.755	1.404	1.563	1.225	0.192	0.172
Age	30.135	10.201	28.719	8.326	1.415	0.105
1(Speaks Kannada)	0.703	0.457	0.686	0.465	0.017	0.800
High Skill Grade	0.354	0.479	0.347	0.476	0.007	0.870
log(Salary) (May 2013)	8.769	0.126	8.756	0.111	0.013	0.206
Efficiency (Announcement Month)	0.598	0.154	0.562	0.154	0.037	0.194
SAM (Announcement Month)	0.637	0.279	0.629	0.230	0.007	0.834
Spillover Treatment (Sewing Department)	**Control Workers in Control Lines**		**Control Workers in Treatment Lines**			
Number of workers	364		367			
	Last Month of Data Collection (February 2015)					
	Control		Treated		Difference	
P.A.C.E. Treatment (Sewing Department)	**Control Workers in Control Lines**		**Treated Workers in Treatment Lines**			
Number of workers	263		373			
	Mean	SD	Mean	SD	Mean Difference	p value
Attendance Rate (Jan-May 2013)	0.914	0.069	0.918	0.073	-0.004	0.487
1(High Education)	0.540	0.499	0.552	0.498	-0.012	0.779
Years of Tenure	1.694	1.259	1.652	1.184	0.042	0.765
Age	30.156	9.103	29.402	8.146	0.754	0.280
1(Speaks Kannada)	0.738	0.441	0.713	0.453	0.025	0.712
High Skill Grade	0.357	0.480	0.357	0.480	0.001	0.987
log(Salary) (May 2013)	8.775	0.131	8.763	0.116	0.013	0.337
Efficiency (Announcement Month)	0.598	0.145	0.565	0.155	0.033	0.232
SAM (Announcement Month)	0.653	0.299	0.631	0.239	0.022	0.570
Spillover Treatment (Sewing Department)	**Control Workers in Control Lines**		**Control Workers in Treatment Lines**			
Number of workers	263		267			

Notes: Tests of differences calculated using errors clustered at the line level according to the experimental design.

A.7 Heterogeneous Retention by Distribution of Baseline Characteristic

Figures A1 through A5 plot estimates and standard errors of treatment effects on retention for each period (i.e., announced, during, after) at equally spaced points along the distribution of baseline balance variables. These plots are meant to explore the possibility that retention, and therefore sample composition for subsequent outcomes such as productivity, are heterogeneous across the distribution of baseline characteristics of workers. If this were the case, we might be concerned that the current weighting procedure used in the empirical analysis in this paper is insufficient in addressing sample selection bias over time in the sample in that these weights correct only for differences in mean values of these variables across retained treatment and control workers for each month of observation. We find no evidence at all of differential retention along the distribution of any of these baseline characteristics at any point in the observation period. This provides strong support of the sufficiency of the current weighting procedure used in the analysis.

Figure A1: Retention Impacts by Baseline Attendance

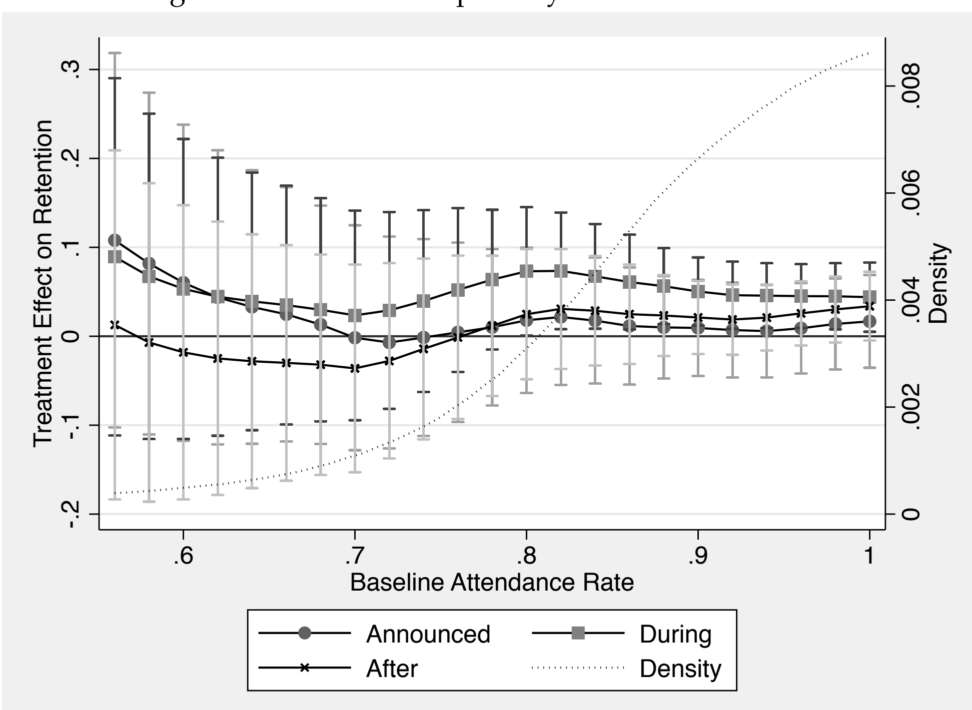

Figure A1 depicts impacts of P.A.C.E. treatment on retention along the distribution of baseline attendance.

Figure A2: Retention Impacts by Baseline Tenure

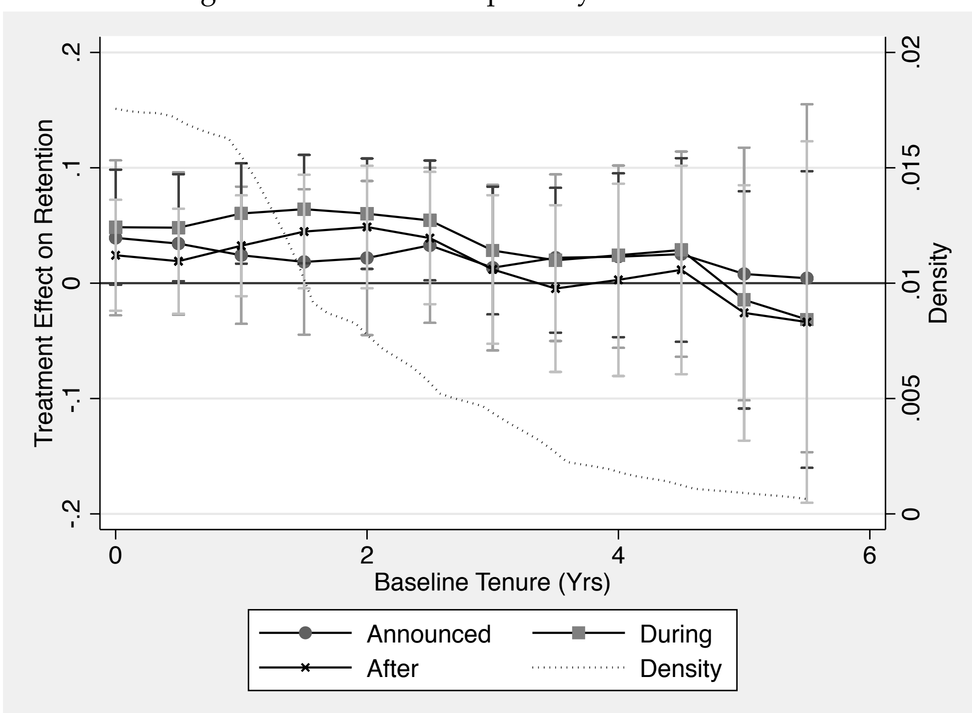

Figure A1 depicts impacts of P.A.C.E. treatment on retention along the distribution of tenure at baseline.

Figure A3: Retention Impacts by Baseline Skill Level

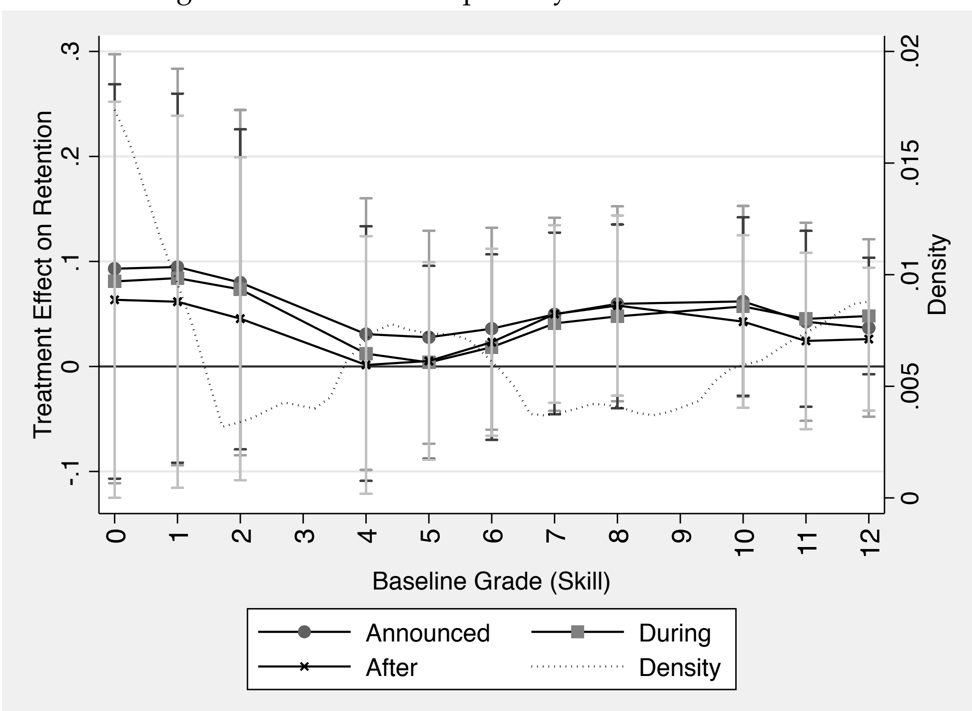

Figure A1 depicts impacts of P.A.C.E. treatment on retention along the distribution of skill grade at baseline.

Figure A4: Retention Impacts by Baseline Education

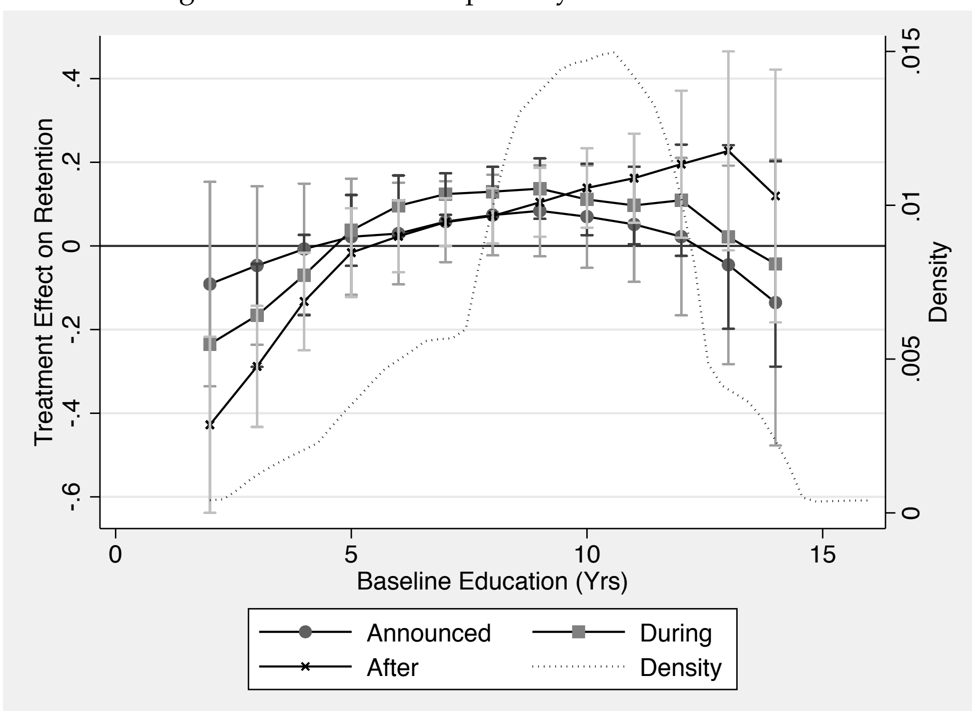

Figure A1 depicts impacts of P.A.C.E. treatment on retention along the distribution of education at baseline.

Figure A5: Retention Impacts by Baseline Age

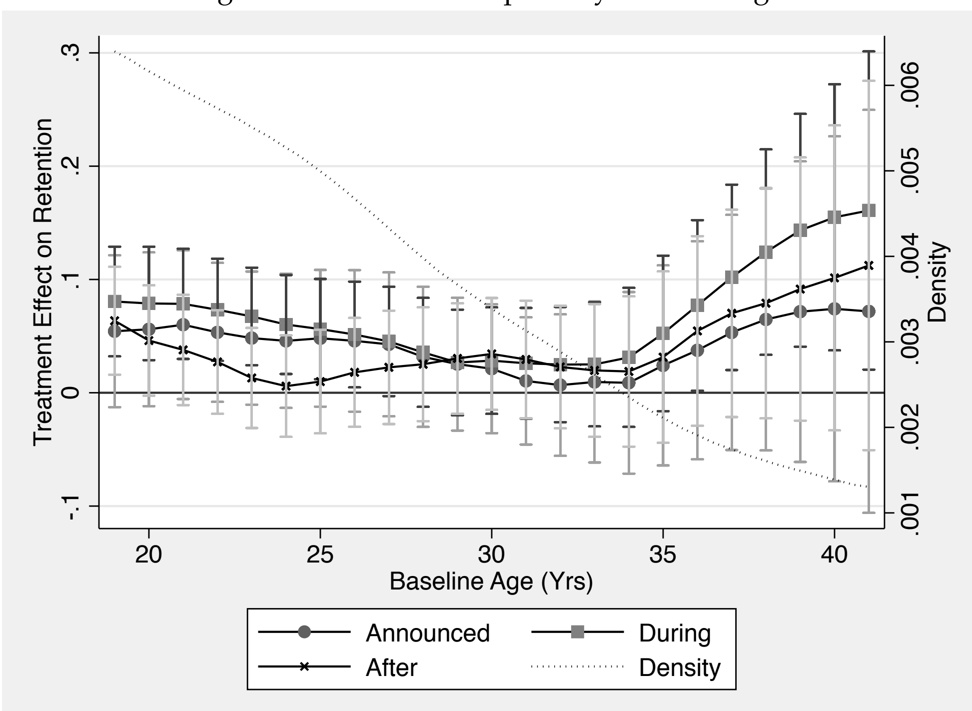

Figure A1 depicts impacts of P.A.C.E. treatment on retention along the distribution of age at baseline.

B Data Appendix

B.1 Retention

- *1(Worker Still on Attendance Roster)*: This variable is defined for each worker i for day d of month m and year y. It is an indicator variable that is 1 if the worker i is either present in the attendance data on day d of month m and year y, or is present at a future date, and 0 if the worker stopped being observed in the attendance data beginning day d of month m and year y, or any date before.

- *1(Worker Still on Payroll Roster)*: This variable is defined for each worker i for month m and year y. An indicator variable that is 1 if the worker i is either present in the payroll data of month m and year y, or is present at a future date, and 0 if the worker stopped being observed in the payroll data beginning month m and year y, or any date before.

B.2 Presence, Unauthorized Absence and Tardiness

- *Presence*: An indicator variable that is 1 if the worker i is present at work on day d of month m and year y, and 0 otherwise. It is missing if the worker has left the factory i.e. it is conditional on retention.

- *Unauthorized Absence*: An indicator variable that is 1 if the worker i is absent at work, and the absence is not authorized on day d of month m and year y, and 0 if either the worker is present at work or has taken authorized leave. It is missing if the worker has left the factory i.e. it is conditional on retention.

- *Tardy*: An indicator variable that is 1 if the worker i came to the factory later than the modal worker on their production line, and 0 if they came on time. It is missing if the worker has left the factory or is not present at work that day.

B.3 Working and Cumulative Man Days

- *Working*: An indicator variable that is 1 if the worker is retained and present in the factory on day d of month m and year y, and 0 otherwise (if the worker has left the factory, or is not present that day). It is thus a combination of retention and attendance, and is not conditional on retention i.e. it is not missing for workers who have left the factory.

- *Cumulative Man Days*: This measures cumulative man days that accrue to the factory from a particular worker, as measured by the cumulative sum of the variable Working. As with Working, it is not conditional on retention.

B.4 Productivity and other Production Variables

- *Pieces Produced*: Number of garments produced at the hourly level (per worker or per line depending on the regression specification). Line-level number of garments in a given hour is the average of the number of garments produced at the worker-level.

- *Standard Allowable Minutes (SAM)*: This is a measure of how many minutes a particular garment style should be completed in. For instance, a garment style with a SAM of .5 is deemed to take a half minute to produce one complete garment. It is a standardized measure across the global garment industry and is drawn from an industrial engineering database, although it might be amended to account for stylistic variations from the representative garment style in the database.

- *Target Quantity*: The target quantity for a given unit of time for a line producing a particular style is calculated as the unit of time in minutes divided by the SAM. That is, the target quantity to be produced by a line in an hour for a style with a SAM of .5 will be $\frac{60}{0.5} = 120$ garments per hour.

- *Efficiency*: $\left(\frac{\text{Number of garments produced}}{\text{Number of target garments}}\right)$*100 at the hourly level (per worker or per line depending on the regression specification). Line-level number efficiency in a given hour is the mean of worker-level efficiency in that hour.

B.5 Career Advancement

B.5.1 Firm's Administrative Data

This variable varies at the monthly level for each worker.

- *Log(Gross Salary)*: It denotes the natural log of all salaried components of wages (excluding production bonuses which are earned at the line level and paid out through a separate system). It is computed from the firm's payroll data.

B.5.2 Worker Survey Data

These are self-reported measures by the worker during the worker survey implemented after treatment. They vary cross-sectionally at the worker-level.

- *Expect Promotion Next 6 Months*: An indicator variable that is 1 if the worker reported that they expect to be promoted in the next 6 months, and 0 otherwise.
- *Skill Development Training*: An indicator variable that is 1 if the worker reported that they requested skill development training some time in the previous 6 months, and 0 otherwise.
- *Production Award Or Incentive*: An indicator variable that is 1 if the worker reports that they received a production incentive bonus any time in the previous 6 months, and 0 otherwise.
- *Peer Self-Assessment*: Workers were requested to imagine a 6-step ladder on which workers on their production line that were the same skill-level as them stood according to their ability, where the worst workers were on the first rung, and the best on the 6th rung. Workers were then asked which rung they believed they should be on.
- *Line Co-Worker Self-Assessment*: Workers were requested to imagine a 6-step ladder on which all the workers on their production line stood according to their ability, where the worst workers were on the first rung, and the best on the 6th rung. Workers were then asked which rung they believed they should be on.

B.6 Other Survey Variables

Like the other variables that were collected during the worker survey implemented after treatment, these variables are self-reported (by the worker), and vary cross-sectionally at the worker-level.

B.6.1 Financial Behaviors and Attitudes

- *1(Any Saving)*: An indicator variable that takes the value 1 if the worker reports having any savings, and 0 otherwise.
- *Saving for Children's Education*:An indicator variable that takes the value 1 if the worker reports having saved any money for children's education, and 0 otherwise.

- *Risk Aversion Index*: Risk aversion was measured from a set of proposed choices between a deterministic amount and a gamble. The questions content is the same as those in the Indonesian Family Life Survey (IFLS), with the amounts under consideration changed to reflect the local context and currency. For instance, a representative question was:

  "Suppose you are given two options of receiving income. In the first option you are guaranteed Rs. X per month. In the second option you are guaranteed Rs. Y or Rs. Z, each with equal chance. Which option would you choose?"

  The coefficient of risk-aversion assuming CRRA preferences was then computed using the payoffs, and solving for the constant of coefficient of risk-aversion. For a detailed description of an identical computation using the IFLS data, readers are referred to Ng (2013).

B.6.2 Government and Firm Entitlements

- *1(Government Pension)*: An indicator variable that takes the value 1 if the worker reports having availed of a government pension program in the last 6 months, and 0 otherwise.

- *Government Subsidized Housing*: An indicator variable that takes the value 1 if the worker reports having availed of a government pension program in the last 6 months, and 0 otherwise.

- *Firm Subsidized Housing*: An indicator variable that takes the value 1 if the worker reports intending to avail of the employer's subsidized housing program in the next 6 months, and 0 otherwise.

- *Firm Subsidized Schooling*: An indicator variable that takes the value 1 if the worker reports intending to avail of the employer's subsidized schooling program in the next 6 months, and 0 otherwise.

B.6.3 Personality

- *Contentiousness (ME)*: This measure captures the net number of behaviors workers identify with that are predictive of contentiousness. Workers were asked about the extent (measured on a 5-point scale of agreement ranging from Strongly Agree to Strongly Disagree) to which they engaged in 5 positive and 5 negative behaviors. The score from each variable was added up for positive and negative behaviors and the score from the negative behaviors was then subtracted from the score for positive behaviors.

The positive behaviors were the following:

- I am always prepared
- I pay attention to details
- I get chores done right away
- I carry out my plans
- I make plans and stick to them

The negative behaviors were the following:

- I procrastinate and waste my time
- I find it difficult to get down to work
- I do just enough work to get by
- I don't see things through
- I shirk my duties

The final measure was computed as the mean effect normalization of the above variables.

- *Locus of Control (ME)*: This measure captures the net number of beliefs workers identify with that are predictive of locus of control. Workers were asked about the extent (measured on a 5-point scale of agreement ranging from Strongly Agree to Strongly Disagree) to which they believed 5 statements, one of which are positively related to locus of control and four of which are negatively related. The score from each variable was added up for the negative statements and the score from the negative statements was then subtracted from the score for positive statement.

  The positive statement was the following:

  - I believe that my success depends on ability rather than luck

  The negative statements were the following:

  - I believe that unfortunate events occur because of bad luck
  - I believe that the world is controlled by a few powerful people
  - I believe some people are born lucky
  - I believe in the power of fate

The final measure was computed as the mean effect normalization of the above variables.

- *Perseverance (ME)*: This measure captures the net number of behaviors workers engage in that are predictive of perseverance. Workers were asked about the extent (measured on a 5-point scale of agreement ranging from Strongly Agree to Strongly Disagree) to which they engaged in 8 behaviors, five of which are positively related to perseverance and three of which are negatively related. The score from each variable was added up for the negative statements and the score from the negative behaviors was then subtracted from the score for positive behaviors.

  The positive behaviors were the following:

  – I don't quit a task before it is finished

  – I am a goal-oriented person

  – I finish things despite obstacles in the way

  – I am a hard worker

  – I don't get sidetracked when I work

  The negative behaviors were the following:

  – I don't finish what I start

  – I give up easily

  – I do not tend to stick with what I decide to do

  The final measure was computed as the mean effect normalization of the above variables.

- *Extraversion (ME)*: This measure captures the net number of beliefs workers identify with that are predictive of extraversion. Workers were asked about the extent (measured on a 5-point scale of agreement ranging from Strongly Agree to Strongly Disagree) to which they believed 10 statements, five of which are positively related to extraversion and five of which are negatively related. The score from each variable was added up for the negative statements and the score from the negative statements was then subtracted from the score for positive statements.

  The positive statements were the following:

  – Am open about my feelings

- Take charge
- Talk to a lot of different people at parties
- Make friends easily
- Never at a loss for words

The negative statements were the following:

- Don't talk a lot
- Keep in the background
- Speak softly
- Have difficulty expressing my feelings
- Hold back my opinions

The final measure was computed as the mean effect normalization of the above variables.

- *Self-Sufficiency (ME)*: This measure captures the net number of beliefs workers identify with that are predictive of self-sufficiency. Workers were asked about the extent (measured on a 5-point scale of agreement ranging from Strongly Agree to Strongly Disagree) to which they believed 10 statements, five of which are positively related to self-sufficiency and five of which are negatively related. The score from each variable was added up for the negative statements and the score from the negative statements was then subtracted from the score for positive statements.

  The positive statements were the following:

  - Act without consulting others
  - Do things men traditionally do
  - Do things my own way
  - Make decisions quickly.
  - Believe that events in my life are determined only by me

  The negative statements were the following:

  - Need protection
  - Often need help.

– Talk about my worries.

– Let myself be directed by others.

– Am easily moved to tears.

The final measure was computed as the mean effect normalization of the above variables.

B.6.4 Mental Health

- *Self-Esteem (ME)*: This measure captures the net number of beliefs workers identify with that are predictive of self-esteem. Workers were asked about the extent (measured on a 5-point scale of agreement ranging from Strongly Agree to Strongly Disagree) to which they believed 10 statements, five of which are positively related to self-esteem and four of which are negatively related. The score from each variable was added up for the negative statements and the score from the negative statements was then subtracted from the score for positive statements.

  The positive statements were the following:

  – On the whole, I am satisfied with myself

  – I feel that I have a number of good qualities

  – I am able to do things as well as most other people

  – I feel that I am person of worth, at least on an equal plane with others

  – I take a positive attitude toward myself

  The negative statements were the following:

  – I feel I do not have much to be proud of

  – At times, I think I am no good at all

  – I certainly feel useless at times

  – I wish I could have more respect for myself

  – All in all, I am inclined to feel that I am a failure

  The final measure was computed as the mean effect normalization of the above variables.

- *Hope or Optimism (ME)*: This measure captures the net number of beliefs workers identify with that are predictive of hope or optimism. Workers were asked about the extent (measured on a

5-point scale of agreement ranging from Strongly Agree to Strongly Disagree) to which they believed 10 statements, five of which are positively related to hope or optimism and three which are negatively related. The score from each variable was added up for the negative statements and the score from the negative statements was then subtracted from the score for positive statements.

The positive statements were the following:

– Look on the bright side.

– Can find the positive in what seems negative to others.

– Remain hopeful despite challenges.

– Will succeed with the goals I set for myself.

– Think about what is good in my life when I feel down.

The negative statements were the following:

– Expect the worst.

– Have no plan for my life five years from now.

– Am not confident that my way of doing things will work out for the best

The final measure was computed as the mean effect normalization of the above variables.

- *Mental Distress*: The two measures of mental health are computed using the 10-question Kessler Psychological Distress Scale, or K10. The K10 was developed by Ron Kessler and Dan Mroczek in 1992 as a measure of mental distress (Kessler et al., 2003). The questionnaire consists of 10 questions about negative emotional states experienced during the past 4 weeks. Respondents give 5-point answers ranging from "none of the time" (scored as a 1) to "all of the time" (scored as a 5), with the intermediate responses scored correspondingly (i.e. "a little of the time" scored as 2, "some of the time" scored as 3, and "most of the time" scored as 4). In particular, respondents are asked:

  – About how often did you feel tired out for no good reason?

  – About how often did you feel nervous?

  – About how often did you feel so nervous that nothing could calm you down?

- About how often did you feel hopeless?
- About how often did you feel restless or fidgety?
- About how often did you feel so restless you could not sit still?
- About how often did you feel depressed?
- About how often did you feel that everything was an effort?
- About how often did you feel so sad that nothing could cheer you up?
- About how often did you feel worthless?

The survey methodology was developed and first validated in the United States. It has since been administered in a variety of contexts around the world, including in low-income populations in South Africa (Myer et al., 2008). Moderate mental distress is indicated by a score of 24 or higher on the scale.