Soft Skills to Pay the Bills: Evidence from Female Garment Workers*

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Abstract

Non-cognitive ("soft") skills – allocating time and money effectively, teamwork, leadership, relationship management, acquiring and assimilating information – account as much for long-term economic success as cognitive ability and educational attainment. But these skills may be very difficult to teach in adulthood, especially to those with low baseline skill sets. Moreover, firms may be reluctant to invest in workers' skills if attrition rates are high, which is particularly the case for frontline workers. We carried out a randomized experiment with female garment workers in Bengaluru, India to test whether it is possible to impart soft skills to frontline workers, and evaluate the labor productivity, retention, and profitability consequences for firms. Treated workers are less likely to leave during the program, and exhibit substantially higher productivity up to nine months after program completion. This leads to being assigned to more complex tasks and a greater likelihood of promotion. Treated workers are also more likely to enroll in workplace skill development and production incentive programs. Survey evidence supports the hypothesis that the stocks of soft skills improved in key dimensions. Two-stage randomization allows us to estimate spillovers within production teams; spillovers in productivity are substantial and persistent. Using actual costing data we find that the program pays for itself several times over by the end of the evaluation period, implying that teaching soft skills in the workplace can be profitable for firms even in high-turnover environments.

Keywords: soft skills, skilling, productivity, ready-made garments
JEL Codes: J24, M53, O15

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1 Introduction

There is emerging consensus on the importance of non-cognitive ("soft") skills – such as allocating resources (e.g., time and money) effectively, teamwork, leadership, relationship management, and acquiring and assimilating information – for labor market success (Deming, 2015; Groh et al., 2015; Guerra et al., 2014; Heckman and Kautz, 2012; Heckman et al., 2006).[1] We know from recent work that it is possible to inculcate these skills at very early ages, for example, through home-based stimulation programs, high quality daycares, and preschool programs (Attanasio et al., 2014; Gertler et al., 2014; Grantham-McGregor et al., 1991). But how malleable are these skills in adulthood? 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). Given the high labor market valuation of non-cognitive skills, this might deepen poverty traps, since family income is strongly positively associated with skill levels (Heckman et al., 2006).

Yet the need for trained workers – in terms of both hard (technical) and soft skills – has never been greater, especially in low-income country contexts where industrial growth has far outstripped growth in the supply of skilled labor (Cunningham and Villaseñor, 2016; Hanushek, 2013). In countries with low public capacity to implement skilling initiatives directly, policymakers often lean on the private sector to impart skills via workplace-based programs (Tan et al., 2016). But firms may be reluctant to invest in human capital in this way, given the high rates of turnover in many low-skill industries, leading to an inefficiently low level of skilling in equilibrium. (Acemoglu and Pischke, 1998, 1999; Autor, 2001; Becker, 1964).

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 it is indeed possible, do these improved skills lead to productivity gains? Finally, given high rates of turnover, does it pay for firms to impart soft skills to their workers?

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 work-

[1] In defining non-cognitive skills, we follow the 1991 SCANS assessment's definition: "the ability to allocate resources (time, money, facilities), interpersonal skills (such as teamwork, teaching others, leadership), the ability to acquire and to use information, the ability to understand systems, and the ability to work well with technology" (Kautz et al., 2014).

ers (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 survey; and 2) workplace outcomes such as retention, attendance, productivity, salary, and promotion. The trial's design allows us to capture spillovers onto untreated workers (both through the transfer of skills as well as through production complementarities). Finally, we compute the firm's returns combining our point estimates with program costing data.

We used a two-stage randomization procedure. We enrolled 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 P.A.C.E. or control. We thus estimate treatment effects by comparing *treatment 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 control worker 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 productive, more likely to be assigned to complex tasks, and more likely to be promoted. Impacts last up to 9 months after program completion, suggesting that learned skills translated into persistent workplace impacts. The program *did not* cause women to leave the firm: the rate of attrition actually declined in the treatment group relative to the control during the program period; this treatment effect diminished slightly after program completion.

Results from a survey administered to treatment and control workers a month after program completion complement these impacts on workplace outcomes. First, treatment workers exhibit greater acquisition and use of information: they are more likely to avail themselves of skill development initiatives at the firm, state-sponsored pension, health-care, and subsidies for schooling and housing. Second, consistent with improved resource management, particularly financial literacy and forward-looking behavior, treatment workers were more likely to be saving for children's education. Third, survey results indicate greater self-assessment of workplace quality (relative to 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, the results suggest that the program effectively increased workers' stocks of soft skills.

The two-stage randomization design allows us to examine treatment spillovers within teams (production lines). We find that untreated workers on treatment lines have more cumulative man-days compared to control workers (on control lines) during the program. They are also more productive and are assigned to more complex operations, which increases their probability of promotion. These impacts are nearly as large as the direct impacts of treatment, suggesting that treated workers boosted team performance on the whole.

Finally, we combine our point estimates of impacts on workplace outcomes with program costing data to calculate the costs and benefits of the program to the firm. The program's rate of return was already considerable by the end of the program period (124%); by the end of the measurement period, nine months after program completion, the return was 420% return. These very large returns are rationalized by the relatively low costs of the program combined with the accumulated effects on productivity and man-days, and are seen in other recent interventions in garment factories (Menzel, 2015).

The weight of the evidence we present suggests that the primary mechanism for improvements in workplace outcomes was the inculcation of soft skills. Our interpretation of the 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 thus directly affects productivity. Retention went up relative to control during the program period likely because workers were receiving an in-kind transfer, thus increasing the likelihood that their effective wage at the firm lay above the wage at their best outside option. Team spillovers were likely generated both by the transfer of skills from treated to untreated team members and by production complementarities.[2]

This study seeks to make three contributions. The first is related to the literature examining the labor market impacts of soft skills (Deming, 2015; Groh et al., 2015; Guerra et al., 2014; Heckman and Kautz, 2012; Heckman et al., 2006; Riordan and Rosas, 2003). As mentioned above, we focus on two important aspects of this question: first, is it possible to substantively change the stock of soft skills

[2] Reciprocity motive is another potential mechanism for changes in workplace outcomes, but our results suggest changes are not substantively driven by this factor. In addition to the direct survey evidence on changes in soft skills, two facts indicate that the role for reciprocity in our context is likely small. First, we observe spillover impacts on workers who enrolled in the lottery for the program but were not chosen for P.A.C.E. treatment (and work on the same lines as treated workers). Second, we observe persistent impacts on productivity that last up to 9 months after program completion. The limited role of reciprocity is consistent with recent work on gift-giving in the workplace (DellaVigna et al., 2016).

after childhood, and second, if so, are these changes reflected in changes in labor outcomes? 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 is mixed, but interventions focused on young women find positive impacts (Buvinić and Furst-Nichols, 2016). The only other study to our knowledge that evaluates (via randomized assignment) the impacts of soft skills separately from other types of training is Groh et al. (2012), which examines 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 income are in general very small (though imprecisely estimated).

Second, our study informs the understanding of firms' incentives for skilling their workers. An old insight from labor economics is that, in competitive labor markets, firms should never invest in general (or "transferrable") skills, i.e., those skills which raise a worker's productivity at all firms (Becker, 1964). This may lead to an inefficiently low level of skill in equilibrium if workers cannot pay for training themselves. More recent work has argued that the fact that skilling programs exist in equilibrium (often without an accompanying reduction in workers' wages) is evidence of labor market imperfections such as search frictions (Acemoglu and Pischke, 1998, 1999).

Our experiment serves as a case study in firms' returns to investing in transferrable skills. We show that though the rate of turnover is in general quite high, treated workers are no more likely to leave than control workers. In fact, during the program (which, perhaps helpfully, is spread over 11 months) treated workers were significantly less likely to leave than controls. Moreover, the treated workers who remain at the firm are substantially more productive after the program, generating large returns for the firm even though overall retention is low.

Finally, we contribute to the study of 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 accompanied by a real 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). Understanding whether improving workplace outcomes for women, via skills training and promotion as Macchiavello et al. (2015) do, or via soft-skills training as we do, can improve retention contributes to understanding the determinants of female labor force participation in ways that are amenable to policy interventions.

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, and section 6 concludes.

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, including 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).[3]

In the sewing department of the study factories (as in most medium and large garment factories), garments are sewn in production lines consisting of 50-150 workers (depending on the complexity of

[3] This is because a standardized measure of output is recorded for each worker in each hour on the sewing floor.

the style) arranged in sequence and grouped in terms of segments of the garment (e.g. sleeve, collar, placket). Roughly two-thirds to three-quarters of the workers on the line are assigned to machines completing sewing tasks. 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).[4]

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. Orders are then packed and sent to ports for export.

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 adapted P.A.C.E. to the local context and implemented it in five factory units in the Bengaluru area. The goal of this 80-hour program is to improve life skills such as time management, effective communication, problem-solving, and financial literacy for its trainees. The program begins with an introductory ceremony for participants, trainers, and firm management. The main teaching modules are: Communication (9.5 hours); Problem Solving and Decision-Making (13 hours); Time and Stress Management (12 hours); Water, Sanitation and Hygiene (6 hours); Financial Literacy (4.5 hours); General and Reproductive Health (10 hours); Legal Literacy and Social Entitlements (8.5 hours); and Execution Excellence (5 hours). Appendix Table A1 provides an overview of the topics covered in each module. After all modules have been completed, there are two review sessions of about 3 hours in total to review the experience and discuss how participants will apply their learnings to personal and professional life situations. At the close of the program, there is a graduation ceremony.

The program is usually conducted for two hours per week. Management allocates one hour of

[4] In general, we describe here the process for woven garments; however, the steps are quite similar for knits, with varying number and complexity of operations. Even within wovens, the production process can vary a bit by style or factory.

workers' production time a week to the program, and workers contribute one hour of their own time) in designated spaces ("P.A.C.E. rooms") in the factories. Due to holidays and festivals (which are times of high absenteeism, meaning that many workers would skip the sessions) and production constraints, sessions were conducted 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 was conducted between July 2013 and May 2014, and the closing ceremony in June 2014.

2.3 Experimental Design

Our experimental design, along with continuously recorded individual-level workplace data, allow us to rigorously evaluate the program's effects on productivity, attendance, and retention, and also measure productivity spillovers within production lines.

Participants were chosen from a pool of workers who expressed interest and committed to enroll in the program. Randomization was conducted at two levels: line level (stratified by unit, above- and below-median efficiency, and above- and below-median attendance at baseline), 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.[5]

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

[5]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.

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 for the line, was trained to input production data directly in the tablet computer.

3.1.1 Productivity

The key measures of production we study are pieces 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 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).

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 0.5 is deemed to take a half minute to produce one complete garment.[6] SAM, as the name suggests, is standardized across the global garment industry and is drawn from an industrial engineering database. However, 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). The target quantity for a given unit of time for a worker producing 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 .5 will be $60/.5 = 120$.

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

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 we use to compute whether a worker has been promoted. Since tailors of different skill grades are paid differently, we can use the salary data to see a "permanent" increase in salary of a worker, indicating that they have been promoted.

3.3 Survey Data

In addition to measuring workplace outcomes, a survey of 1,000 randomly chosen treated and control workers (of whom 538 were treated, and the remainder controls) was conducted in June 2014, one month after program completion. The survey covered, among other things, questions related to financial decisions (including savings and debt), awareness of and participation in welfare programs (government or employer sponsored), and labor market choices, such as whether workers held a second job. It also covered personality characteristics (conscientiousness, extraversion, locus of control, hope/optimism, perseverance, self-esteem and self-sufficiency), mental health (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.[7] 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 asking them which step they would place themselves on), and participation in skill development and production award 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, year of schooling, years of tenure with the firm, age, and skill level

[7]Risk and time preference measures were taken from the Indonesian Family Life Survey (IFLS), and survey responses regarding preferences over lotteries were mapped on to risk and time preferences using the approach detailed in Ng (2013).

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	t-stat	p value
Attendnace Rate	0.882	0.235	0.895	0.203	-1.464	0.144
Years of Schooling	8.592	3.634	8.508	3.280	0.506	0.614
Years of Tenure	1.432	2.709	1.353	2.119	0.677	0.500
Age	27.712	14.087	27.420	11.638	0.473	0.637
1(Speaks a South Indian Language)	0.657	1.560	0.671	1.156	-0.210	0.834
Grade (Skill level: 0 to 12)	5.674	6.631	5.822	5.625	-0.505	0.615
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	t-stat	p value
Attendnace Rate	0.898	0.117	0.903	0.103	-0.881	0.380
Years of Schooling	8.592	3.634	8.508	3.280	0.506	0.614
Years of Tenure	1.432	2.709	1.353	2.119	0.677	0.500
Age	27.712	14.087	27.420	11.638	0.473	0.637
1(Speaks a South Indian Language)	0.657	1.560	0.671	1.156	-0.210	0.834
Grade (Skill level: 0 to 12)	5.674	6.631	5.822	5.625	-0.505	0.615
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.

(from 0 to 12, where 0 is the lowest and 12 the highest), and an indicator for speaking a South Indian language. The first set of comparisons is between the 1,087 treated workers to the 779 *control workers in control lines* (lines with no treated workers). 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 1.2-1.4 years. The average worker is about 27 years old, with about 8 years of schooling, and a 80% likelihood of speaking a South Indian language.

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 Specifications

4.2.1 Retention

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 treated worker and 0 if she is a control worker, 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, thus allowing comparison relative to the *pre-announcement period*. Each regression includes unit x year x month fixed effects ψ_{uym}, and worker fixed effects η_w (which absorb 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. In particular, since we designed the experiment to measure spillover effects and in fact find evidence of significant spillovers, we think it is most appropriate to use this conservative level of clustering.

4.2.2 Attendance, Unauthorized Leave and Tardiness

As shown in section 5.1, we do not find any differential retention at the end point of the program period (February 2015). In addition, there is no heterogeneity in retention impacts across distributions of baseline characteristics, as shown in the appendix. Despite this, we still need to 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 ensure the unbiasedness of our results it is necessary to weight all prior observations to recover population average treatment effects. Accordingly, we follow the approach suggested in Wooldridge (2010), which comprises the following:

1. Estimate a probit specification for the probability of being retained, which is a dummy variable that takes the value 1 if the worker is in the sample on any given month for all the outcome variables (except those not conditional on retention, like cumulative man-days) from the attendance and salary data (whether the worker is present at work, whether the absence is unauthorized, and whether the worker was tardy in coming to work) and 0 otherwise, on the treatment indicator interacted with month by year fixed effects and each baseline characteristic reported in Table 1. From this specification, we predict for each month the probability that the worker is in the data, and use the inverse of this predicted probability as the probability weights or sample weights in all the regressions.

2. We then re-estimate Equation 1 using the other outcome variables on the left-hand side and these estimated weights. Note that because in the intermediate data (after the announcement but before the endline) 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 (we explore robustness to different weights, as well as using the unweighted data, in the appendix).

We focus the analysis on three related outcome variables: whether the worker is present at work, whether the absence is unauthorized, and whether the worker was tardy in coming to work.

4.2.3 Working Days

To estimate the impact of treatment on the additional total number of working days from the worker, we consider two outcomes: the first is a binary 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 man days as measured by the cumulative sum of the first variable. Both are at 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. Note that since these are not conditional variables (unlike, e.g., attendance, which is conditional on retention), no weighting is required.

4.2.4 Productivity and Task Complexity

The productivity regressions follow a similar estimation procedure as for attendance and late-coming, except that we need to re-weight the sample based on whether the worker is working (a binary variable that is 1 if the worker is retained and present in the production data and 0 otherwise), rather than only whether they are retained or not. Thus, the two-step estimation procedure is as follows:

- We estimate a probit specification for the probability of the worker being present in the production data on that particular day, which is a dummy variable that takes the value 1 if the worker is in the production data sample on any given day (0 if the worker has already left the firm, or is still employed with the firm but not present on that day) for all the outcome variables (daily efficiency, total pieces produced, and the complexity of the garment produced by the worker as measured by the Standard Allowable Minutes (SAM)), on the different treatment indicators interacted with month by year fixed effects and each baseline characteristic. From this specification, we predict for each day the probability that the worker is in the data, and use the inverse of this predicted probability as the probability weights or sample weights in the next step.

- We then re-estimate Equation 1 using productivity and task complexity variables on the left-hand side and the estimated weights in the previous step. Additional controls include the number of days that a worker worked on a particular line producing that particular item up until the day of the data (to account for learning), the total order quantity, the number of workers working alongside each worker on the line that day (which will determine how many operations the garment is being split into, and so will affect target quantities), in addition to unit by year by month and worker by garment level fixed effects.

As before, clustering is at the line-level. Finally, when pieces produced is the left-hand side variable instead of efficiency (actual production divided by target production), we control for target production as well.

4.2.5 Career Advancement and Career Expectations

To study the impact of the program on career advancement, we utilize the follow variable from the administrative data: whether the workers' salary showed a permanent increase (indicating a promotion). We first estimate the retention probability weights as detailed in section **??**, and then estimate equation 1 using those inverse probability weights, with the left-hand side variable a binary variable for whether the worker was promoted (Note that the administrative salary data is at the monthly level for each worker rather than the daily-level.).

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 skills development training in the last six months. Self-reported performance was measured by asking whether workers have received production 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 is only cross-sectional, we regress these outcomes on a binary variable for treatment or control, and include factory unit fixed effects.

4.2.6 Other Survey Outcomes

We consider the impact of the program on outcomes that might plausibly be impacted by the skills taught by P.A.C.E. For instance, since the program targets impacts on 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 healthcare, schooling and housing. Similarly, since the program aims to make workers more forward-looking, we test whether there is an increase workers' savings, especially for important future considerations like their children's schooling. Furthermore, we test whether the program impacted personality characteristics (conscientiousness, locus of control, perseverance, extrovertedness and self-sufficiency) and mental health (self-esteem, hope/optimism, and moderate and severe 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.

4.2.7 Figures

We create figures illustrating the month-by-month treatment impacts by re-estimating all the retention, attendance, late-coming, productivity, task complexity, and promotion regressions with the treatment binary interacted with monthly dummies from June 2013 onwards. All regression analogs are reported in tables in the appendix, and May 2013 is the excluded dummy variable for most outcomes except those taken from the hourly production data (converted to daily level), for which June 2013 is the excluded dummy variable, since production data is available only from June 2013 onwards.

4.2.8 Spillover Effects

To study spillover effects, we re-run all of the specifications mentioned above, replacing the binary treatment variable with the binary spillover treatment variable. This variable compares control 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 underwent 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 treated workers). Thus, it takes the value 1 if the worker is a control worker in a treated line, and 0 if the worker is a control worker (signed up for the program lottery but did not receive the program) in an untreated line, since workers who work with the treated workers are most likely to experience spillover impacts.

5 Results

5.1 Retention and Daily Working Status

We begin by testing whether retention and the probability that a worker is on the job on a given day are impacted by treatment. In addition to being important outcomes in their own right, if these outcomes are impacted by treatment, other outcomes that are a function of worker retention and presence (e.g., productivity) need to be re-weighted as discussed in the previous section.

Figure 2A: Raw Retention (Sewing)

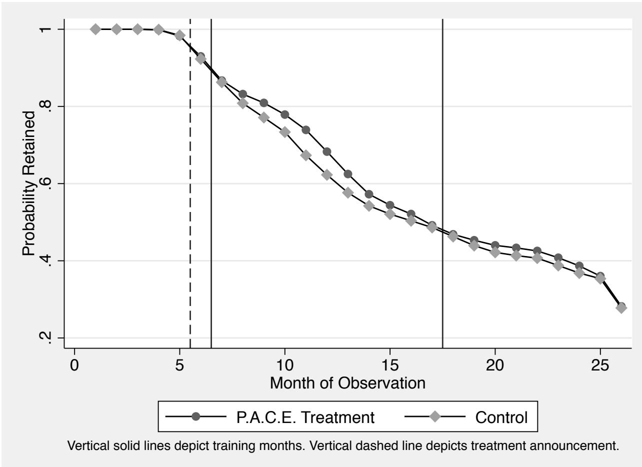
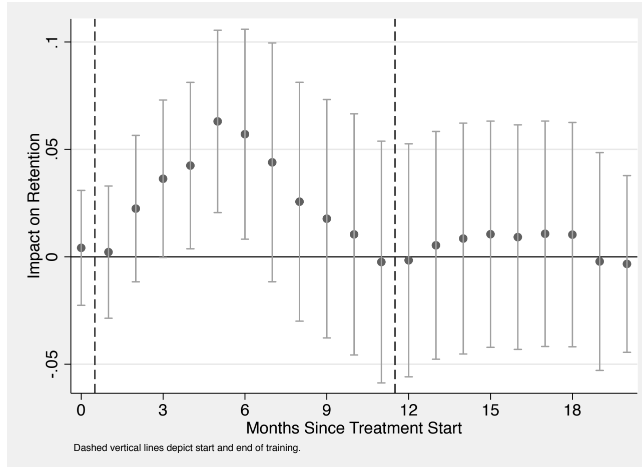


Figure 2B: Monthly Retention (Sewing)



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 and promotion). Analogous figures for the whole sample available upon request. Note that, for completeness, we present impacts on working (combination of retained and present) for the whole sample of workers (both sewing and non-sewing) below. Figures using payroll roster data instead of attendance data look nearly identical. Accordingly, these are not presented, but are also available upon request.

Figure 2A shows raw retention data for both treatment and control groups over the observation period with training months denoted. The dotted vertical line in Figure 2A denotes the announcement of assignment to treatment. 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 figures shown here are from the sample of sewing workers using the attendance data (to ensure consistency with other outcomes for which we only have data for the sewing sample like productivity). Using the entire sample and the payroll data yields nearly identical results.

The second outcome of interest is the probability that a worker retained *and* is working on a given day. This "working" dummy is, therefore, equal to 0 on a given day if 1) she has permanently left the factory, or 2) is still working for the firm but is not present on a given day. Figure 3A shows raw data on the binary variable for working for both treatment and control groups over the observation period

Figure 3A: Raw Working (Whole Sample)

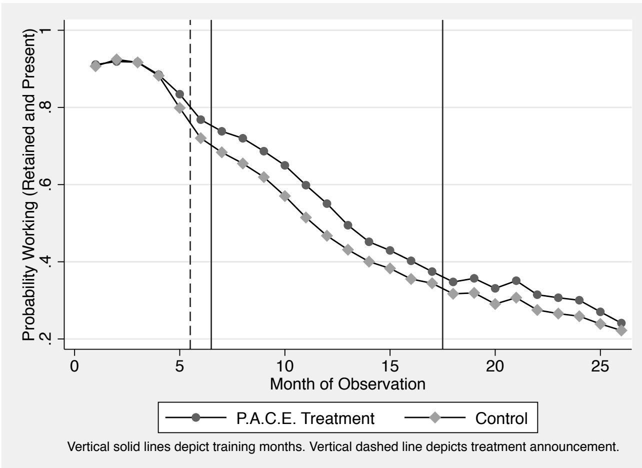
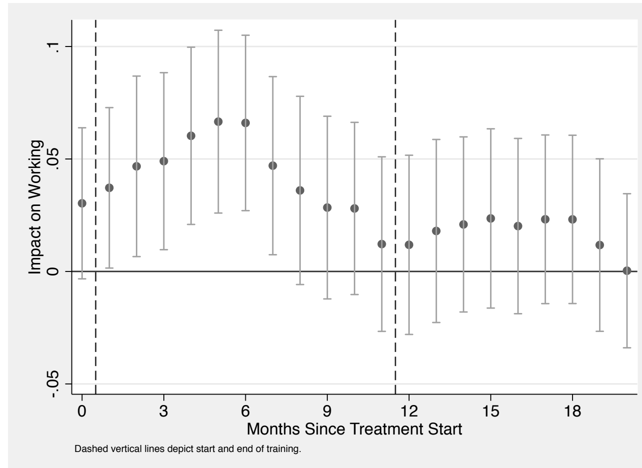


Figure 3B: Monthly Working (Whole Sample)



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 whole sample of workers (both sewing and non-sewing workers) for completeness. Figures depicting sewing workers only reflect the combination of patterns shown in Figures 3A-3B, and accordingly are omitted here for brevity.

(with the treatment announcement period indicated again by the vertical dotted line). Figure 3B 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 the treatment period and after, although Figure 3B indicates that these impacts are statistically significant for most of the treatment period but not after.

Table 2 presents the results for retention and the working binary. 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 about 5 percentage points (pp) during the treatment, and about 4pp when the treatment is announced; the pattern is consistent across both sources of data, but is statistically stronger when 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 full 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

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

	(1)	(2)	(3)	(4)	(5)	(6)
	Retained 1(Worker Still on ***Attendance*** Roster)		Retained 1(Worker Still on ***Payroll*** Roster)		Working 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	0.0767*
	(0.0230)	(0.0257)	(0.0265)	-0.0274	(0.0190)	(0.0403)
During X P.A.C.E. Treatment	0.0575**	0.0289	0.0595**	0.0283	0.0431**	0.0926***
	(0.0228)	(0.0212)	(0.0255)	(0.0216)	(0.0180)	(0.0352)
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	666,038
Control Mean of Dependent Variable	0.589	0.628	0.619	0.656	0.480	0.376

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.

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 sewing 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 similar - 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 mentioned in the previous section, the impacts on retention and worker presence also have implications for the estimation of impacts on outcomes conditional on these outcomes. In particular, since there are treatment effects on these variables during program announcement and implementation, estimation of other outcomes that are conditional on retention (like attendance) or worker presence (like productivity) require re-weighting of these outcomes.

5.2 Productivity and Task Complexity

To the extent that the program increased the stock of non-cognitive skills like time-management, internal motivation, and communication, it is plausible that it makes workers better at handling complex tasks, as well as more productive. To test this, we consider three outcomes. The first is efficiency (daily production divided by target production), total daily production (controlling for target production), and the complexity of the task that the workers are assigned to, as measured by SAM (as described previously, the number of minutes that the task should be completed in – a higher SAM thus denotes a more complex task, since it is expected to take longer).

Figure 4A: Monthly Efficiency (Sewing)

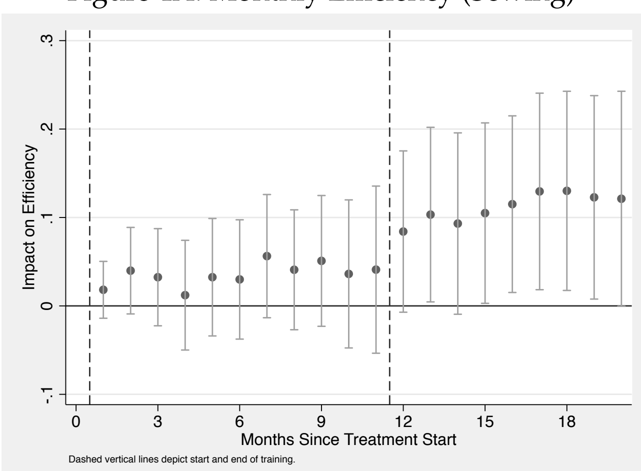
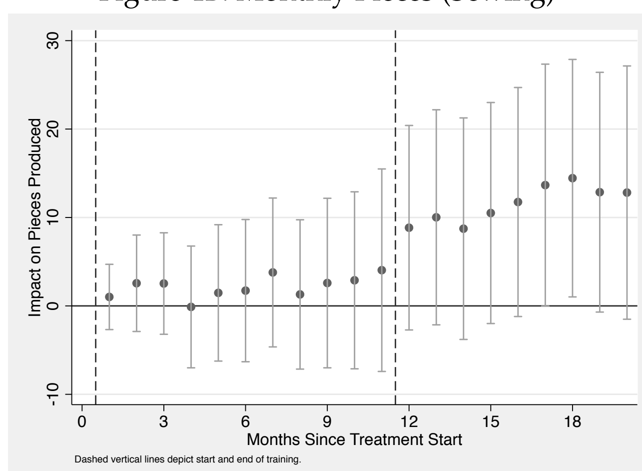


Figure 4B: Monthly Pieces (Sewing)



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 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 as 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.

Figure 5A: Raw SAM (Sewing)

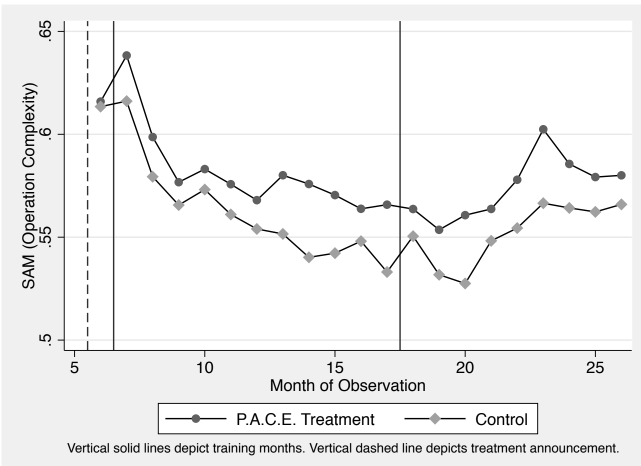
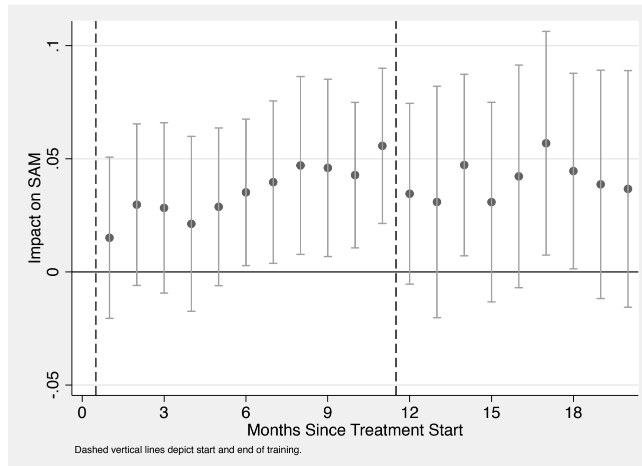


Figure 5B: Monthly SAM (Sewing)



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.

Figures 4A and 4B indicate that treatment increases efficiency and the total production of the workers (controlling for target production) after the program concludes, although the impacts are 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). This is also shown in Table 3, which, as in the figures in this section, uses production data. Treated workers are 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 on average relative to the control group after the treatment, about 10% of the control group mean.

Interestingly, treated workers are assigned to more complex tasks both during and after treatment - tasks that they are assigned to are expected to take about 2 seconds (0.03 minutes) more, about 5%

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	0.0662**	6.359*	0.0350*
	(0.0306)	(3.322)	(0.0192)
During X P.A.C.E. Treatment	0.0196	1.124	0.0334**
	(0.0161)	(1.836)	(0.0139)
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	263,322	263,322	263,322
Control Mean of Dependent Variable	0.541	61.719	0.568

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.

of the control group mean (0.56 minutes). Thus, not only are workers in the treatment group assigned to more complex tasks during and after the program, they are more efficient than the control group 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. Table **??** reports the results of the analogous regressions, in which impacts are grouped into two time periods (as before) – during and after P.A.C.E. program implementation. 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.

5.3 Man 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 actually promoted 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 objective measure of actual promotion using the payroll 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 Man Days (Sewing)

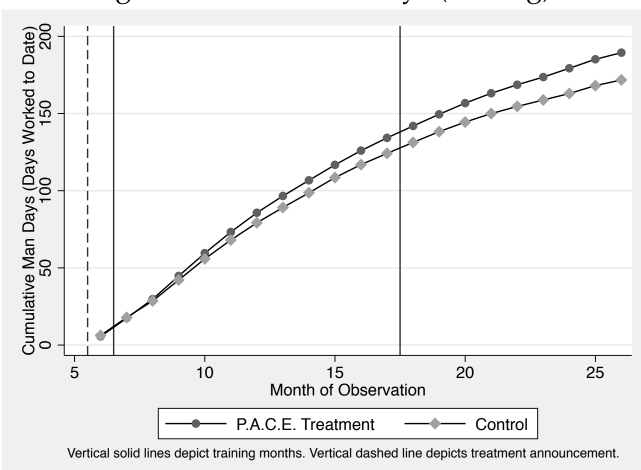
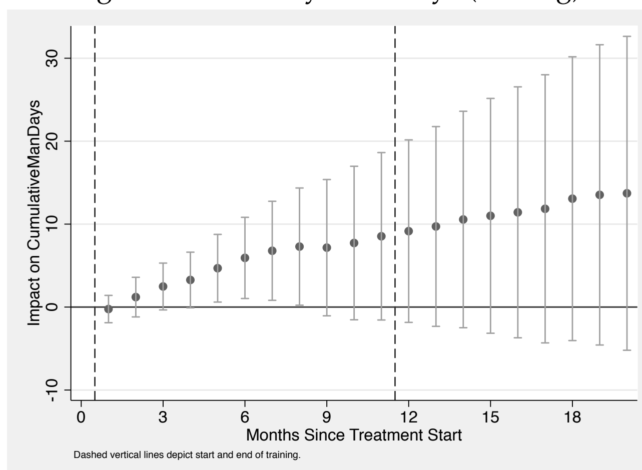


Figure 6B: Monthly Man Days (Sewing)



Figures 6A and 6B depict impacts of P.A.C.E treatment on cumulative man days in the factory from the start of the observation period (January 1, 2013) to date. Figure 6A depicts raw man 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 similar impacts for the cumulative number of man days[8] to those of worker presence- the number of cumulative man days are higher for the treatment group relative to the control, and are statistically significant from about 3 months into the program until about 8 months, and are not statistically significant after, although the point estimates are still positive.

Table 4 shows the impacts on cumulative man days during and after the program. We present the results from the attendance data for the entire sample and from the production data for the sewing workers. The treatment increases the cumulative man 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

[8]Although the treatment and control workers are all women, we use the term "man-day" to denote one full day of work by a person, in accordance with the term's dictionary definition.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Cumulative Man-Days		Levels Promoted to Date	Expect Promotion Next 6 Mos	Skill Development Training	Production Award or Incentive	Peer Self-Assessment	Line Co-Worker Self-Assessment
	Sum of Days Working for Each Worker to Date		# of Levels Promoted Since Start of Observation	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**	11.55	0.0666*					
	(8.495)	(7.598)	(0.0380)					
During X P.A.C.E. Treatment	8.410*	5.032*	0.0256					
	(4.281)	(2.624)	(0.0217)					
Announced X P.A.C.E. Treatment	-1.058		0.00683					
	(4.938)		(0.00560)					
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	666,038	26,820	621	621	621	621	621
Control Mean of Dependent Variable	201.408	105.763	0.045	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 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 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.

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 man-days during and after the program. Thus, the treatment has a strong positive impact of the number of man days for the firm, which is an important consideration in the cost-benefit analysis conducted in section 6.

Figure 7A shows raw data on the number of levels promoted using the payroll data for both treatment and control groups over the observation period with training months denoted. Figure 7B shows analogous regression coefficients to those from Table 4, but with impacts split up monthly. Since the probability of promotion is conditional on being retained, the regressions are weighted by the inverse predicted probability of retention as a function of the interaction between the treatment binary with year by month fixed effects and baseline characteristics, as for the attendance and latecoming outcomes. Figure 7A shows that PACE workers are more likely to be promoted relative to control workers with the gap widening during the program as well as after. Figure 7B shows treatment increases the probability of promotion during and after treatment, although the impact is just short of statistical significance at the 5% level.

Columns 3 and 4 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 are about 1.4 percentage points more likely to be promoted during treatment both for the entire sample and only the sewing sample. These impacts are quite large relative to the control group mean, nearly 50% relative to the control group mean of the entire sample, and about 87% relative to the control group mean of the sewing sample. After treatment, the impacts are even larger - about 2.5 percentage points for both samples, which is about 85% of the control group mean for the entire sample and about 160% of the control group mean for the sewing sample. Thus, in addition to being assigned to more complex tasks and being more efficient, treated workers are much more likely to be promoted within the firm. Since sourcing higher-skilled labor is usually more costly, the program is able to reduce these costs by creating a pool of workers that are eligible to be promoted within the firm.

Columns 5-9 of Table 4 presents the results from the worker survey. Treatment workers are 7 percentage points more likely to report that they expect a promotion within the next six months (about 13% of the control group mean), and are about 0.15 percentage points more likely to request skill devel-

Figure 7A: Raw Promotion (Sewing)

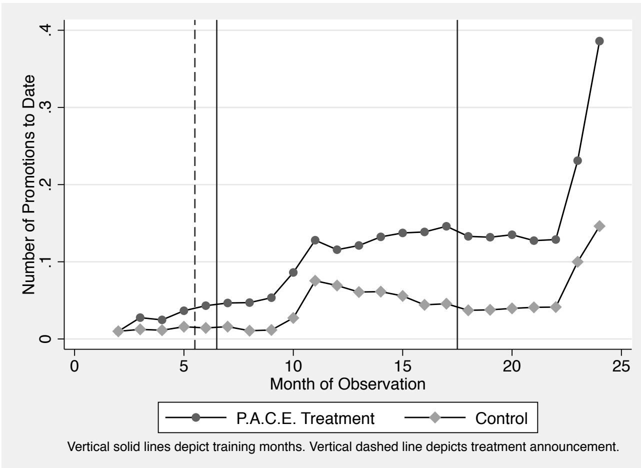
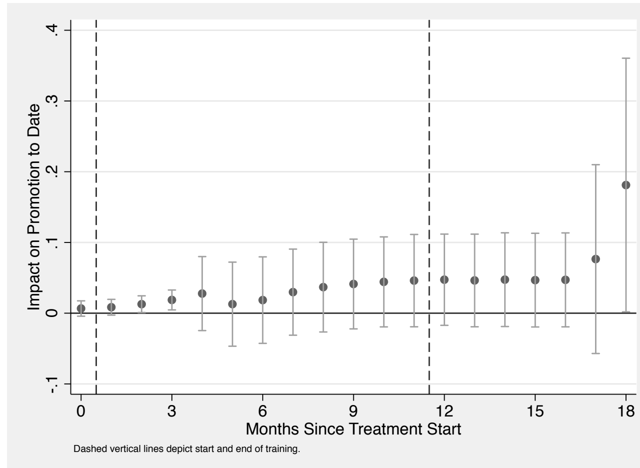


Figure 7B: Monthly Promotion (Sewing)



Figures 7A and 7B depict impacts of P.A.C.E treatment on promotion to date (i.e., helper becomes a tailor or tailor upgraders her skill level). Figure 7A depicts raw promotion to date from the payroll data across P.A.C.E treatment and control groups over the full observation period (January 1, 2013 onwards in the payroll data). Figure 7B depicts coefficients of monthly impacts from the preferred regression specification. These figures depict data for sewing department workers only as promotion is mostly relevant only for these workers. As shown in the corresponding results in Table 4, the pattern of impacts is nearly identical when including the small portion of non-sewing department workers for whom the promotion outcome is relevant (e.g., sampling). The corresponding full results are reported in Table A4 in the Appendix.

opment training (about 59% of the control group mean). In addition, When asked to rank themselves relative to workers in their production line (where a rating of 1 is the worst and 6 is best), they are more likely to rate themselves at a higher level. They are also more likely to report having received a production incentive or award, and rate themselves higher relative to relative to workers in their production line of the same skill level, although these two impacts are not statistically significant. These results indicate that workers subjective self-assessment, requesting skill development training and expectations of promotion all increase with treatment, in addition to actual promotion, which shows a large increase both during and after treatment.

Appendix Table A4 presents the month-by-month estimation results for cumulative man-days and promotion probability. As shown i the figures, promotion probabilities are higher for treatment workers after about 2 months of treatment, although the magnitude of the impacts are larger after the program.

5.4 Other Survey Measures and Mechanisms

Table 5 uses the worker-level survey data to test the impact of treatment on non-workplace outcomes for four aspects: financial behaviors and attitudes (shown in panel A), availing of firm and government programs (shown in panel B), personality (shown in Panel C), and mental health (shown in panel D).

Financial behaviors and attitudes tests whether treatment makes people more likely to save, whether it makes people more likely to save for children's education, and risk-aversion. The results indicate that there is a positive and statistically significant impact on saving for children's education, and the impacts are quite large relative to the control group mean (about 24% of the control group mean). Treatment workers do appear more likely to save in general and be less risk-averse, but the impacts are not statistically significant.

As shown in panel B, in line with increased effectiveness in information acquisition, treated workers are much more likely to avail of firm and government welfare programs - the impacts on binary indicators of availing of government pension, government subsidized healthcare, firm subsidized schooling, and firm subsidized housing all indicate that treated workers are more likely to avail of these programs. Furthermore, for all of these outcomes, the magnitude of the impacts are very large, nearly 100% or more of the control group mean.

The third aspect considers personality characteristics - we consider conscientiousness, locus of control, perseverance, extravertedness, and self-sufficiency. Treatment has a large positive and statistically significance on extravertedness, although the other personality characteristics do not show statistically significant impacts.

Finally, panel D indicates the impact on mental health, as measured by self-esteem, hope/optimism, and moderate and severe mental distress. There is no statistically significant impact of treatment on these outcomes.

5.5 Additional Outcomes

5.5.1 Attendance: Presence, Unauthorized Absence, and Tardiness

Additional outcome variables of interest related to attendance 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 informed the employer), and tardiness (a binary variable that is 1 if the worker was late and 0

Table 5: Impacts of P.A.C.E. Treatment on Survey Measures

	(1)	(2)	(3)	(4)	(5)
Panel A: Financial Behaviors and Attitudes	Any Savings	Saving for Child's Education	Risk Aversion Index		
P.A.C.E. Treatment	0.0400	0.0628*	-0.185		
	(0.0409)	(0.0354)	(0.115)		
Control Group Mean of Dependent Variable	0.543	0.260	3.381		
Panel B: Government and Firm Entitlements	Gov. Pension	Gov. Subsidized Healthcare	Firm Subsidized Housing	Firm Subsidized Schooling	
P.A.C.E. Treatment	0.0232*	0.0234***	0.0142*	0.0236*	
	(0.0137)	(0.00886)	(0.00752)	(0.0131)	
Control Group Mean of Dependent Variable	0.038	0.003	0.010	0.022	
Panel C: Personality	Conscientiousness	Locus of Control	Perserverance	Extravertedness	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	Self-Esteem	Hope/Optimism	Moderate Distress	Severe Distress	
P.A.C.E. Treatment	-0.158	-0.0634	-0.0147	0.0110	
	(0.113)	(0.0837)	(0.0270)	(0.0118)	
Control Group Mean of Dependent Variable	0.049	0.027	0.089	0.022	
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.

if not).[9] Note that as described in Section 5.1, since retention is affected by treatment in the first few months of the program, we need to reweight the regressions with the inverse probability of retention computed by a probit regression of the retention binary variable on the treatment binary variable and its interaction with month by year fixed effects and each baseline characteristic reported in Table 1. From this regression, we predict the probability that the worker is in the data for each day, and use the inverse of this predicted probability as the probability weights or sample weights.

Appendix Table A5 illustrates the impacts of treatment on outcomes during announcement of treatment assignment and during and after program implementation (relative to before announcement of the treatment assignment). 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, although as the figures show, there is an increase in worker presence and decrease in unauthorized leave in the first two months of the program for treated workers. Appendix Table A6 presents the regression results of the month-by-month estimation. The results indicate that reatment workers are more likely to attend work in the first two months of the program, and absences are more likely to authorized during the same months. Worker tardiness does not appear to be impacted during or after treatment.

5.6 Spillovers on Co-Workers of P.A.C.E. Treated Workers

This section considers the impact of the program on workers who enrolled in the lottery to receive the training but did not receive it and who work with workers who did receive the training (control workers in treatment lines) with workers who enrolled in the lottery to receive the training but did not receive it and who work in lines without any treated workers (control workers in control lines). Table 6 presents the results for all the workplace outcomes of interest for spillovers (the weighting of the regressions, when necessary, is done exactly in line with the estimation of the main treatment effects).

Panel A presents the impacts on retention and attendance. There are no impacts on any of the outcomes on retention, attendance, or latecoming, although the direction of the impacts is in most instances as expected (e.g. decrease in authorize absence). Panel B presents the results for man 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 much stronger result for cumulative man days - control workers who work with treated workers work for about 7.8 more days during treatment relative to control workers. Furthermore, efficiency, pieces produced and SAM show

[9] All three variables are conditional on retention, so they are missing if the worker left the firm.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Retention and Attendance	Retained		Present	Unauthorized Absent	Tardy		
	Attendance Roster	***Payroll*** Roster					
After X Spillover	-0.0177	-0.0155	-0.00118	-0.00251	-0.00531		
	(0.0246)	(0.0257)	(0.00944)	(0.00820)	(0.0174)		
During X Spillover	0.0361	0.0386	-0.00312	0.00136	-0.00174		
	(0.0234)	(0.0239)	(0.00760)	(0.00740)	(0.0157)		
Announced X Spillover	0.0173	0.0197	0.0182	-0.0171	-0.00713		
	(0.0161)	(0.0177)	(0.0112)	(0.0114)	(0.0115)		
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	628,218	628,218	479,574		
Control Mean of Dependent Variable	0.628	0.656	0.893	0.097	0.367		
Panel B: Production	Working		Cumulative Man Days		Efficiency	Pieces Produced	SAM (Operation Complexity)
	Attendance	***Production***	***Attendance***	***Production***			
After X Spillover	-0.0163	0.0318	8.867	5.211	0.0635*	6.226*	0.0477**
	(0.0207)	(0.0484)	(9.079)	(8.173)	(0.0335)	(3.615)	(0.0205)
During X Spillover	0.0297	0.0579	7.780**	3.487	0.00725	-0.210	0.0174
	(0.0211)	(0.0438)	(3.568)	(2.938)	(0.0165)	(1.958)	(0.0134)
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	1,102,880	562,478	562,478	216,002	216,002	216,002
Control Mean of Dependent Variable	0.519	213.714	0.390	109.642	0.547	62.577	0.567
Panel C: Career Advancement	Levels Promoted to Date	Expect Promotion Next 6 Mos	Skill Development Training	Production Award or Incentive	Peer Self-Assessment	Line Co-Worker Self-Assessment	
After X Spillover	0.0544**						
	(0.0256)						
During X Spillover	0.0354**						
	(0.0170)						
Announced X Spillover	0.00714						
	(0.00474)						
Spillover		-0.0383	0.0168	0.0116	0.132*	0.0933	
		(0.0493)	(0.0584)	(0.0226)	(0.0717)	(0.0704)	
Fixed Effects	Unit X Month X Year, Worker	Unit					
Weights	Inverse Predicted Probability from Probit of Retention on Treatments X Mo-Yr X Baseline Characteristics						
Observations	22,878	527	527	527	527	527	
Control Mean of Dependent Variable	0.045	0.567	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. 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. Obersvations in attendnace 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.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Survey (Financial Behaviors, Government and Firm Entitlements)	Any Savings	Saving for Child's Education	Risk Aversion Index	Gov. Pension	Gov. Subsidized Healthcare	Firm Subsidized Housing	Firm Subsidized Schooling
Spillover	0.0723	0.0496	-0.107	0.0107	0.0265**	-0.00724	0.0335*
	(0.0506)	(0.0417)	(0.137)	(0.0178)	(0.0127)	(0.00489)	(0.0176)
Control Group Mean of Dependent Variable	0.544	0.266	3.411	0.027	0.004	0.008	0.023
Panel B: Survey (Personality)	Conscientiousness	Locus of Control	Perserverance	Extravertedness	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 C: Survey (Mental Health)	Self-Esteem	Hope/Optimism	Moderate Distress	Severe Distress			
Spillover	-0.169*	-0.0949	0.00632	0.00412			
	(0.0974)	(0.0956)	(0.0279)	(0.0124)			
Control Group Mean of Dependent Variable	0.057	0.051	0.091	0.019			
Fixed Effects	Unit	Unit	Unit	Unit	Unit	Unit	Unit
Weighted	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	527	527	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. 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.

spillover impacts nearly as large as actual treatment. Panel C presents the results for career advancement variables. As in the productivity and task complexity outcomes, the spillover impacts on the probability of being promoted are about as large as the actual treatment. The worker survey outcomes on expected probability of promotion, requesting skill development training, receiving a production incentive or self-assessment relative to workers of the same skill in their production line are not statistically significant. Workers who work with treated workers are more likely to rate themselves higher when asked to assess themselves relative to workers in their production line-level overall, though the impacts are weakly statistically significant.

Overall, for workplace outcomes, we see strong spillover impacts on the cumulative man days accrued to the firm, efficiency and pieces production, task complexity, and the probability of promotion, and many of these impacts are nearly as large as actual treatment.

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. Thus, impacts on non-workplace outcomes do not show any robust spillover impacts.

6 Conclusion

We conclude with back-of-the-envelope calculations of the rate of return of the P.A.C.E. training program. We report in Table 8 calculations of the full costs of the P.A.C.E. training program, and detailed calculations of the benefits to the firm in terms of additional man days and incremental productivity from treated workers. We ignore spillover impacts and focus on direct treatment benefits only. Table 8 first outlines costs of the program, both overhead costs and variable costs. The overhead costs are primarily the costs of hiring two full-time trainers per factory for the entirety of the program. The variable costs are from lost production hours. For the 1,087 treated workers total program costs are about $ 105,339, about $30,000 of which are overhead costs, and the remaining are variable costs.

Details on profit margins on additional revenue both from an additional man-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 man-days accrued to the firm, and higher productivity. At the end of the first year after the program, the net present value of the these benefits are nearly $ 236,000, most of which (about $ 208,000) are the result of higher productivity. The net rate

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)	-$75,274.34
Total Cost *(All numbers in present value)*	-$105,339.60
1 Year After Program Announcement	
Additional Man Days	$28,034.44
Additional Productivity	$207,753.60
Net Present Value of Subtotal	$235,788.00
Net Rate of Return	**124%**
20 Mos After Program Announcement	
Additional Man Days (End of Observation)	$30,516.44
Additional Productivity (Garments per 8 hr day)	$516,768.70
Net Present Value of Subtotal	$547,285.10
Net Rate of Return	**420%**

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.

of return is thus 124 % after one year. This increases to a rate of return of 420% 20 months after the program. Thus, the P.A.C.E. training program had lasting positive returns to the employer, indicating that soft-skills training can not only be taught to adults, but that the workplace impacts of such training can be positive and lasting.

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A Additional Results

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 (PDSM)	Basic concepts in PSDM, problem analysis and solution finding, creative thinking for solutions,, problem-solving in groups and accountability, consenus-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 legal system and structure, and their 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**	0.0991**
	(0.0229)	(0.0157)	(0.0254)	(0.0171)	(0.0182)	(0.0423)
Treatment Month 2 X Treatment	0.0500**	0.0224	0.0492*	0.0182	0.0467**	0.121***
	(0.0229)	(0.0174)	(0.0257)	(0.0184)	(0.0205)	(0.0368)
Treatment Month 3 X Treatment	0.0605**	0.0363*	0.0618**	0.0354*	0.0490**	0.0963**
	(0.0235)	(0.0187)	(0.0263)	(0.0192)	(0.0201)	(0.0394)
Treatment Month 4 X Treatment	0.0682***	0.0425**	0.0652**	0.0366*	0.0603***	0.102***
	(0.0242)	(0.0198)	(0.0268)	(0.0205)	(0.0201)	(0.0345)
Treatment Month 5 X Treatment	0.0876***	0.0630***	0.0909***	0.0633***	0.0666***	0.135***
	(0.0242)	(0.0217)	(0.0274)	(0.0219)	(0.0207)	(0.0383)
Treatment Month 6 X Treatment	0.0806***	0.0571**	0.0840***	0.0587**	0.0660***	0.0811*
	(0.0248)	(0.0249)	(0.0276)	(0.0253)	(0.0199)	(0.0415)
Treatment Month 7 X Treatment	0.0727***	0.0440	0.0768***	0.0465	0.0470**	0.0942**
	(0.0259)	(0.0284)	(0.0284)	(0.0294)	(0.0202)	(0.0424)
Treatment Month 8 X Treatment	0.0591**	0.0256	0.0635**	0.0264	0.0360*	0.0572
	(0.0259)	(0.0284)	(0.0289)	(0.0295)	(0.0213)	(0.0460)
Treatment Month 9 X Treatment	0.0469*	0.0177	0.0496*	0.0182	0.0284	0.0589
	(0.0253)	(0.0283)	(0.0280)	(0.0292)	(0.0207)	(0.0430)
Treatment Month 10 X Treatment	0.0413	0.0104	0.0459	0.0123	0.0280	0.0895**
	(0.0255)	(0.0286)	(0.0283)	(0.0295)	(0.0195)	(0.0415)
Treatment Month 11 X Treatment	0.0286	-0.00244	0.0302	-0.00297	0.0122	0.0911**
	(0.0254)	(0.0287)	(0.0281)	(0.0299)	(0.0198)	(0.0401)
Post Treatment Month 1 X Treatment	0.0317	-0.00164	0.0327	-0.00395	0.0118	0.0693*
	(0.0250)	(0.0277)	(0.0282)	(0.0289)	(0.0203)	(0.0396)
Post Treatment Month 2 X Treatment	0.0358	0.00534	0.0374	0.00418	0.0180	0.0871**
	(0.0242)	(0.0270)	(0.0269)	(0.0279)	(0.0208)	(0.0406)
Post Treatment Month 3 X Treatment	0.0377	0.00849	0.0399	0.00712	0.0209	0.0835*
	(0.0238)	(0.0274)	(0.0271)	(0.0285)	(0.0199)	(0.0452)
Post Treatment Month 4 X Treatment	0.0364	0.0105	0.0387	0.0101	0.0236	0.0712
	(0.0234)	(0.0269)	(0.0262)	(0.0277)	(0.0203)	(0.0433)
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)	(0.0427)
Post Treatment Month 6 X Treatment	0.0392*	0.0107	0.0413	0.00955	0.0232	0.0897**
	(0.0236)	(0.0268)	(0.0266)	(0.0277)	(0.0191)	(0.0441)
Post Treatment Month 7 X Treatment	0.0372	0.0103	0.0405	0.0125	0.0231	0.0915**
	(0.0236)	(0.0266)	(0.0263)	(0.0274)	(0.0191)	(0.0443)
Post Treatment Month 8 X Treatment	0.0293	-0.00220			0.0117	0.0615
	(0.0235)	(0.0259)			(0.0196)	(0.0423)
Post Treatment Month 9 X Treatment	0.0191	-0.00333			0.000313	0.0564
	(0.0206)	(0.0210)			(0.0175)	(0.0430)
Fixed Effects	Unit X Month X Year, Worker					
Observations	2,078,400	1,433,981	62,585	43,141	1,848,003	666,038
Control Mean of Dependent Variable	0.589	0.628	0.619	0.656	0.480	0.376

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)	(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. 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 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.

Figure A1: Retention Impacts by Baseline Attendance

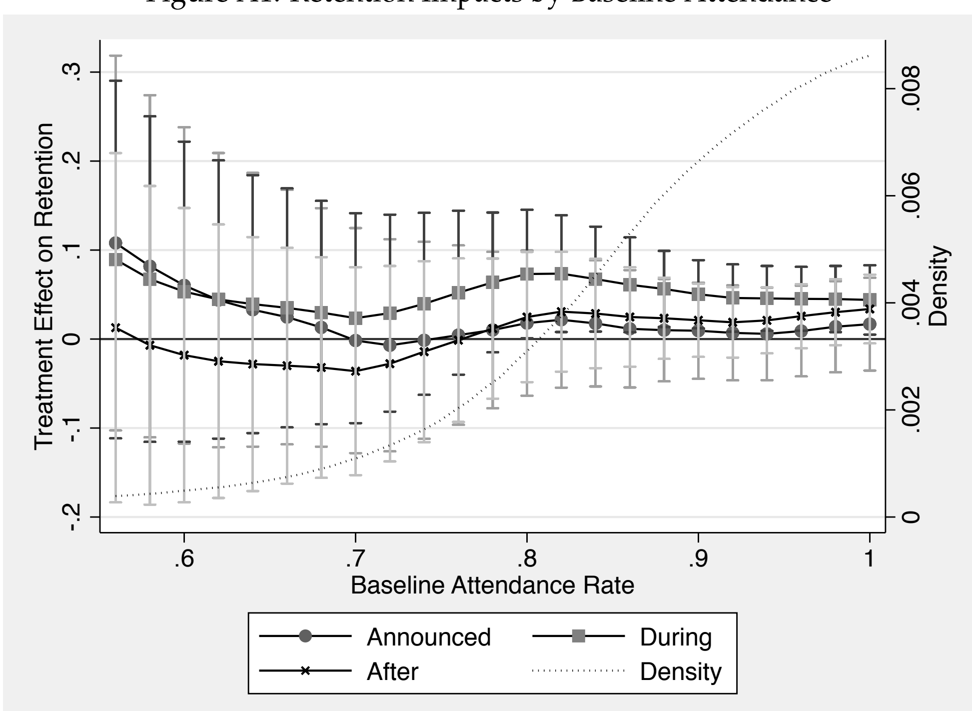

Figure A1 depicts impacts of P.A.C.E. treatment on retention along the distribution of baseline attendance.

Table A4: Monthly Impacts of P.A.C.E. Treatment on Cumulative Man-Days and Promotion

	(1)	(2)	(3)
	Cumulative Man Days		Levels Promoted to Date
	Sum of Days Working for Each Worker to Date		# of Levels Promoted Since Start of Observation in Salary Data
	Attendance Roster	***Production Data***	
	(Whole Sample)	*(Sewing Dept Only)*	*(Sewing Dept Only)*
Announcement Month X Treatment	-1.063		0.00659
	(4.946)		(0.00553)
Treatment Month 1 X Treatment	0.266	-0.239	0.00840
	(5.045)	(0.840)	(0.00568)
Treatment Month 2 X Treatment	1.346	1.201	0.0127**
	(4.919)	(1.218)	(0.00602)
Treatment Month 3 X Treatment	2.991	2.480*	0.0187**
	(4.626)	(1.440)	(0.00718)
Treatment Month 4 X Treatment	4.449	3.266*	0.0277
	(4.477)	(1.712)	(0.0267)
Treatment Month 5 X Treatment	6.581	4.683**	0.0128
	(4.571)	(2.080)	(0.0303)
Treatment Month 6 X Treatment	8.755*	5.926**	0.0185
	(4.467)	(2.497)	(0.0312)
Treatment Month 7 X Treatment	10.84**	6.785**	0.0298
	(4.523)	(3.047)	(0.0311)
Treatment Month 8 X Treatment	12.37**	7.291**	0.0368
	(4.754)	(3.605)	(0.0323)
Treatment Month 9 X Treatment	13.44***	7.162*	0.0412
	(4.998)	(4.191)	(0.0324)
Treatment Month 10 X Treatment	14.65***	7.723	0.0442
	(5.381)	(4.721)	(0.0325)
Treatment Month 11 X Treatment	15.47***	8.535	0.0460
	(5.849)	(5.149)	(0.0333)
Post Treatment Month 1 X Treatment	16.11**	9.154	0.0473
	(6.356)	(5.613)	(0.0329)
Post Treatment Month 2 X Treatment	16.74**	9.718	0.0463
	(6.911)	(6.143)	(0.0334)
Post Treatment Month 3 X Treatment	17.64**	10.56	0.0474
	(7.431)	(6.657)	(0.0338)
Post Treatment Month 4 X Treatment	18.70**	11.00	0.0467
	(8.096)	(7.221)	(0.0338)
Post Treatment Month 5 X Treatment	19.30**	11.42	0.0471
	(8.603)	(7.718)	(0.0339)
Post Treatment Month 6 X Treatment	20.25**	11.84	0.0765
	(9.064)	(8.246)	(0.0681)
Post Treatment Month 7 X Treatment	21.19**	13.07	0.181*
	(9.585)	(8.728)	(0.0915)
Post Treatment Month 8 X Treatment	22.15**	13.53	
	(10.04)	(9.233)	
Post Treatment Month 9 X Treatment	22.74**	13.72	
	(10.76)	(9.653)	
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	666,038	26,820
Control Mean of Dependent Variable	201.408	105.763	0.045

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

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. 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 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.

Figure A2: Retention Impacts by Baseline Tenure

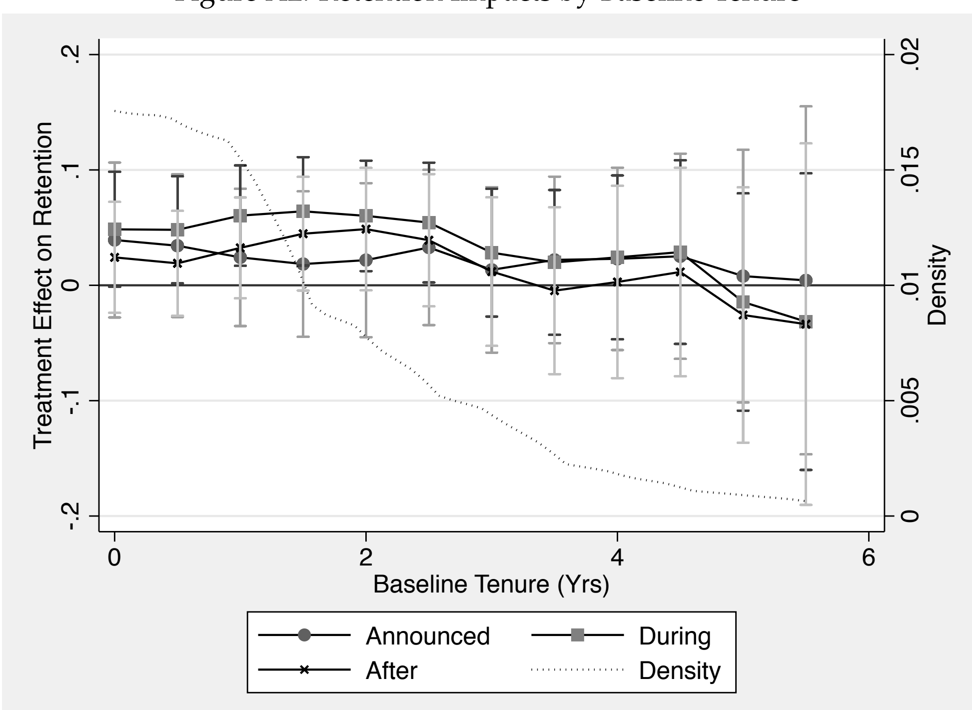

Figure A1 depicts impacts of P.A.C.E. treatment on retention along the distribution of tenure at baseline.

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. 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 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.

Figure A3: Retention Impacts by Baseline Skill Level

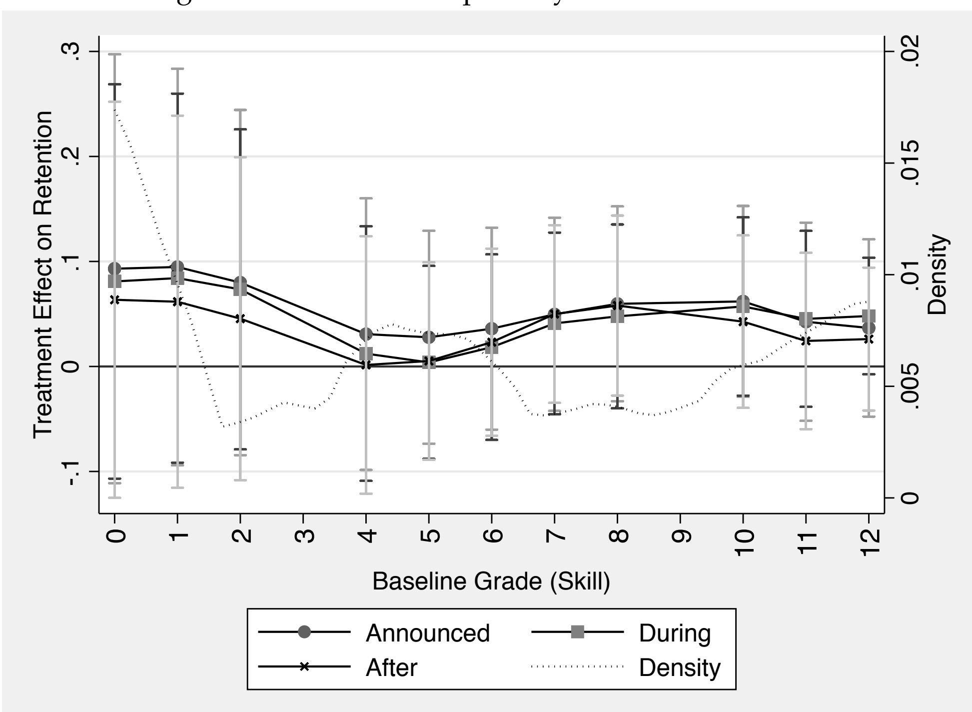

Figure A1 depicts impacts of P.A.C.E. treatment on retention along the distribution of skill grade at baseline.

Figure A4: Retention Impacts by Baseline Education

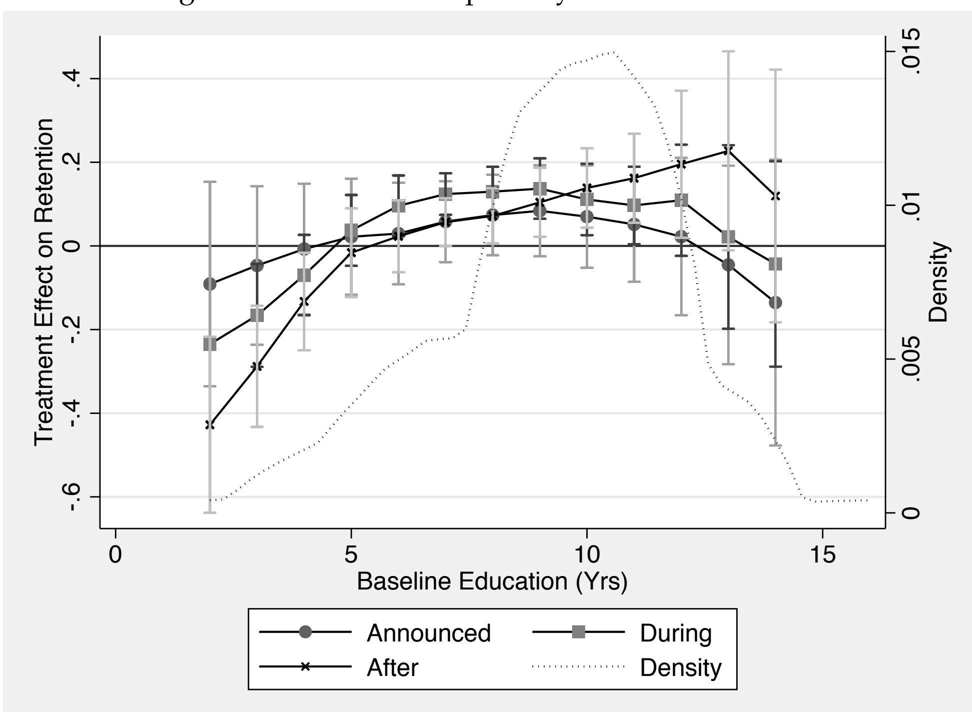

Figure A1 depicts impacts of P.A.C.E. treatment on retention along the distribution of education at baseline.

Figure A5: Retention Impacts by Baseline Age

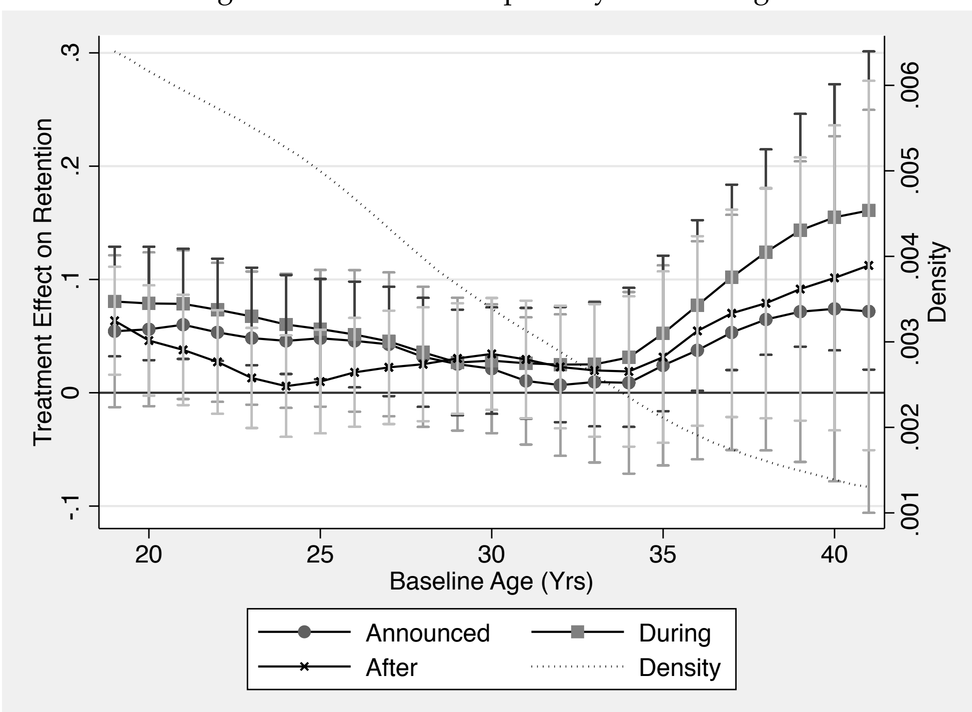

Figure A1 depicts impacts of P.A.C.E. treatment on retention along the distribution of age at baseline.

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.

- *Promoted to Date*: An indicator variable that is 1 if the worker's salary showed a permanent increase in month m in year y, and 0 otherwise. 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 Mos*: 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 though they would 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 though they would 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 supervisors 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 supervisors 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 supervisors 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 supervisors 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 supervisors 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 supervisors identify with that are predictive of self-esteem. Supervisors 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 supervisors identify with that are predictive of hope or optimism. Supervisors 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 anxietydepression spectrum mental distress (**?**). 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 interemediate 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 and Severe Mental Distress are measured using different cutoff rules from the score in this scale. Moderate mental distress is indicated by a score of 24 or higher, and severe mental distress is indicated by a score of 30 or higher.