DISCUSSION PAPER SERIES

IZA DP No. 8552

Learning and Earning: Evidence from a Randomized Evaluation in India

Pushkar Maitra
Subha Mani

October 2014

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

Learning and Earning: Evidence from a Randomized Evaluation in India

Pushkar Maitra
Monash University

Subha Mani
CIPS, Fordham University,
PSC, University of Pennsylvania and IZA

Discussion Paper No. 8552
October 2014

IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

IZA Discussion Paper No. 8552
October 2014

ABSTRACT

Learning and Earning: Evidence from a Randomized Evaluation in India*

This paper presents the treatment effects from participating in a subsidized vocational training program targeted at women residing in low-income households in India. We combine pre-intervention data with two rounds of post-intervention data from a randomized field experiment to quantify the 6- and 18-month treatment effects of the program. The 6-month effects of the program indicate that women who were offered the training program are 6 percentage points more likely to be employed, 4 percentage points more likely to be self-employed, work 2.5 additional hours per week, and earn 150 percent more per month than women in the control group. Using a second round of follow-up data collected 18 months after the intervention, we find that the 6-month treatment effects are all sustained over this period. Our findings indicate credit constraints, distance, and lack of proper child care support as important barriers to program completion. Further, we also rule out two alternative mechanisms – signalling and behavior that could drive these findings. Finally, a simple cost-benefit analysis suggests that the program is highly cost-effective.

JEL Classification: I21, J19, J24, 015

Keywords: vocational training, panel data, India, economic returns, field experiment

Corresponding author:

Subha Mani
Fordham University
Department of Economics
441 East Fordham Road
Dealy Hall, E 520
Bronx, New York 10458
USA
E-mail: smani@fordham.edu

* We would like to thank Utteeyo Dasgupta, Lata Gangadharan, Samyukta Subramanian, and Shailendra Kumar Sharma for their helpful comments, suggestions, and tremendous support throughout the project. We have also benefitted from comments by Jere R. Behrman, Erlend Berg, Jing Cai, Pascaline Dupas, John Hoddinott, Alfonso Flores-Lagunes, Kim Lehrer, Pieter Serneels, John Strauss, Choon Wang, seminar and conference participants at Fordham University, University of Pennsylvania, University of Southern California, Hunter College-CUNY, University of Reading, University of Manchester, University of Oxford, LSE, Namur, Jadavpur University, Monash University, participants at the Australasian Labour Econometrics Workshop, AEA meetings, PAA meetings, SOLE Meetings, the IZA-World Bank Conference on Growth and Development, and the IGC-ISI Conference. Special thanks to Rukmini Banerji and Bharat Ramaswami for their help. Neha Agarwal, Bailey Evans, Tushar Bharati, Tyler Boston, Subhadip Biswas, Shelly Gautam, Peter Lachman, Inderjeet Saggu, Sarah Scarcelli, and Raghav Shrivastav provided excellent research assistance at various stages of the project. This study acknowledges funding support from Monash University, Fordham University, the ADRA scheme, Grand Challenges Canada (Grant 0072-03 to the Grantee, The Trustees of the University of Pennsylvania), and IGC-India Central. The findings and conclusions contained within are those of the authors and do not necessarily reflect positions or policies of Grand Challenges Canada, IGC-India Central, or other funders. We are especially grateful to Jogendra Singh, Tasleem Bano, and the rest of the staff at SATYA, and Pratham for their outstanding work in managing the implementation of the vocational training program. The usual caveat applies.

1 Introduction

In recent years, continued low levels of school completion combined with high rates of unemployment and increased opportunity cost of obtaining formal education among young adults has renewed the focus on the "Young and Unemployed". The 2013 World Development Report on "Jobs" notes that, "*200 million people, a disproportionate share of them youth, are unemployed and actively looking for work. Almost 2 billion working-age adults are neither working nor looking for work; the majority of these are women, and an unknown number are eager to have a job"* (WDR, 2013, page 48). Despite the importance of youth unemployment in low-and-middle income countries, there is little knowledge on how to create smooth school-to-work transitions in these countries, and/or how to improve the human capital of those who can no longer return to school. Vocational training is viewed as a promising avenue through which young adults can acquire marketable skills that can enable them to secure employment.[1] However, despite the large scale expansionary policies aimed at increasing access to vocational training programs, evidence on the effectiveness of training programs in developing countries is rather limited. Experimental evidence is particularly scarce and none from Asia.

This paper quantifies the economic returns from participating in a subsidized vocational training program in stitching and tailoring, targeted at women between ages 18 and 39 years, with at least 5 or more grades of schooling residing in low socio-economic areas or slums of New Delhi, India. Applicants to this training program were randomly assigned to one of the following two groups: treatment group (received access to the 6-month training) and the control group (did not receive access to the training). The 6-month treatment effects of the program indicate that women who were offered the training program are 6 percentage points more likely to be employed, 4 percentage points more likely to be self-employed, work 2.5 additional hours per week, and earn 150 percent more per month than women in the control group. Using a second round of follow-up data collected 18 months after the intervention, we find that the 6-month impact estimates on employment, hours worked, and earnings are all sustained over this longer period. In addition to evaluating the benefits from receiving vocational training, this paper also examines the barriers to program take-up and completion. Further, we rule out two alternative pathways that can explain the impacts of the training program.

In recent years, randomized evaluations of vocational training programs (closely related

[1]India, Argentina, Chile, Peru, Uruguay, are some of the developing countries that have designed such programs. See Annex 2 of Betcherman et al. (2004) for a complete list of countries and details on skill building and other labor market training programs.

to ours) have been conducted in the Dominican Republic (Card et al., 2011), Colombia (Attanasio et al., 2011), Malawi (Cho et al., 2013), and Turkey (Hirshleifer et al., 2014). The results from these studies are mixed. Attanasio et al. (2011), who study the effects of a vocational training program aimed at disadvantaged youth in Colombia in 2005, find that the program raised earnings and employment for women. Card et al. (2011) find that a government subsidized training program for low-income youth in urban areas of the Dominican Republic only marginally improved hourly wages, and the probability of health insurance coverage, conditional on employment, but find no significant impact of the training program on the subsequent employability of trainees. Cho et al. (2013) in their analysis of the effects of vocational and entrepreneurial training for Malawian youth find that the training results in skills development, continued investment in human capital, and improved well-being for men, but no effects on labor market outcomes in the short run. Women in their setting do not gain at all from the training program. Hirshleifer et al. (2014) analyze a vocational training program for the unemployed in Turkey and find that while the average impact of training on employment is positive, it is close to zero in magnitude and statistically insignificant. They also find virtually no persistence of any effect over a longer period.[2]

To the best of our knowledge, there are no experimental impact evaluation studies of vocational training programs in Asia and in particular, India. The high levels of economic growth accompanied by rising inequality and skill shortage as experienced by India makes it an important setting to evaluate the effectiveness of labor market training programs. Recent surveys conducted by the World Bank and the Federation of Indian Chamber of Commerce and Industry (FICCI) ally these concerns (Blom and Saeki, 2011, FICCI, 2011). The Economist in a recent opinion piece adds to this concern by stating, "... *a lot of training is required. Many of India's young leave school ill-prepared even for rudimentary jobs*", (Angry Young Indians, The Economist, May $11^{th} - 17^{th}$, 2013, page 12). These problems are however not restricted only to India. Worldwide recession along with increasing unemployment necessitate that workers accumulate additional skills to obtain new jobs and or retain current ones.

The rest of the paper is organized as follows. Section 2 provides a complete description

[2] Recent studies in Africa, which offered vocational training in conjunction with a cash grant (Blattman et al., 2014) or with training in life skills (Bandiera et al., 2012) found positive impacts on employment and/or earnings. Macours et al. (2012) find that in the context of Nicaragua access to vocational training in conjunction with a conditional cash transfer program enabled households to insure against weather related shocks. On the other hand Groh et al. (2012) find that soft skills training program provided to female graduates in Jordan has had very limited impact. Note that skills training is only one component of these impact evaluation studies and hence the results from these four papers are not directly comparable to what we do in this paper.

of the intervention and the data. The 6-month and 18-month post-training impact estimates are presented in Section 3. Section 4 discusses the barriers to program completion. Alternative explanations that could potentially drive the results are discussed in Section 5. Finally, concluding remarks, including a cost-benefit analysis of the program follow in Section 6.

2 Design

2.1 The Program

We examine the effects of having access to and completing a vocational training program in stitching and tailoring services designed and implemented by two non-governmental organizations (NGOs): Pratham and SATYA (Social Awakening Through Youth Action). The program will henceforth be referred to as the SATYA-Pratham program.

During May 2010, a complete census was administered in the targeted areas in New Delhi.[3] While the targeted areas are commonly referred to as *slums*, these are permanent settlements with concrete houses, and some public amenities (electricity, water, etc.). To be more specific, these are *resettlement colonies*, typically $10-20$ years old, that have absorbed migrants from other parts of the country during New Delhi's expansion in the 1980's and 1990's. All women, between ages 18 and 39 years, with at least five completed grades of schooling and residing in the target areas were eligible to apply to the program. These women were informed of the program through an extensive advertising campaign that lasted for almost 3 weeks. The women were invited to apply to have a chance at being selected to receive this training. The English version of the advertisement for the program is presented in Figure 1, though the actual pamphlets handed out were in Hindi. The advertisement of the training program was not targeted to any specific sub-group in the population, and was distributed to every household in the target area to ensure maximum outreach for the program. The description of the program was kept general enough to encourage all eligible women to apply in order to avoid attracting only women with specific characteristics. The advertisement pamphlets and information sessions specified that training would be provided by well-qualified and reputable staff, using modern techniques of stitching and tailoring. In addition, they were informed that new sewing machines and other related resources would

[3]This was a pre-baseline census that was used to generate basic information on all households located in the target areas (South Shahdara and North Shahdara region in New Delhi, India). The census conducted in May 2010 used a standard house listing method to list details on the names of all household members and collected information on their age and highest grade of schooling. The house listing/census survey was conducted by Pratham.

be provided on site. The participants were further told that they would receive a certificate at the end of the program.

Through this process, the potential applicants were therefore informed of the associated details of the program such as the location of the training centers, the extent of commitment required (participants were required to commit up to two hours per day in a five-day week), the method of selection (random), course content, and the expected time-span of the program (six-months, starting August 2010). They were also informed of the deposit requirement: all selected participants were required to deposit Rs 50 per month for continuing in the program with a promise from the NGOs that women who stayed through the entire duration of the program would be repaid Rs 350. This feature is unique to the program and was introduced by the two implementing NGOs to increase commitment and encourage regular attendance. The amount of Rs 50 per month was around one percent of the average household income for the population.[4] By the end of June 2010, Pratham received 658 applications.[5]

As a part of the pre-baseline census of the eligible population, the implementing organizations collected information on age and educational attainment for all eligible women in the population. A simple comparison of average age and completed grades of schooling between applicants (sample) and non-applicants (general population) suggests that applicants on average are younger (average age of applicants is 23.16 years compared to 25.4 years for non-applicants, the difference is statistically significant with a $p - value = 0.00$) and have completed more grades of schooling (on average applicants completed 9.15 grades of schooling whereas non-applicants completed 8.95 grades, the difference is statistically significant with a $p - value = 0.04$).[6] These differences between applicants and non-applicants is

[4] It is unlikely that the 50 Rupee deposit requirement acted as a major barrier to program participation. Informal interviews with women who were offered the chance to apply for the program but chose not to apply (the non-applicants) conducted by SATYA and Pratham employees revealed time constraints and a lack of interest in the specific program as the two main reasons for not applying. There was never any mention of the Rs 50 deposit requirement being a significant constraint. It is not surprising for NGOs to require such deposites. For example Ashraf et al. (2010) note that in their marketing experiment in Zambia, their collaborating NGO suggests that, "paying something results in more drinking-water use than paying nothing". Note also that this deposit requirement was always specified as Rs 50 per month as opposed to Rs 300 in total.

[5] The pre-baseline census identified 7525 eligible (between the age of 18 and 39 years with at least five or more completed grades of schooling) women in these slums. Of the eligible women, approximately 9 percent applied to the training program. This is a fairly small proportion of the eligible population. It is therefore unlikely for this program to generate general equilibrium effects: for example it is unlikely that the intervention will create a large inflow of women tailors in the population and consequently, affect the equilibrium wages of tailors in this area.

[6] Age has an inverted u-shaped effect on the likelihood of choosing to apply – rising initially until the age of 23 and falling thereafter. Completed grades of schooling does not have a statistically significant effect on the probability of applying to the program.

not surprising and echoes the selection problem persistent in non-experimental evaluation studies. In a companion paper, Dasgupta et al. (2014), we examine the decision to apply to the program and find that applicants and non-applicants differ along both demographic characteristics and behavioral characteristics (like risk preferences, competitiveness, and confidence).

The program was offered in two slums, North and South Shahdara, and randomization was stratified by location. Two-thirds of all applications from each area were assigned to the treatment group and the remaining one-third were assigned to the control group.[7] The baseline survey was underway at the time of the lottery and we made sure that the applicants were not aware of their assignment status to the program at the time of the baseline survey. North Shahdara is a bigger geographical cluster, received more applications and had three training centers; the remaining two training centers were in South Shahdara. Women were assigned to the training center nearest to their home and for classes, allotted their most preferred time, though they had the option to change both, if they needed to. All of the instructors were females and the instructors jointly had a say in curriculum design. All program participants, irrespective of the center they were assigned to, received the same training.

2.2 Data

The baseline socioeconomic survey was implemented during July – August 2010 and attempted to survey all 658 women who applied to the program. This is the applicant sample. However during the 2010 baseline survey, data could only be collected for 594 applicants, mainly because of refusal to participate in the survey. None of the women who refused to participate in the survey participated in the program even if they were assigned to the treatment group. Of these 594 women, 409 belong to the treatment group, and the remaining 185 to the control group. The actual program started during the third week of August 2010 and continued through to the last week of January 2011. We conducted two rounds of follow-up data collection.[8] The midline survey was conducted during July – August 2011,

[7]The randomization was conducted as follows. First, every applicant was given an ID number. These ID numbers were written on chits of papers that were placed in a box. Specially recruited research assistants randomly drew chits from this box. The first two chits drawn were assigned to the treatment group, the third to the control group. This process was repeated until all applicants had been assigned to one of the two groups. High ranking officials from the two partner NGOs were present when the randomization was conducted.

[8] From a policy perspective it is critical to know if the program generates any immediate effects (as captured by the 6-month post-treatment impacts here) and if these effects continue to sustain over time. McKenzie and Woodruff (2012) argue, the impacts of training may differ between the short and medium term, so measuring outcomes at multiple points in time will provide a better understanding of whether

approximately 6 months after the training program was completed. The endline survey was conducted during July – August 2012, roughly 18 months after the training program was completed. The survey questionnaires were designed to collect detailed information on household demographic characteristics, background characteristics, labor market outcomes, measures of bargaining power, and life satisfaction. The list of outcome variables, that we use in our analysis, is presented in Panels A and B of Table 1.

2.2.1 Sample Balance

Table 2 presents the tracking rates of the applicants (658) disaggregated by group (treatment and control) and years (2010, 2011, and 2012). The tracking rates presented in Rows 3, 4, and 5 are measured in percentages and computed by dividing the number of women surveyed in each round by the total number of applicants for Row 3 (= 658)and the total number of applicants in the treatment group (= 442) and control group (=216) respectively for Rows 4 and 5. The tracking rate is considerably lower in the midline and the endline surveys. This is true for the full sample and the disaggregated samples (see Rows 3, 4, and 5). The baseline tracking rate is significantly higher for women in the treatment group (see Row 6, Column 1). This significant difference in the availability of baseline data between the treatment and the control group would result in selection bias if these differences continued to exist in the post-intervention survey rounds. Notice however that there is no difference in the tracking rate between the two groups both in 2011 and 2012 (see Row 6, Columns 2 and 3, Table 2).

Our goal during both rounds of follow-up data collection was to target and interview all 594 women surveyed in the baseline and *not* the original 658 applicants.[9] Of the 594 women surveyed in 2010 (the baseline sample), 504 were re-surveyed in 2011 (this is the 2010 – 2011 sample) and of these 504 women, 439 were re-surveyed in the 2012 round of the survey (this is the 2010 – 2011 – 2012 sample). Our enumerators were also able to trace an additional 52 women during 2012, who were not traced in 2011. Therefore 491 women were surveyed in 2010 and 2012 (this is the 2010 – 2012 sample). The survey completion rates measured

impacts continue to persist or not in the long run as it is unlikely for a program that has no effect beyond the short run to be cost-effective. At the same time given the substantial path dependent nature of labor market outcomes (see Narendranathan and Elias (1993) and Gregg (2001)), no impacts in the short run is likely to be a permanent outcome, that is, if there are no positive program effects in the short run, one is unlikely to observe any effects in the long run. The absence of short-run program effects may also suggest early on the need for complementary interventions that can make the program cost-effective. We believe that for designing and implementing a cost-effective intervention, it is essential to measure both the short-and-long-run effects of the intervention.

[9]This was partly driven by the human ethics committee requirement that we not pursue in subsequent rounds anyone who refused to answer the survey in any particular round.

in percentages (computed by dividing the number of women surveyed in each round by the number of women sought in the midline and endline surveys) is approximately 85 percent in both follow-up rounds and is higher than the tracking rates in both the years (see Row 8, Columns 2 and 3, Table 2). Additionally there is no significant difference in the survey completion rates between the treatment and the control group in 2011 and 2012 (see Row 11, Columns 2 and 3, Table 2).

An implication of our evaluation design is that none of the baseline characteristics must be significantly different between the treatment and the control group. To examine this, we first check for sample balance using the administrative data on age and completed grades of schooling (see Table 3). This data is available for all 658 applicants. For every sample (658 in the applicant sample, 594 in the baseline sample, 504 in the 2010 – 2011 sample, 491 in the 2011 – 2012 sample, and 439 in the 2010 – 2011 – 2012 sample) we have balance in age and completed grades of schooling between the treatment and control group, that is, the average difference in these characteristics between the treatment and control group is never statistically significant (see Column 4, Table 3).

Next we examine sample balance in more detail by focussing on the survey data. We report in Table 4 the pre-intervention averages of all variables used later in the analysis. The sample is restricted to the 439 women who are included in the 2010 – 2011 – 2012 sample. Columns 2 and 3 present sample averages for the treatment and the control group respectively. Column 4 reports mean differences between the two groups and the statistical significance of this difference. The results presented in Table 4 show that with the exception of ownership of sewing machine (higher for women in the control group), the sample is balanced in terms of labor market outcomes (Panel A), empowerment and life satisfaction measures (Panel B), and socioeconomic characteristics (Panel C) at baseline. The overall joint F-test on the regression of the treatment dummy on all baseline covariates (excluding ownership of sewing machine) shows that we cannot reject the null hypothesis that baseline characteristics of women does not predict assignment to treatment. Similar balance in baseline characteristics emerges in: the 2010 sample (594 women), the 2010 – 2011 sample (504 women), and the 2010 – 2012 sample (491 women) as reported in Tables A-1, A-2, and A-3 in the appendix.

The average woman in our sample is 22 years old and more than fifty percent of these women have not completed secondary schooling. About one-third of the women in our sample are married and there is an almost equal distribution of both Hindu and Muslim women in our sample. More than fifty percent of the women belong to scheduled castes.[10] In

[10]Scheduled castes are individuals who belong to the second lowest tier of the Hindu Caste System.

our sample, labor market participation rates are low to begin with – in terms of employment, hours worked, and also monthly earnings.

2.2.2 Attrition

Our identification strategy also assumes that there is no differential attrition between the treatment and the control group. To check this, we first use the administrative data and find that there is no significant difference in age and completed grades of schooling between attritors and non-attritors between the two groups in all three waves of the survey (see Table 5).

Second we examine how the baseline socioeconomic characteristics affect the likelihood of attrition. In Column 1 of Table 6 we present the marginal effects from a probit regression, where, the dependent variable is *Attrite*, which takes a value 1 if the woman could not be traced during either follow-up surveys (i.e., is not included in all three surveys) and 0 otherwise. The results show that being assigned to the treatment group does not have a statistically significant effect on the likelihood of attrition. We also find that baseline socioeconomic characteristics have no influence on attrition.

Finally, following Becketti et al. (1988) we also regress each of our outcome variables at baseline on baseline observables, the attrition dummy (*Attrite*), the treatment dummy (*Treatment*), and a full set of interaction terms (between the attrition dummy and each of the explanatory variables including the treatment dummy). Specifically we run the following regression:

$$\begin{aligned} y_i =& \alpha_0 + \alpha_1 Attrite + \alpha_2 Treatment + \alpha_3 \mathbf{X}_i \\ &= + \alpha_4 (Attrite \times Treatment) + \alpha_5 (Attrite \times \mathbf{X}_i) + \varepsilon_i \end{aligned} \tag{1}$$

This allows us to test whether the (ultimately) attriting women are different in terms of the baseline outcome variables. The non-interacted coefficients give us the effects for the non-attrited women while the interacted coefficients give us the difference between the attritors and non-attritors at the baseline. The results are presented in Table 7. The joint F statistics on the attrition dummy (*Attrite*) and all the interaction terms appended in Table 7 are never jointly significant. The null hypothesis that the attriting women are no different from the non-attriting women at the baseline is therefore never rejected, ruling out concerns of differential attrition biasing our impact estimates. Additionally the coefficient estimate associated with the interaction term (*Treatment* $\times$ *Attrite*) is also never

individually significant.[11]

We have a sample of 52 women who could not be surveyed in 2011 (during the midline survey) but could be re-surveyed in 2012 (endline survey). While our primary estimating sample excludes these women, we would like to ensure that these women are no different the primary estimating sample in terms of baseline characteristics. In Table 6, Column 2 we present the marginal effects from a probit regression where the dependent variable, *Attite_Temp*, takes the value of 1 if the woman is not surveyed in 2011 but is surveyed in 2010 and in 2012, that is, the woman attrites temporarily. Notice again that being assigned to the *Treatment* group does not have a statistically significant effect on the likelihood of temporary attrition. Our results are therefore not confounded by the exclusion of these temporary attritors.

3 Program Impacts

3.1 Treatment Effects

The availability of pre- and-post-intervention data from a randomized field experiment allows us to estimate the causal effect of being offered the *TRAINING* on a range of outcome variables of interest. We estimate the following equation, controlling for baseline differences in the outcome variables and also for any pre-intervention differences in socioeconomic characteristics between the treatment and the control group.

$$\begin{aligned} Y_{it} =& \beta_0 + \beta_1 Y_{i0} + \beta_2 TRAINING_i + \beta_3 YEAR_t + \beta_4 TRAINING_i \times YEAR_t \\ &+ \sum_{j=1}^{K} \gamma_j \mathbf{X}_{ij} + \epsilon_{it} \end{aligned} \tag{2}$$

Here Y_{it} is the outcome of interest for woman i in year t and Y_{i0} is the baseline outcome variable. The baseline outcome variable from 2010 is included in the right hand side to control for pre-intervention imbalance in outcomes (if any) between the treatment and the control group. Inclusion of the baseline outcome variables in the right hand side also allows for path dependence in labor market outcomes, which further improves the precision of the

[11] Since we can rule out selective attrition only for the 594 applicants and not the full sample of 658 applicants due to missing baseline data for 64 women, for the validity of our correlates in the attrition table, we need to assume that the women for whom baseline data is missing, would mimic, on average the women for whom the baseline data is available. This must hold for both the attritors and the non-attritors in both the treatment and the control group. Unfortunately, this assumption cannot be tested given the information that we have.

estimates. $TRAINING$ is a dummy variable that takes the value 1 if the woman is offered the training (i.e., is assigned to the treatment group), 0 otherwise. $YEAR$ is a dummy variable that takes the value 1 if year is 2012, 0 otherwise. $TRAINING \times YEAR$ is an interaction term; $\mathbf{X}$ is a set of additional individual and household level characteristics that control for any remaining pre-intervention differences between women in the two groups.[12] Finally, ϵ_{it} is a random i.i.d. disturbance term. So β_2 gives us the 6-month (i.e., over the period $2010 - 2011$) intent-to-treat (or ITT) effects of the vocational training program. The coefficient estimate on the interaction term, β_4 gives us the additional effect over the period $2011 - 2012$. The overall 18-month intent-to-treat effect of the program is given by $\beta_2 + \beta_4$. In all regressions that follow, standard errors are clustered at the individual level.

The set of baseline explanatory variables included in the regressions are: *Age* of the woman in years, *Completed secondary school* (= 1 if the woman completed ten grades of schooling; 0 otherwise), *SC* (= 1 if the respondent belongs to a scheduled caste; 0 otherwise), *Hindu* (= 1 if religion = Hindu; 0 otherwise), *Experienced in stitching and tailoring* (= 1 if woman reports sewing/stitching by hand for more than 7 hours during the last week; 0 otherwise), *Married* (= 1 if the woman is married; 0 otherwise), and *Dependency ratio* (the ratio of the number of children under age 5 to the number of adult females in the household). The regressions also include center dummies and a dummy for region of residence to account for region and center specific unobservables.

Using data from the 2010 – 2011 – 2012 sample, we report the 6-and 18-month ITT effects of the vocational training program in columns 1 and 3 of Table 8. In columns 5 and 7, we present the robustness of these results using alternative samples: column 5 presents the 6-month treatment effects using the 2010 – 2011 sample and column 7 the 18-month treatment effects using the 2010 – 2012 sample. Since the results are robust across the different samples, we will discuss the estimates corresponding to the 2010 – 2011 – 2012 (our primary estimating) sample below.

In only six months post program completion, the treatment has increased the likelihood of: casual or full-time employment by more than 5 percentage points, self-employment by almost 4 percentage points, and any (casual, full-time, and or self-employment) employment by 6 percentage points. Women in the treatment group work an additional 2.5 hours per week and earn Rs 138 more than women in the control group.[13] In percentage terms, these are large effects. The probability of being employed in the control group in 2011 is 6 percent. These women worked on average 1.12 hours a week and earned Rs 91 per month.

[12]Note that the impact estimates are robust to the exclusion of the lagged dependent variable in the right hand side

[13]Earnings are in real terms, base 2000, deflated using the Consumer Price Index of New Delhi.

Women in the treatment group work 2.5 times and earn 1.5 times as much as women in the control group.

The 18-months effects presented in Column 3 shows that the 6-month treatment effects have been sustained over the longer period. The coefficient estimate on the interaction term (β_4) is always positive, though not statistically significant. During 2011 – 2012, while overall the average for the control group has also increased (hours worked increasing from 1.12 to 3.30 and real monthly earnings from Rs 91 to Rs 207), the magnitude of the treatment effect has also increased in step.

TRAINING has a positive effect on ownership of capital goods and entrepreneurship. In the 6-month period after the intervention, women assigned to the treatment group (*TRAINING*) are 8 percentage points more likely to own a sewing machine (see Panel B in Table 8) – this effect is however not statistically significant ($p - value = 0.108$). Over the 18-month period, this likelihood increases to 13 percentage points and the effect is now statistically significant. This increase in the likelihood of owning a sewing machine could be viewed as a measure of entrepreneurship. During informal conversations with the applicants, we asked why they wished to participate in the program. A large proportion responded saying that they wanted, "to use this skill to increase income or set up their own small businesses". Purchasing a sewing machine can be viewed as the first step in this direction. On the other hand *TRAINING* has no effect on empowerment and happiness at home (see Panel B in Table 8).

The overall proportion of employed women in our sample in 2011 is 0.10, of whom 80 percent belong to the treatment group. The proportion employed increases to 0.15 in 2012, and once again 80 percent of those employed in 2012 belong to the treatment group. Finally, 95 percent of women who were employed in both 2011 and 2012 were assigned to the treatment group. This suggest that assignment to the treatment group increases the likelihood of both current and continued employment.

The results using the two subsamples (2010 – 2011 and 2010 – 2012) presented in Columns 5 and 7 are very similar (both qualitatively and quantitatively) to the estimates presented in Columns 1 and 3 respectively. Both the 6-month and 18-month effects are positive and in general the 18-month effects are larger than the 6-month effects. Achieving consistent results across the two sub-samples for the 6-month and 18-month treatment effects further alleviates attrition related selection concerns.

3.2 Inference with Multiple Outcomes

Typically the probability of a false positive, that is, Type I error increases in the number of outcomes tested. To rule this out we examine the ITT effects of the training program on summary indices using the approach outlined in Kling et al. (2007) and Karlan and Zinman (2009). By taking a weighted average over selected standardized outcome variables, we construct the following three summary indices: (*a*) an *all labor index* that includes employment, hours worked, and monthly earnings; (*b*) a *self index* that includes self employment and ownership of sewing machine; (*c*) an *all other outcome index* that includes happy at home and ROSCA membership.

We re-estimate equation (2) replacing the index measures as the outcome variables of interest. The 6-month intent-to-treat estimates of assignment to *TRAINING* are presented in Panel C of Table 8). On average, being offered the *TRAINING* improves the labor index by almost 0.33 standard deviation units. However, the training program has no impact on measures of empowerment and happiness. We reject the null hypothesis that the *TRAINING* has no effect on labor market outcomes in both the 6-month and the 18-month period at the 5% level of significance. Rejection of the null here alleviates concerns relating to incorrect inference associated with multiple outcome variables.

We also compute average effect size using the method outlined in Kling et al. (2007) and Clingingsmith et al. (2009). The average effect size is constructed by taking a weighted average of individual treatment effects over the standardized outcome variables using a system of Seemingly Unrelated Regression (SUR) equations. This allows for correlation between the error terms across the equations and improves the precision of the treatment effects thereby reducing type II error, i.e., the risk of attaining low statistical power. We find that assignment to the treatment increases the 6-month average effect size by 0.19 standard deviation units and the 18-month average effect size by 0.18 standard deviation units, both statistically significant at the 5% level of significance.

3.3 Effect of Program Completion

Not everyone assigned to the treatment group completed the program and received the certificate at the end of the program. In our sample, 56 percent of all women assigned to the treatment group were program completers, that is, completed the entire program and received a certificate at the end of the program.[14] On an average program completers

[14]The main reasons for non-completion include own sickness, sickness of other members in the family, child care options not being available, other family members were not happy or did not give permission, and the program being very time consuming.

(hereafter *TRAINED*) attended more than 70 percent of all classes in comparison to the program drop-outs who attended only 4 percent percent of all classes during the training period. In Figure 2 we present the average monthly attendance rate for program completers and drop-outs. Among program completers, average attendance is typically more than 70 percent.[15] Average monthly attendance among program drop-outs starts out at around 20 percent in the beginning of the program in August 2010 and declines to 10 percent by September 2010 and remains at an average of 4 percent during November 2010 – January 2011. The majority of the drop-outs therefore occurred at the beginning of the program.

Learning and skill accumulation is likely to be considerably higher for those women who completed the training. To the extent that skill accummulation in important, the program effects will be higher for women who completed the program. To capture the returns from completing the program, we estimate the following variant of equation (2) where the coefficient estimate on *TRAINED* and the interaction term $TRAINED \times YEAR$ respectively capture the 6-month and 18-month impact of the treatment on the treated.

$$Y_{it} = \delta_0 + \delta_1 Y_{i0} + \delta_2 TRAINED_i + \delta_3 YEAR_t + \delta_4 TRAINED_i \times YEAR_t + \sum_{j=1}^{K} \eta_j \mathbf{X}_{ij} + \epsilon_{it} \quad (3)$$

Here *TRAINED*, takes a value 1 if the women completed the program, that is, attended the program consistently, took the final exam, and received a certificate at the end of the program, δ_2 gives us the corresponding 6-month (i.e., over the period 2010 – 2011) TOT effect of the vocational training program. The coefficient estimate on the interaction term, δ_4 gives us the additional effect over the 2011 – 2012 period. The overall 18-month impact of the treatment on the treated is given by the composite term, $\delta_2 + \delta_4$. Since both *TRAINED* and $TRAINED \times YEAR$ are both endogenously determined, we use random assignment to the treatment, that is, being offered the training program along with its interaction with age and the year dummy as instruments. The first-stage regression results are reported in Appendix Tables A-4 and A-5 respectively. It is not surprising that the TOT estimates are systematically higher compared to the ITT estimates. The results presented in Panel A, Column 1 in Table 9 suggest that in the 6-month post-intervention period, the *TRAINED* experience a 9 percentage point increase in the likelihood of obtaining casual or full-time wage employment, a 7 percentage point increase in the likelihood of obtaining self-employment, a 11 percentage point increase in the likelihood of obtaining employment, work an additional 4.2 hours during the last week, earn an additional Rs 244 in monthly

[15]The only exception is November when it falls to 60 percent due to the popular religious festival of Diwali.

earnings, and are 15 percentage point more likely to own a sewing machine, relative to the control group. The 18-month impacts are presented in Column 2 of Table 9. Once again we find that the 6-month TOT effects persist over the longer period.

Note that the SATYA-Pratham program was not the only program available to the women. There were other private providers, government training schools, and other NGOs that offered similar courses in the city. An additional 9 percent of the women from the treatment group and 13 percent of women in the control group also completed a different course in stitching and tailoring.[16]

To examine how completion of *any* program affects outcomes, we estimate a version of equation (3), where the coefficient estimate on *TRAINED* and the composite term (*TRAINED* plus *TRAINED* $\times$ *YEAR*), respectively capture the 6-month, and 18-month impacts of the treatment on the treated for *any* program (including the SATYA-Pratham program). The corresponding first-stage regression results for *TRAINED* and *TRAINED* $\times$ *YEAR* for *any* program are reported in Appendix Tables A-6 and A-7 respectively. The 6-month and 18-month effects of *any* program completion are presented in Columns 4 and 5 of Table 9 respectively. They are very similar to the effects presented in Columns 1 and 2. This is not a surprising result as 80 percent of the women who completed the *any* program indeed completed the *SATYA-Pratham* program.

The sign and statistical significance of the *Treatment* indicator and the interaction term *Treatment* $\times$ *Year* reported in the first-stage regression results suggest that assignment to the program alleviates an important constraint that acts as a significant barrier to skill accumulation and this in turn affects the likelihood of program completion. For instance, we find that women assigned to the treatment group are almost 67 percent more likely to complete the *SATYA-Pratham* program and 69 percent more likely to complete the *any* program (including the *SATYA-Pratham* program). This negligible difference in the program completion rates between the *SATYA-Pratham* and *any* program suggest that a very small proportion of the sample women take-up courses offered by other providers.

4 Barriers to Program Completion

One of the main reasons for the low cost-effectiveness of labor market training programs (in both developed and developing countries) is low program completion rates.[17] Our data on

[16] The difference between the two groups is not statistically significant ($p - value = 0.23$).

[17] On average around only 60% of all program participants reach the finish line. For example the average program completion rate in the United States Job Training and Partnership Program (JTPA) is 58%. Similar, low rates of program completion are observed in other developed and developing countries as well:

program completion allows us to examine the barriers to program completion. We compute and present in Column 1 of Table 10 the marginal effects from a probit regression, where the dependent variable (*TRAINED*) takes the value of 1 if the woman completed the SATYA-Pratham program and received a certificate at the end of the program and 0 if she dropped out. The sample here is restricted to women who were assigned to receive the training, that is, women in the treatment group. Women who have completed secondary schooling are 25 percentage points more likely to complete the training program. Perhaps women who have completed secondary schooling are better able to internalize the benefits of training and hence complete the program. Local access is crucially important. Distance to the training center, captured by the time taken to walk to the training center, is a significant barrier to program completion and hence skill accumulation – a 10 minute increase in the time taken to walk to the training center is associated with a 14 percentage point reduction in the likelihood of program completion. This negative relationship between the time to walk to the center and the likelihood of program completion is corroborated by the lowess plots presented in Figure 3.[18]

In column 2 of Table 10 we present the marginal effects from a probit regression of *any* program completed. Here we cannot include distance to the center as an explanatory variable as we do not have any measure of the distance to the training center for the other programs. We include assignment to treatment (SATYA-Pratham program) status as an additional explanatory variable to examine if the treatment alleviated credit and access related constraints resulting in increased overall take-up. We find that women assigned to the treatment group are 55 percentage points more likely to participate and complete *any* program compared to women assigned to the control group. The other results are similar to those presented in Column 1. We also find that a lack of child care support in the household appears to have had a significant impact on program completion. Relative to unmarried women, married women with mother-in-law present in the household are 29 percentage points ($p - value = 0.06$) more likely to complete the program. The results also show that program completion rates are lower for unmarried women – this is possibly driven by restrictions on the movement of unmarried women because of social norms and/or safety concerns.

Germany (69%), Dominican Republic (60%), Uruguay (51%), and Peru (60%). See Kluve et al. (2007), Ibarraran and Rosas. (2009), Card et al. (2011).

[18]The time taken to walk to the training center is not self-reported. It is the time taken by an employee of Pratham to walk from each respondent's home to the training center she is assigned to. Therefore this measure does not suffer from self-reporting bias. Given the concentration of houses in the slums of North and South Shahdara, it was difficult to use a GPS device for measuring distance. The average time to walk to the assigned training center is approximately 12 minutes (around 13 minutes in North Shahdara and 10 minutes in South Shahdara).

5 Alternative Pathways?

Are the intent-to-treat effects observed here the result of skill accumulation or "something else"? For example, it is possible that labor market training programs increase labor market outcomes not only through skill accumulation but also increase participants' overall confidence level and competitiveness. Alternatively, it is plausible that women take-up the training program not to acquire skills but rather to acquire the certificate they would receive at the completion of the program to signal their ability in the labor market (Spence, 1973).

5.1 Behavioral Impacts

To examine if the training program resulted in changes in behavioral characteristics, we requested a randomly selected sample of the applicants to participate in an artefactual field experiment prior to randomization in 2010 and five months after completion of the intervention in July 2011. We call this the behavioral sample. Due to operational constraints, the artefactual field experiment could only be conducted in South Shahdara (see Dasgupta et al. (2014) for details on the artefactual field experiment). A total of 135 applicants participated in the artefactual field experiment in 2010. Despite all effort, we were unable to trace around 15 percent of the participants in 2011. There are no systematic differences in the attrition rates between women in the treatment group and the control group.[19] We have pre-and post-intervention data on behavioral characteristics for 117 women. However, given the small sample size and high attrition rates, the results from the artefactual field experiment need to be interpreted carefully.

The basic structure of each game is similar to the games used in previous studies (see for example: Gneezy et al. (2009)). The first was an *Investment Game* where each player was given the option of investing any part of an endowment in a hypothetical risky project that had a 50-percent chance of quadrupling the amount invested; alternatively the amount invested could be lost with a 50-percent probability. The individual could keep any amount he/she chose not to invest. The second game was the *competition game*, designed to investigate the competitiveness of the subjects. The subjects were required to participate in a real-effort task, which determined their payoffs in the experiment. Prior to the task each subject had to choose one of two possible methods of compensation: a *piece-rate* or a *competition-rate* compensation method. In the *piece-rate* compensation method, the woman's earnings depended solely on her own performance. In the *competition-rate* com-

[19]The artefactual field experiment was conducted only at the baseline and the midline (that is, only in 2010 and 2011). Hence, we are unable to compute the longer run intent-to-treat effects of the training program on behavior.

pensation method, her earnings depended on how she performed relative to a randomly chosen subject in the same session. The subjects also had to guess their performance in the game, by answering questions on the number of bags they expected to be able to fill, and their expected rank based on their performance in the task. See Dasgupta et al. (2014) for more details on the experiment. The behavioral outcome variables are presented in Panel C, Table 1.

Columns 1 and 2 in Panel A of Table 11 present the means for the behavioral outcome variables at the baseline (2010), separately for women assigned (ultimately) to the treatment (Column 1) and control (Column 2) group. Column 3 presents the corresponding difference in averages. Notice that there is no significant difference in baseline behavioral outcomes except, relative rank, that shows that women in the treatment group appear to be more self confident compared to the women in the control group. Columns 1 and 2 in Panel B of Table 11 presents the averages of the socioeconomic characteristics at the baseline for the women in the treatment and control group respectively. As the difference effects in Column 3 show, with the exception of marital status (more women in the treatment group were married), there is no difference baseline socioeconomic characteristics.

Columns 4 and 5 in Panel A of Table 11 present the means of the behavioral outcomes from the follow-up (2011) experiments, separately for women in the treatment and the control group. The difference in averages (Column 6) show that again there is no difference between women in the treatment and the control group at the follow-up except, self-assessment of the number of bags they expect to fill in the *Competition Game*. As in the baseline, post-intervention women in the treatment group continue to be more self confident in a relative sense compared to the women in the control group.

The panel dimension of the data on behavioral characteristics along with a randomized evaluation design implemented here allows us to measure the 6-month ITT effects of the program on behavioral outcomes. Columns 7 and 8 present the corresponding difference-in-difference estimates. The estimates presented in Column 8 control for baseline socioeconomic characteristics, while those in Column 7 do not. These difference-in-difference estimates show that *TRAINING* does not result in any change in the behavioral characteristics of these women. Consequently, we can rule out any program effects operating through a change in behavioral outcomes.

5.2 Certificate Effect

A second possible pathway through which the treatment affects outcome is via the signalling effect. Program completion involves receiving a certificate from SATYA stating

that the woman completed a course on stitching and tailoring. In most developing countries certificates have a large intrinsic value. So it is worth examining whether the program impacts are indeed the result of skill accumulation or is it because program completers are offered a certificate, i.e., is this simply a *certificate effect*?

To examine this we estimate the following equation:

$$\begin{aligned} Y_{it} =& \alpha_0 + \alpha_1 Y_{i0} + \alpha_2 ATTENDANCE_i + \alpha_3 YEAR_t \\ &+ \alpha_4 ATTENDANCE_i \times YEAR_t + \sum_{j=1}^{K} \eta_j \mathbf{X}_{ij} + \epsilon_{it} \end{aligned} \tag{4}$$

We estimate equation (4) using the $2010-2011-2012$ sample, where the coefficient estimate on *ATTENDANCE* and the composite term (*ATTENDANCE* plus *ATTENDANCE* $\times$ *YEAR*), respectively capture the 6-month, and 18-month effects of increased attendance. Since *ATTENDANCE* and *ATTENDANCE* $\times$ *YEAR* are both endogenous, once again, we use random assignment to the treatment along with its interaction with age and the year dummy to compute the corresponding IV estimates.

In Columns 1 and 2 of Table 12 we define *ATTENDANCE* to be the number of days the woman actually attends class. As an alternative, we define *ATTENDANCE* as the number of months the woman attends the program. These results are presented in Columns 3 and 4 of Table 12. The results are similar, irrespective of the definition of *ATTENDANCE*.

Comparing the implied impacts of increased intensity of training (*ATTENDANCE*) reported in Columns 1 and 2 in Table 12 and program completion (*TRAINED*) presented in Table 9 we see that while the signalling component is very small and the effect appears to be primarily driven by skill accumulation. The certificate effect is defined by the difference between the implied effect of increased intensity and that of program completion. Let us consider the case of real monthly earnings. The average program completer attended 70 percent of total classes. For the average program completer, in the 6-months since program completion the increase in real monthly earning is Rs 226.1 (3.23×70). Compare this to the increase of Rs 244 for the *TRAINED* (see Table 9). So approximately 92 percent of the effect for the program completers is explained by intensity of training, that is, attendance. The results therefore suggest that it is attendance and skill accumulation that is driving the results and not the *certificate effect* or *signalling*.

6 Discussion

Youth underemployment, especially among less educated populations perpetuates poverty. The situation is particularly dire for women in low income households. Despite the well-known fact that increasing the income level of women will have a strong positive impact on both current welfare and the welfare of the next generation, little is known about how to best help women in low-income households and communities in developing countries to acquire skills, find jobs, and increase earnings.

There are a number of different policy options. One would be to inject credit and reduce the credit constraints that appear to hamper the ability of women to take advantage of their entrepreneurial skills. Indeed the entire microfinance revolution was built around this model - provide microloans that will serve as working capital for setting up small businesses, leading to increased income over time. However, recent studies are increasingly skeptical of the success of such a model of development (see for example Karlan and Valdivia, 2011, Banerjee et al., 2013). Capital injection is a second option. Using a field experiment in Sri Lanka, de Mel et al. (2009) find that while the average returns to capital injection to microenterprises is very high (considerably higher than the average interest rates charged by microlenders), the effects are significantly gender biased. They argue that the capital injections generated large profit increases for male microenterprise owners, but not for female owners. Fafchamps et al. (2011) and Berge et al. (2011) find similar gender biased results. This finding has potentially serious implications for development policy because most microlending organizations target women. They argue that cash injections directed at women could be confiscated by their husbands and other members of their household leading to considerable inefficiencies.

One alternative tool for expanding the labor market opportunities for young women in these settings is vocational training or skills training, which could help women learn a trade and acquire the skills needed to take advantage of employment opportunities, and/or create successful small businesses. An important advantage of this kind of training is that it results in human capital accumulation that is specific to the person undertaking the training and cannot be confiscated by their spouse. Despite pro-training policies undertaken by policy makers in several developing countries, the economic benefits from participating in vocational training programs is relatively unknown. This paper thus fills a considerable gap in the literature.

The Indian case is particularly interesting. Lack of adequate skills is now being increasingly identified as one of the most significant barriers to India's growth. For example, the National Skills Development Corporation in India predicts that over the next five years,

more than 80 million new jobs will be created, but many of these positions will be difficult to fill, because of an inadequate skilled labor force (see, http://www.nsdcindia.org and Unni and Rani, 2004). Will vocational training, which is increasingly viewed as a way out of poverty, work?

To the best of our knowledge, this is the first paper to experimentally examine the returns to vocational training from India and Asia. We show that there are positive and statistically significant returns to obtaining such skills. In addition, we also examine the barriers to program take-up and completion. Further, we rule out two alternative pathways that can explain these results.

The program that we evaluate is also cost-effective. The NGO's total cost of the underlying vocational training program amounts to Rs 1910 per person[20], including both fixed (for example purchasing sewing machines) and variable (for example teacher salary, rent, teaching materials) costs. In addition to this, the average program participant also incurs personal time costs, which is proxied by the 2010 average monthly earnings of the employed women in the control group. The total time costs amount to Rs 3675 (Rs 612.50 $\times$ 6 months). As a result, the final annual per person cost of the program sums to Rs 5585 (Rs 3675 + Rs 1910). The short-run ITT effects of the program reported in Table 8 indicate that the program increases annual earnings by Rs 1656 (Rs 138 $\times$ 12 months). Under the assumption of no appreciation or depreciation in annual earnings and a 5 percent discount rate, the total cost of the program can be recovered with less than four years of continued employment.[21]

Although this paper focuses on the effects of obtaining subsidized training (in a specific program) for a specific population, we believe that this population is of considerable interest and insights gained from this population can be applied elsewhere. First, the young population in our sample is reflective of the population structure in the majority of developing countries. Second, more than a third of the urban population in developing countries reside in slums and improving the condition of these slum dwellers by providing them marketable skills is of crucial importance to governments and policy makers. Third, in developing countries around the world, women typically have low rates of skill accumulation, and labor force participation. Increasing skills and labor force participation rates of women therefore, can have significant effects on aggregate productivity in these countries (see UNESCO, 2012, Census, 2011, for more on these issues). Lessons from this

[20] 1USD = Rs 50 using the exchange rate that was prevailing at the time of the intervention.

[21] Even if the discount rate was increased to $15 - 25$ percent, the total cost of the program could be recovered with less than five-six years of continued employment. If the discount rate was however excessively high ($\sim$ 100 percent), the program benefits will not outweigh the costs.

study are therefore applicable to many other countries facing similar challenges involving demographics, growth, skill accumulation, and development.

There are two important policy implications that emerge from the findings in this paper. First, investing in vocational training programs can result in substantial economic gains for women in low-income households in developing countries. Second, constraints relating to accessibility, credit/resources, and availability of child care support in the household are crucial will severely constrain women from participating and completing training programs of any kind, not just vocational training in stitching and tailoring. Findings from this paper therefore speak to not only policy makers, NGOs, and researchers in India, but has implications for influencing policy choices in a number of other low-and-middle income countries in Asia and sub-Saharan Africa, which experience similar challenges in attaining economic growth, development, and gender equality.

References

Ashraf, N., J. Berry, and J. M. Shapiro (2010). Can higher prices stimulate product use? evidence from a field experiment in zambia. American Economic Review 100(5), 2383 – 2413.

Attanasio, O., A. Kugler, and C. Meghir (2011). Subsidizing vocational training for disadvantaged youth in colombia: Evidence from a randomized trial. American Economic Journal: Applied Economics 3, 188–220.

Bandiera, O., N. Buehren, R. Burgess, M. Goldstein, S. Gulesci, I. Rasul, and M. Sulaiman (2012). Empowering adolescent girls: Evidence from a randomized control trial in uganda. Technical report, London School of Economics.

Banerjee, A., E. Duflo, R. Glennerster, and C. Kinnan (2013). The miracle of microfinance: Evidence from a randomized evaluation. Technical report, MIT.

Becketti, S., W. Gould, L. Lillard, and F. Welch (1988). The panel study of income dynamics after fourteen years: An evaluation. Journal of Labor Economics 6(4), 472 – 492.

Berge, L. V. O., K. Bjorvatn, and B. Tungodden (2011). Human and financial capital for microenterprise development: Evidence from a field and lab experiment. Working paper, Chr. Michelsen Institute (CMI).

Betcherman, G., K. Olivas, and A. Dar (2004). Impacts of active labor market programs: New evidence from evaluations with particular attention to developing and transition countries. Social protection discussion paper no 0402, World Bank.

Bettinger, E. P. and R. Slonim (2005). The effects of educational vouchers on student over-confidence. In G. Harrisson (Ed.), Field Experiments in Economics. Elsevier.

Blattman, C., N. Fiala, and S. Martinez (2014). Generating skilled self-employment in developing countries: Experimental evidence from uganda. Quarterly Journal of Economics Forthcoming.

Blom, A. and H. Saeki (2011). Employability and skill set of newly graduated engineers in india. Technical report, World Bank Policy Research Working Paper 5640.

Bommier, A. and S. Lambert (2000). Education demand and age at school enrollment in tanzania. The Journal of Human Resources 35(1), 177 – 203.

Card, D., P. Ibarraran, F. Regalia, D. Rosas, and Y. Soares (2011). The labor market impacts of youth training in the dominican republic: Evidence from a randomized evaluation. Journal of Labor Economics 29(2), 267–300.

Census (2011). Status of literacy. Technical report, Registrar General of India.

Cho, Y., D. Kalomba, A. M. Mobarak, and V. Orozco (2013). Gender difference in the effects of vocational training: Constraints on women and drop-out behavior. Technical report, Yale University.

Clingingsmith, D., A. I. Khwaja, and M. R. Kremer (2009). Estimating the impact of the hajj: Religion and tolerance in islam's global gathering. Quarterly Journal of Economics 124(3), 1133 – 1170.

Dasgupta, U., L. Gangadharan, P. Maitra, S. Mani, and S. Subramanian (2014). Choosing to be trained: Do behavioral traits matter? Technical report, Mimeo Monash University.

de Mel, S., D. McKenzie, and C. Woodruff (2009). Are women more credit constrained? experimental evidence on gender and microenterprise returns. American Economic Journal: Applied Economics 1(3), 1–32.

Fafchamps, M., D. McKenzie, S. Quinn, and C. Woodruff (2011). Female microenterprises and the fly-paper effect: Evidence from a randomized experiment in ghana. Technical report, CSAE, University of Oxford.

FICCI (2011). Ficci survey on labour / skill shortage for industry. Technical report, FICCI.

Gneezy, U., K. L. Leonard, and J. List (2009). Gender differences in competition: Evidence from a matrilineal and patriarchal society. Econometrica 77(5), 1637 – 1664.

Gregg, P. (2001). The impact of youth unemployment on adult employment in the ncds. Economic Journal 111(475), 623 – 653.

Groh, M., N. Krishnan, D. McKenzie, and T. Vishwanath (2012). Soft skills or hard cash? the impact of training and wage subsidy programs on female youth employment in jordan. Technical report, World Bank Policy Research Working Paper 6141.

Hirshleifer, S., D. McKenzie, R. Almeida, and C. Ridao-Cano (2014). The impact of vocational training for the unemployed: Experimental evidence from turkey. Technical report, World Bank.

Holmes, J. (2003). Measuring the determinants of school completion in pakistan: analysis of censoring and selection bias. Economics of Education Review 22, 249 – 264.

Ibarraran, P. and D. Rosas. (2009). Evaluating the impact of job training programmes in latin america: Evidence from idb funded operations. The Journal of Development Effectiveness 1(2), 195 – 216.

Karlan, D. and M. Valdivia (2011). Teaching entrepreneurship: Impact of business training on microfinance clients and institutions. Review of Economics and Statistics 93(2), 510 – 527.

Karlan, D. and J. Zinman (2009). Expanding credit access: Using randomized supply decisions to estimate the impacts. Review of Financial Studies 23(1), 433 – 464.

King, E. and L. A. Lillard. (1987). Education policy and schooling attainment in malaysia and the philippines. Economics of Education Review 6(2), 167 – 181.

Kling, J. R., J. B. Liebman, and L. F. Katz (2007). Experimental analysis of neighborhood effects. Econometrica 75, 83 – 119.

Kluve, J., D. Card, M. Fertig, M. Gora, L. Jacobi, P. Jensen, R. Leetmaa, L. Nima, E. Patacchini, S. Schaffner, C. M. Schmidt, B. van der Klaauw, and A. Weber (2007). Active labor market policies in Europe: Performance and perspectives. Berlin: Springer.

Lavy, V. (1996). School supply constraints and children's educational outcomes in rural ghana. Journal of Development Economics 51(2), 291 – 314.

Macours, K., P. Premand, and R. Vakis (2012). Transfers, diversification and household risk strategies: Experimental evidence with lessons for climate change adaptation. Technical report, Mimeo Paris School of Economics.

McKenzie, D. and C. Woodruff (2012). What are we learning from business training and entrepreneurship evaluations around the developing world? Technical report, World Bank Policy Research Working Paper 6202.

Narendranathan, W. and P. Elias (1993). Influences of past history on the incidence of youth unemployment: Empirical findings for the uk. Oxford Bulletin of Economics and Statistics 55(2), 161 – 185.

Spence, M. (1973). Job market signaling. Quarterly Journal of Economics 87(3), 355 – 374.

Tansel, A. (2002). Determinants of school attainment of boys and girls in turkey: individual, household and community factors. Economics of Education Review 21(5), 455 – 470.

UNESCO (2012). Youth and skills: Putting education to work. Technical report, UNESCO Education for all Global Monitoring Report.

Unni, J. and U. Rani (2004). Technical change and workforce in india: Skill biased growth? Indian Journal of Labor Economics 47(4).

WDR (2013). World development report: Jobs. Technical report, World Bank.

Figure 1: The Advertisement Campaign of the Program

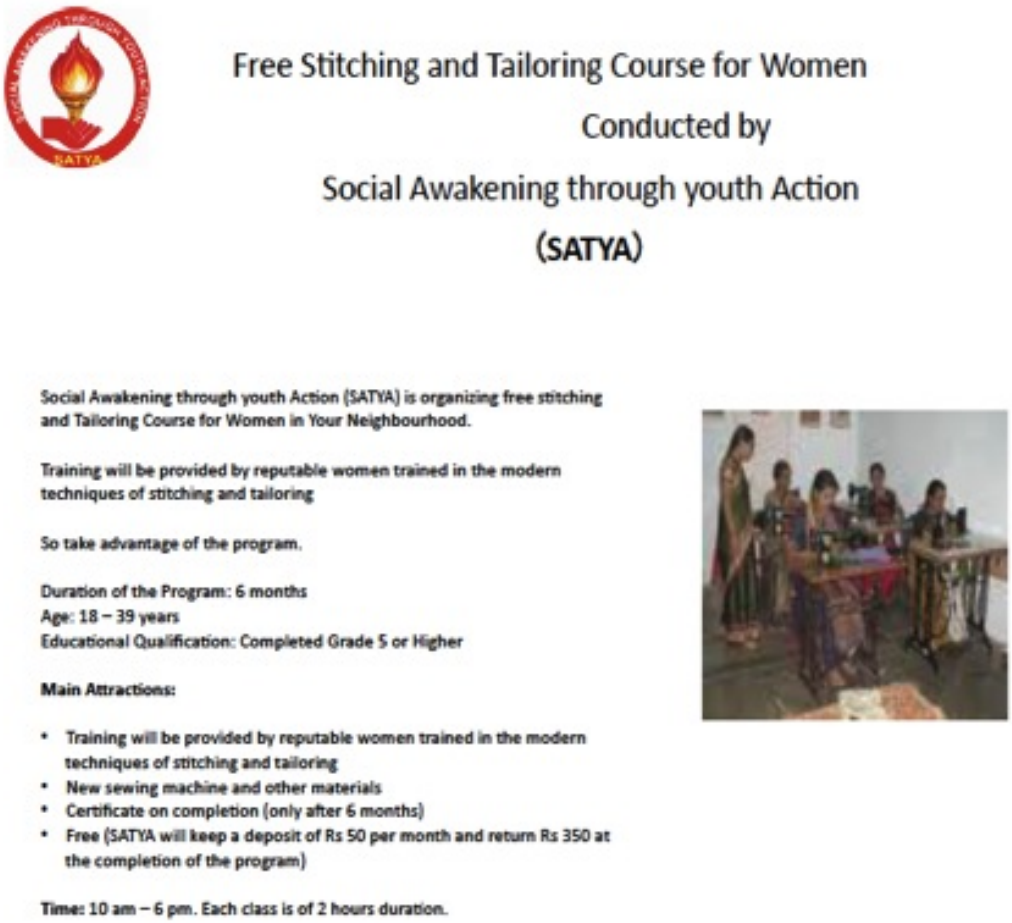

Figure 2: Average Monthly Attendance

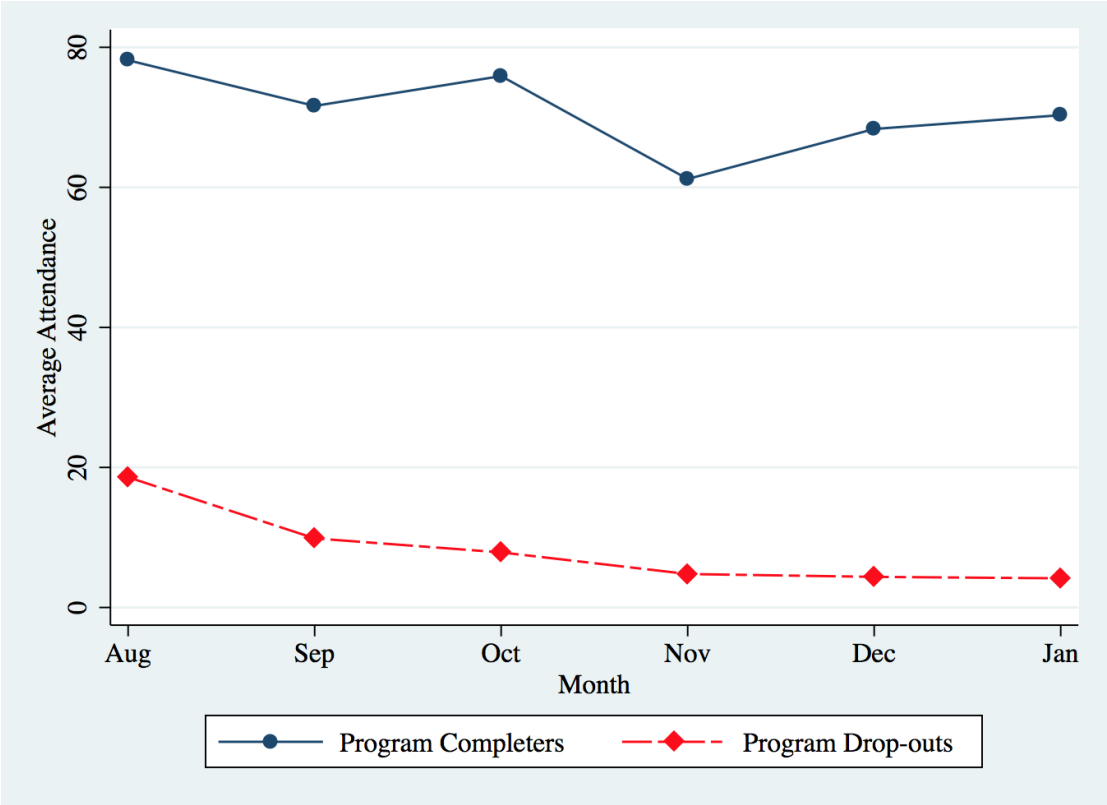

Figure 3: Walking time to training center and Program Completion

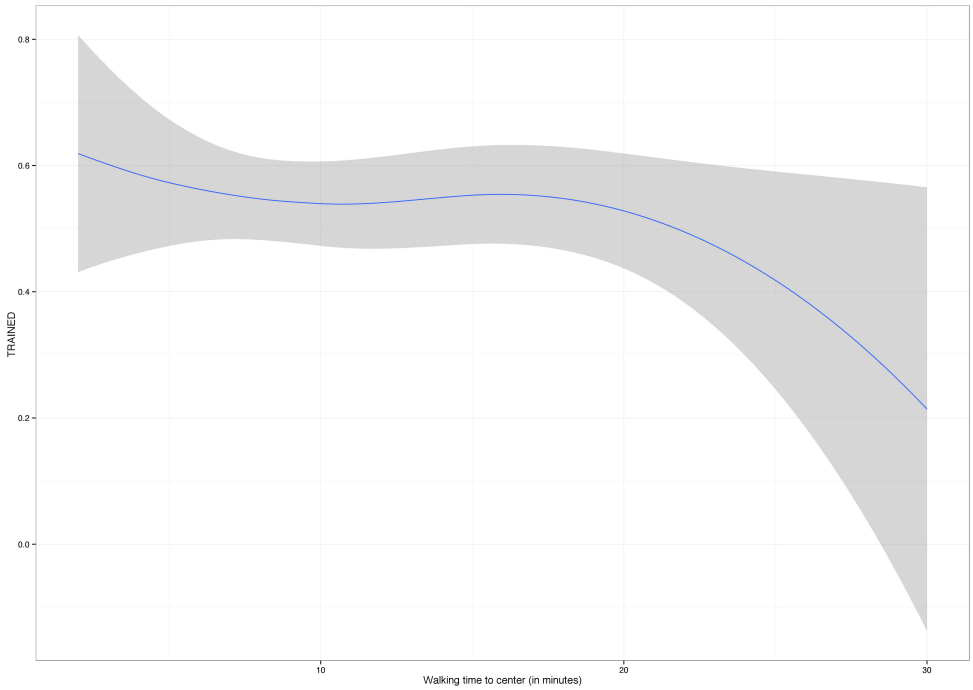

Table 1: Outcome Variables

Panel A: Labor Market Outcomes	
Casual/full-time employment:	= 1 if the respondent is employed for casual wage or is employed full-time, 0 otherwise
Self-employment:	= 1 if the respondent is self-employed, 0 otherwise
Employed:	= 1 if the respondent is employed (casual, full-time, and or self), 0 otherwise
Hours worked:	Number of hours worked during the last week, where hours worked is a continuous variable
Monthly earnings (in real terms)†:	Total monthly earnings from casual, full-time, and or self-employment during the last month
Panel B: Entrepreneurship, Empowerment, and Life Satisfaction	
Own sewing machine:	= 1 if the respondent owns a sewing machine at home, 0 otherwise
ROSCA membership‡:	= 1 if the respondent is a member of a Rotating Savings and Credit Association (ROSCA), 0 otherwise
Happy at home:	= 1 if the respondent is very satisfied or moderately satisfied at home, 0 otherwise
Panel C: Behavioral Characteristics	
Proportion allocated to the risky option	Proportion allocated to the risky option in the investment game
Competitive wage scheme	= 1 if the woman chooses the competition wage scheme in the competition game, 0 otherwise
Self assessment	Number of bags the woman expects to fill in the competition game
Relative rank	Estimate about her relative standing (rank) in the competition game

Notes: †: Nominal monthly earnings is top coded at Rs 10,000. The monthly earnings for 2011 and 2012 are deflated using the monthly Consumer Price Index for New Delhi made available by the Directorate of Economics and Statistics, Delhi Government. ‡: ROSCA membership may be viewed as both a measure of empowerment as well as a source for hiding personal earnings.

Table 2: Survey Response Rates

		2010 (Baseline)	2011 (Midline)	2012 (Endline)
		(1)	(2)	(3)
(1)	Number of applicants	658	658	658
(2)	Number surveyed	594	504	491
(3)	Tracking rate in % [3 = 2/1]	90.27	76.6	74.6
(4)	Tracking rate in % Treatment	92.53	78.05	75.8
(5)	Tracking rate in % Control	85.64	73.61	72.22
(6)	Difference [6 = 4 − 5]	6.89***	4.44	3.58
		(2.45)	(3.51)	(3.61)
(7)	Number sought	658	594	594
(8)	% Surveyed [8 = 2/7]	90.27	84.85	82.65
(9)	% Surveyed Treatment	92.53	84.35	81.9
(10)	% Surveyed Control	85.64	85.95	84.3
(11)	Difference [11 = 9 − 10]	6.89***	-1.6	-2.4
		(2.45)	(3.18)	(3.35)

Notes: Standard errors reported in parentheses. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$.

Table 3: Sample Balance on Administrative Data

	Full Sample (1)	Treatment (2)	Control (3)	Difference (4 = 2 – 3)
Panel A: Full Sample: 658 Applicants				
Age	22.37	22.37	22.36	0.01
	[5.66]	[5.65]	[5.67]	(0.47)
Completed grades of schooling	8.80	8.88	8.65	0.23
	[2.54]	[2.56]	[2.49]	(0.21)
Sample Size	658	442	216	
Panel B: Surveyed in 2010: 594 Applicants				
Age	22.36	22.41	22.26	0.15
	[5.69]	[5.71]	[5.65]	(0.50)
Completed grades of schooling	8.86	8.92	8.73	0.19
	[2.54]	[2.54]	[2.53]	(0.22)
Sample Size	594	409	185	
Panel C: Surveyed in 2010 and 2011: 504 Applicants				
Age	22.24	22.31	22.08	0.23
	[5.60]	[5.62]	[5.58]	(0.53)
Completed grades of schooling	8.88	8.95	8.73	0.22
	[2.50]	[2.50]	[2.51]	(0.24)
Sample Size	504	345	159	
Panel D: Surveyed in 2010, 2011, and 2012: 439 Applicants				
Age	22.33	22.32	22.34	-0.02
	[5.78]	[5.79]	[5.78]	(0.59)
Completed grades of schooling	8.90	8.95	8.78	0.17
	[2.51]	[2.52]	[2.51]	(0.25)
Sample Size	439	298	141	
Panel E: Surveyed in 2010 and 2012: 491 Applicants				
Age	22.30	22.26	22.36	-0.10
	[5.72]	[5.72]	[5.74]	(0.55)
Completed grades of schooling	8.89	8.90	8.86	0.04
	[2.53]	[2.54]	[2.51]	(0.24)
Sample Size	491	335	156	

Notes: Standard deviation reported in brackets and standard errors in parentheses. $^{***}p < 0.01$,** $p < 0.05$,* $p < 0.1$.

Table 4: Baseline Characteristics

	Full Sample (1)	Treatment (2)	Control (3)	Difference (4 = 2-3)
Panel A: Labor Market Outcomes				
Casual/full-time	0.034	0.037	0.028	0.009
employment	[0.18]	[0.19]	[0.16]	(0.018)
Self-employment	0.027	0.027	0.028	-0.001
	[0.16]	[0.16]	[0.16]	(0.016)
Employed	0.052	0.053	0.05	0.003
	[0.22]	[0.22]	[0.21]	(0.02)
Hours worked	1.00	1.08	0.82	0.26
	[5.70]	[5.94]	[5.16]	(0.58)
Hours worked [if employed]	19.00	20.06	16.57	3.49
	[17.00]	[17.12]	[17.80]	(7.85)
Monthly earnings	45.68	52.91	30.41	22.50
	[297.70]	[337.67]	[187.17]	(30.45)
Monthly earnings [if employed]	872.00	985.53	612.50	373.03
	[1005.57]	[1130.16]	[633.65]	(459.25)
Panel B: Entrepreneurship, Empowerment, and Life Satisfaction				
Own sewing machine	0.36	0.32	0.44	-0.12**
	[0.48]	[0.47]	[0.50]	(0.05)
ROSCA membership	0.11	0.12	0.11	0.01
	[0.32]	[0.32]	[0.31]	(0.03)
Happy at home	0.87	0.86	0.89	-0.03
	[0.33]	[0.35]	[0.31]	(0.034)
Panel C: Socioeconomic Characteristics				
Age	22.33	22.34	22.33	0.01
	[5.82]	[5.81]	[5.87]	(0.60)
Completed secondary schooling	0.45	0.45	0.44	0.01
	[0.50]	[0.50]	[0.50]	(0.05)
Experienced in stitching and tailoring	0.22	0.22	0.23	-0.01
	[0.42]	[0.41]	[0.42]	(0.04)
Married	0.33	0.33	0.32	0.01
	[0.47]	[0.47]	[0.47]	(0.05)
SC	0.52	0.53	0.51	0.02
	[0.50]	[0.50]	[0.50]	(0.05)
Hindu	0.51	0.50	0.52	-0.02
	[0.50]	[0.50]	[0.50]	(0.05)
Dependent ratio	0.25	0.26	0.24	0.02
	[0.48]	[0.49]	[0.45]	(0.05)
Test of Joint Significance†				1.36
Test of Joint Significance††				1.51
Sample Size	439	298	141	

Notes: Standard deviation reported in brackets and standard errors in parentheses. $^{***}p < 0.01$,** $p < 0.05$,* $p < 0.1$. Sample uses women surveyed in all three rounds (2010 – 2011 – 2012 sample). † : excluding ownership of sewing machine. †† : including ownership of sewing machine.

Table 5: Attrition using Administrative Data

	Treatment			Control			
	Non-Attritors (1)	Attritors (2)	Difference (3 = 1 – 2)	Non-Attritors (4)	Attritors (5)	Difference (6 = 4 – 5)	Difference in Difference (7 = 3 – 6)
Panel A: Surveyed in 2010: 594 Applicants							
Age	22.41	21.90	0.51	22.26	22.93	-0.67	1.18
	[5.71]	[4.86]	(1.02)	[5.64]	[5.91]	(1.10)	(1.43)
Completed grades	8.92	8.33	0.59	8.73	8.16	0.57	0.02
of schooling	[2.54]	[2.75]	(0.46)	[2.53]	[2.19]	(0.48)	(0.65)
Sample Size	409	33		185	31		
Panel B: Surveyed in 2010 and 2011: 504 Applicants							
Age	22.31	22.58	-0.27	22.08	23.12	-1.04	0.77
	[5.62]	[5.78]	(0.65)	[5.57]	[5.93]	(0.87)	(1.11)
Completed grades	8.95	8.63	0.32	8.73	8.40	0.33	-0.01
of schooling	[2.50]	[2.77]	(0.29)	[2.51]	[2.44]	(0.38)	(0.49)
Sample Size	345	97		159	57		
Panel C: Surveyed in 2010, 2011, and 2012: 439 Applicants							
Age	22.32	22.48	-0.16	22.34	22.38	-0.04	-0.12
	[5.78]	[5.38]	(0.57)	[5.78]	[5.51]	(0.81)	(0.97)
Completed grades	8.95	8.72	0.23	8.78	8.40	0.38	-0.15
of schooling	[2.52]	[2.66]	(0.26)	[2.51]	[2.44]	(0.35)	(0.44)
Sample Size	298	144		141	75		
Panel C: Surveyed in 2010 and 2012: 491 Applicants							
Age	22.26	22.71	-0.45	22.36	22.35	0.01	-0.45
	[5.78]	[5.45]	(0.63)	[5.74]	[5.54]	(0.86)	(1.04)
Completed grades	8.90	8.80	0.10	8.86	8.08	0.78**	-0.68
of schooling	[2.54]	[2.65]	(0.72)	[2.51]	[2.36]	(0.38)	(0.46)
Sample Size	335	107		156	60		

Notes: Standard deviation reported in square brackets and standard errors reported in parentheses. $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

Table 6: Likelihood of Attrition: Marginal Effects from a Probit Regression

	Attrite (1)	*Attrite_Temp* (2)
Treatment	0.035	0.011
	(0.038)	(0.028)
Age $\times$ (10^{-2})	0.007	0.002
	(0.50)	(0.004)
Completed secondary schooling	-0.008	0.003
	(0.037)	(0.027)
Married	0.031	-0.035
	(0.065)	(0.047)
Hindu	-0.082	0.012
	(0.055)	(0.038)
SC	-0.027	0.001
	(0.036)	(0.026)
Experienced in stitching and tailoring	-0.066	-0.042
	(0.047)	(0.032)
Dependency ratio	0.031	0.021
	(0.041)	(0.032)
Sample size	594	491

Notes: Dependent variable in Column 1 (*Attrite*) takes a value 0 if the woman is included in all 3 surveys, 1 otherwise. Dependent Variable in Column 2 (*Attite_Temp*) takes the value of 1 if the woman is not surveyed in 2011 (midline) but is surveyed in 2012 (endline). Regressions include region of residence and center dummies. Robust standard errors in parentheses. $^{***}p < 0.01$,** $p < 0.05$,* $p < 0.1$.

Table 7: Differential Attrition?

	Casual/full-time employment (1)	Self employment (2)	Employed (3)	Hours worked (4)	Monthly earnings (5)	Own sewing machine (6)	ROSCA membership (7)	Happy at home (8)
Treatment	0.011	0.000	0.008	0.308	24.617	-0.112**	0.013	-0.037
	(0.018)	(0.017)	(0.023)	(0.573)	(25.913)	(0.046)	(0.032)	(0.033)
Attrite	-0.032	0.051	-0.041	-4.075	-253.076	-0.586**	0.202	-0.216
	(0.147)	(0.074)	(0.160)	(5.171)	(347.638)	(0.284)	(0.192)	(0.221)
Attrite × *Treatment*	-0.013	0.013	-0.002	0.039	-1.060	-0.034	-0.030	0.023
	(0.033)	(0.020)	(0.037)	(0.873)	(51.005)	(0.094)	(0.066)	(0.066)
Experienced in stitching	0.036	0.043	0.048	0.720	37.575	0.473***	0.166***	0.001
and tailoring	(0.028)	(0.026)	(0.035)	(0.741)	(34.894)	(0.057)	(0.047)	(0.044)
Age	0.004**	0.004**	0.006**	0.111*	5.463*	-0.003	0.003	-0.000
	(0.002)	(0.002)	(0.003)	(0.061)	(2.974)	(0.006)	(0.004)	(0.004)
Completed secondary	0.025	-0.013	0.026	0.774	45.971	0.023	0.076**	0.094***
schooling	(0.017)	(0.014)	(0.021)	(0.551)	(28.781)	(0.043)	(0.031)	(0.033)
Married	-0.034	-0.070***	-0.078**	-0.879	-61.348	0.070	-0.036	0.053
	(0.026)	(0.026)	(0.033)	(0.825)	(47.542)	(0.080)	(0.043)	(0.056)
Hindu	0.034	0.012	0.043	1.381	109.616	0.030	0.044	-0.120**
	(0.027)	(0.019)	(0.033)	(1.223)	(90.696)	(0.063)	(0.045)	(0.051)
SC	-0.010	0.013	-0.003	-0.727	-43.275	-0.042	0.045	0.017
	(0.018)	(0.015)	(0.022)	(0.589)	(31.415)	(0.044)	(0.030)	(0.033)
Dependency ratio	0.019	0.017	0.024	0.527	48.001	-0.032	-0.025	-0.031
	(0.025)	(0.022)	(0.026)	(0.785)	(44.701)	(0.053)	(0.029)	(0.040)
Attrite × Age	-0.002	-0.007**	-0.005	0.058	4.009	0.014	-0.010	-0.003
	(0.005)	(0.003)	(0.006)	(0.180)	(9.539)	(0.012)	(0.007)	(0.008)
Attrite × Completed secondary	0.021	-0.010	0.010	1.138	112.002	0.091	-0.019	0.022
schooling	(0.036)	(0.021)	(0.040)	(1.309)	(116.067)	(0.081)	(0.063)	(0.061)
Attrite × Married	0.083	0.077**	0.113	1.973	224.324	-0.117	0.069	-0.036
	(0.070)	(0.036)	(0.074)	(2.235)	(180.988)	(0.149)	(0.099)	(0.087)
Attrite × Hindu	0.022	0.048	0.070	-0.072	-86.839	0.039	0.041	0.052
	(0.064)	(0.058)	(0.085)	(2.054)	(116.922)	(0.142)	(0.098)	(0.104)
Attrite × SC	0.019	-0.015	0.022	1.397	144.721	0.156*	0.029	0.044
	(0.034)	(0.023)	(0.038)	(1.244)	(109.729)	(0.085)	(0.060)	(0.066)
Attrite × experienced	0.029	-0.015	0.012	1.788	259.159	0.074	0.039	0.186***
in stitching and tailoring	(0.076)	(0.042)	(0.080)	(3.080)	(300.152)	(0.109)	(0.108)	(0.068)
Attrite × dependency ratio	-0.055	-0.007	-0.063	-2.082	-195.672	-0.127	0.034	0.107*
	(0.040)	(0.026)	(0.042)	(1.427)	(125.910)	(0.103)	(0.054)	(0.061)
Constant	-0.108*	-0.068	-0.146**	-2.878	-200.892	0.318**	-0.126	0.959***
	(0.060)	(0.046)	(0.071)	(2.019)	(130.621)	(0.143)	(0.095)	(0.097)
F-test of joint significance	0.46	1.00	0.68	0.51	0.39	1.25	0.73	0.62
ms ($p-value$)	(0.94)	(0.44)	(0.77)	(0.90)	(0.97)	(0.24)	(0.72)	(0.83)
Sample size	594	594	594	594	594	594	594	594

Notes: Robust standard errors in parentheses. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$. F-test on the joint significance of the attrition dummy and all its interaction terms are appended at the end of the table. Regressions also include region of residence and center dummies and their interactions with *Attrite*.

Table 8: ITT effects of *TRAINING*

	2010 – 2011 – 2012 sample				2010 – 2011 sample		2010 – 2012 sample	
	6-month Effect (1)	Mean Control (2011) (2)	18-month Effect (3)	Mean Control (2012) (4)	6-month Effect (5)	Mean Control (2011) (6)	18-month Effect (7)	Mean Control (2012) (8)
Panel A: Labor Market Outcomes								
Casual/full-time employment	0.053**	0.05	0.064**	0.08	0.043*	0.05	0.072**	0.076
	(0.02)		(0.03)		(0.022)		(0.028)	
Self-employment	0.039**	0.014	0.039**	0.014	0.045***	0.012	0.041***	0.013
	(0.017)		(0.016)		(0.015)		(0.016)	
Employed	0.06**	0.063	0.081**	0.099	0.057**	0.06	0.09***	0.096
	(0.03)		(0.03)		(0.02)		(0.03)	
Hours worked	2.44***	1.12	3.41***	3.30	2.06***	1.17	3.56***	3.25
	(0.80)		(1.31)		(0.74)		(1.25)	
Monthly earnings	138.19**	91.23	199.05**	207.90	137.41**	88.26	173.50*	240.85
	(69.00)		(95.74)		(62.10)		(101.35)	
Panel B: Entrepreneurship, Empowerment, and Life Satisfaction								
Own sewing machine	0.08	0.53	0.13***	0.59	0.07	0.54	0.12***	0.59
	(0.05)		(0.05)		(0.046)		(0.04)	
ROSCA membership	-0.0008	0.042	0.0002	0.085	0.010	0.037	0.003	0.076
	(0.021)		(0.03)		(0.019)		(0.025)	
Happy at home	-0.035	0.93	-0.025	0.94	-0.019	0.93	-0.027	0.93
	(0.026)		(0.025)		(0.025)		(0.025)	

Table 8 (continued): ITT effects of *TRAINING*

	2010 – 2011 – 2012 sample				2010 – 2011 sample		2010 – 2012 sample	
	6-month Effect (1)	Mean Control (2011) (2)	18-month Effect (3)	Mean Control (2012) (4)	6-month Effect (5)	Mean Control (2011) (6)	18-month Effect (7)	Mean Control (2012) (8)
Panel C: Index Measures								
All labor index	0.33***		0.27**		0.296***		0.279***	
	(0.12)		(0.11)		(0.11)		(0.10)	
Self index	0.26***		0.30***		0.271***		0.32***	
	(0.09)		(0.09)		(0.085)		(0.08)	
All other outcome index	-0.071		-0.052		-0.012		-0.03	
	(0.073)		(0.07)		(0.069)		(0.068)	
Panel D: Average Effect Size								
Average effect size	0.19***		0.18***		0.21***		0.19***	
	(0.07)		(0.06)		(0.06)		(0.06)	
Sample size			878			504		491

Notes: Regressions control for a full set of pre-intervention socioeconomic characteristics (age, completed secondary schooling, married, experienced in stitching and tailoring, hindu, SC, dependency ratio), lagged outcome variable, region of residence, and center dummies. Sample in Columns 1 – 4 includes all women who have been surveyed in all 3 rounds (2010 – 2011 – 2012 sample). Sample in Columns 5 and 6 include those in the 2010 – 2011 sample, and the sample in Columns 7 and 8 include those in the 2010 – 2012 sample. Robust standard errors clustered at the individual level in parentheses. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 9: Effect of Program Completion

	SATYA-Pratham Program			Any Program		
	6-month Effect	18-month Effect	Hansen J-statistic	6-month Effect	18-month Effect	Hansen J-statistic
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Labor Market Outcomes						
Casual/full-time employment	0.093**	0.114**	0.03	0.10**	0.121**	0.059
	(0.043)	(0.054)	[0.86]	(0.04)	(0.06)	[0.81]
Self-employment	0.068**	0.068**	1.80	0.072**	0.072**	1.90
	(0.03)	(0.03)	[0.18]	(0.03)	(0.03)	[0.16]
Employed	0.107**	0.145**	0.006	0.11**	0.15**	0.024
	(0.048)	(0.059)	[0.94]	(0.05)	(0.063)	[0.88]
Hours worked	4.22***	5.95***	2.81*	4.38***	6.24**	2.88*
	(1.43)	(2.31)	[0.10]	(1.52)	(2.48)	[0.09]
Monthly earnings	244.02**	352.62**	0.31	372.21**	452.07**	0.39
	(120.95)	(168.84)	[0.58]	(127.14)	(180.42)	[0.53]
Panel B: Entrepreneurship, Empowerment, and Life Satisfaction						
Own sewing machine	0.15*	0.233***	1.01	0.16*	0.25***	0.91
	(0.087)	(0.087)	[0.31]	(0.09)	(0.09)	[0.34]
ROSCA membership	-0.0009	0.0009	0.07	-0.0008	0.0012	0.07
	(0.037)	(0.05)	[0.80]	(0.039)	(0.05)	[0.78]
Happy at home	-0.06	-0.04	0.76	-0.14	-0.17	0.80
	(0.04)	(0.04)	[0.38]	(0.130)	(0.127)	[0.36]
Cragg-Donald F statistic†		117.51***			90.20***	
Sample size		878			878	

Notes: Regressions control for a full set of pre-intervention socioeconomic characteristics (age, completed secondary schooling, married, experienced in stitching and tailoring, hindu, SC, dependency ratio), lagged outcome variable, region of residence, and center dummies. Excluded instruments include *Treatment*, *Treatment* $\times$ *Age* and *Treatment* $\times$ *Year*. Sample includes all women who have been surveyed in all 3 rounds. Robust standard errors clustered at the individual level in parentheses. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$. The Cragg-Donald F statistic is the same across the different outcome variables of interest as the only source of difference between the different first-stage regressions comes from the difference in the lagged outcome variable in each first-stage equation, which has little explanatory power.

Table 10: Determinants of Program Completion

	Completed Program (SATYA/Pratham) *TRAINED* (1)	Completed Program (Any) *TRAINED_ANY* (2)
Age	-0.002	-0.003
	(0.008)	(0.007)
Completed secondary schooling	0.266***	0.165***
	(0.059)	(0.054)
Married	0.011	0.015
	(0.110)	(0.102)
Married × Mother-in-law Present	0.167	0.266*
	(0.164)	(0.134)
Hindu	-0.124	-0.063
	(0.094)	(0.082)
SC	-0.014	-0.031
	(0.061)	(0.053)
Dependency ratio	0.024	-0.008
	(0.072)	(0.065)
Experienced in stitching and tailoring	0.138*	0.145**
	(0.076)	(0.072)
Distance to training center	-0.014**	
	(0.006)	
Treatment		0.547***
		(0.040)
Joint Test:		
Married + Married × Mother-in-law Present	0.18	0.29*
	(0.19)	(0.16)
Sample size	298	439

Notes: Dependent Variable in Column 1 is completing SATYA-Pratham Program *TRAINED*. Sample in Column 1 includes only women assigned to the treatment group. Dependent Variable in Column 2 is completing any program in stitching and tailoring in the last year (self-reported), *TRAINED_ANY* . Sample in Column 2 includes all women in the final estimation sample. Marginal effects from probit regression are presented. Regressions include region of residence dummy. Center dummies included only in Column 1 as this information is not available for women in the control group. Robust standard errors in parentheses. $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

Table 11: Behavioral Outcomes

	Baseline (2010)			Follow-up (2011)			Difference-in-Difference Additional Controls	
	Treatment (1)	Control (2)	Difference (3 = 1 − 2)	Treatment (4)	Control (5)	Difference (6 = 4 − 5)	No (7)	Yes (8)
Panel A: Behavioral Outcomes								
Proportion allocated to the	53.08	50.77	2.31	48.10	54.56	-6.46	-8.77	-8.77
risky option	[20.91]	[18.12]	(3.93)	[20.83]	[24.79]	(4.36)	(5.43)	(5.53)
Competitive wage scheme	0.41	0.36	0.05	0.50	0.38	0.12	0.06	0.06
	[0.50]	[0.49]	(0.10)	[0.50]	[0.49]	(0.10)	(0.12)	(0.12)
Self Assessment	4.38	4.55	-0.17	4.54	4.10	0.44	0.61	0.61
	[1.88]	[2.45]	(0.41)	[2.49]	[1.85]	(0.45)	(0.63)	(0.64)
Relative Rank	4.20	3.82	0.38**	4.01	3.51	0.50**	0.11	0.11
	[0.89]	[1.12]	(0.19)	[1.09]	[1.23]	(0.22)	(0.29)	(0.30)
Panel B: Socioeconomic Characteristics								
Age	24.33	22.51	1.82					
	[6.05]	[5.76]	(1.16)					
Completed secondary schooling	0.43	0.51	-0.08					
	[0.49]	[0.50]	(0.09)					
Experienced in stitching and tailoring	0.52	0.51	0.01					
	[0.50]	[0.50]	(0.09)					
Married	0.52	0.35	0.17*					
	[0.50]	[0.48]	(0.09)					
SC	0.615	0.615	0.000					
	[0.49]	[0.49]	(0.09)					
Hindu	0.974	0.974	0.000					
	[0.15]	[0.16]	(0.03)					
Dependency ratio	0.37	0.32	0.05					
	[0.63]	[0.50]	(0.11)					
F-test of joint significance†			0.83					
p-value			(0.61)					
Sample size	78	39		78	39		234	234

Notes: Standard deviation reported in square brackets and standard errors in parentheses. In Column 8, we control for a full set of pre-intervention socioeconomic characteristics (see notes of Tabe 8) and center dummies. In Columns 7 and 8, robust standard errors clustered at the individual level in parentheses. Sample Includes all women included in the behavioral sample. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$.

Table 12: Certificate Effect?

	6-month effect	18-month effect	6-month effect	18-month effect
	ATTENDANCE: Number of days attended		*ATTENDANCE*: Number of months attended	
	(1)	(2)	(3)	(4)
Panel A: Labor Market Outcomes				
Casual/full-time employment	0.0012**	0.0015**	0.019**	0.023**
	(0.0005)	(0.0007)	(0.008)	(0.011)
Self-employment	0.0009**	0.0009***	0.014**	0.014**
	(0.0003)	(0.0003)	(0.006)	(0.006)
Employed	0.0014**	0.0019**	0.021**	0.029**
	(0.0006)	(0.0007)	(0.010)	(0.012)
Hours worked	0.054***	0.080***	0.88***	1.23***
	(0.019)	(0.03)	(0.29)	(0.47)
Monthly earnings	3.23**	4.64**	49.85**	71.70**
	(1.62)	(2.20)	(24.92)	(34.11)
Panel B: Entrepreneurship, Empowerment, and Life Satisfaction				
Own sewing machine	0.002	0.003***	0.03	0.046***
	(0.001)	(0.0011)	(0.018)	(0.018)
ROSCA membership	-0.00002	0.000003	-0.0003	0.00008
	(0.0005)	(0.0006)	(0.007)	(0.010)
Happy at home	-0.0008	-0.0006	-0.013	-0.009
	(0.0006)	(0.0006)	(0.009)	(0.009)
Sample size	878		878	

Notes: $2010 - -2011 - -2012$ sample used. Excluded instruments include *Treatment*, *Treatment* $\times$ *Age*, and *Treatment* $\times$ *Year*. Regressions control for a full set of pre-intervention socioeconomic characteristics (age, completed secondary schooling, married, experienced in stitching and tailoring, hindu, SC, dependency ratio), lagged outcome variable, region of residence, and center dummies. Robust standard errors clustered at the individual level in parentheses. $^{***}p < 0.01,^{**}$ $p < 0.05,^{*}$ $p < 0.1$. In Columns 1 and 2, *ATTENDANCE* is defined as the number of days the woman actually attends class. In Columns 3 and 4, *ATTENDANCE* is defined as the total number of months a woman assigned to the treatment group attends class.

Table A-1: Baseline Characteristics: 2010 Sample

	Full Sample (1)	Treatment (2)	Control (3)	Difference (4 = 2 - 3)
Panel A: Labor Market Outcomes				
Casual/full-time employment	0.033	0.036	0.027	0.009
	[0.18]	[0.19]	[0.16]	(0.016)
Self-employment	0.023	0.024	0.021	0.03
	[0.15]	[0.15]	[0.14]	(0.013)
Employed	0.048	0.051	0.043	0.008
	[0.21]	[0.22]	[0.20]	(0.02)
Hours worked	0.98	1.10	0.72	0.38
	[5.72]	[6.14]	[4.69]	(0.50)
Hours worked [if employed]	20.24	21.57	16.75	4.82
	[17.06]	[17.48]	[16.49]	(7.16)
Monthly earnings	49.70	59.26	28.58	30.68
	[342.24]	[394.36]	[178.82]	(30.32)
Monthly earnings [if employed]	1018.12	1154.19	660.93	493.25
	[1208.06]	[1358.56]	[602.43]	(502.24)
Panel B: Entrepreneurship, Empowerment, and Life Satisfaction				
Own sewing machine	0.35	0.31	0.43	-0.12***
	[0.47]	[0.46]	[0.49]	(0.04)
ROSCA membership	0.114	0.11	0.10	0.01
	[0.32]	[0.32]	[0.31]	(0.03)
Happy at home	0.87	0.86	0.89	-0.03
	[0.33]	[0.34]	[0.31]	(0.03)
Panel C: Socioeconomic Characteristics				
Age	22.33	22.39	22.18	0.21
	[5.77]	[5.78]	[5.77]	(0.51)
Completed secondary schooling	0.44	0.45	0.44	0.01
	[0.49]	[0.49]	[0.50]	(0.048)
Experienced in stitching and tailoring	0.203	0.202	0.205	-0.003
	[0.40]	[0.40]	[0.40]	(0.035)
Married	0.33	0.34	0.31	0.03
	[0.47]	[0.47]	[0.46]	(0.04)
SC	0.51	0.511	0.508	0.003
	[0.50]	[0.50]	[0.50]	(0.04)
Hindu	0.47	0.47	0.46	0.01
	[0.49]	[0.49]	[0.50]	(0.04)
Dependency ratio	0.26	0.27	0.24	0.03
	[0.48]	[0.49]	[0.45]	(0.04)
Test of Joint Significance†				2.29
Test of Joint Significance††				2.65
Sample Size	594	409	185	

Notes: Standard deviation reported in brackets and standard errors reported in parentheses. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$. † : excluding ownership of sewing machine. †† : including ownership of sewing machine.

Table A-2: Baseline Characteristics: 2010 – 2011 Panel

	Full Sample (1)	Treatment (2)	Control (3)	Difference (4 = 2 - 3)
Panel A: Labor Market Outcomes				
Casual/full-time employment	0.037	0.043	0.025	0.018
	[0.19]	[0.20]	[0.15]	(0.018)
Self-employment	0.025	0.026	0.025	0.001
	[0.16]	[0.16]	[0.15]	(0.015)
Employed	0.053	0.057	0.044	0.013
	[0.22]	[0.23]	[0.20]	(0.021)
Hours worked	1.12	1.31	0.73	0.58
	[6.15]	[6.66]	[4.86]	(0.59)
Hours worked [if employed]	21.07	22.65	16.57	6.08
	[17.22]	[17.20]	[17.80]	(7.61)
Monthly earnings	56.59	70.25	26.96	43.28
	[368.51]	[428.58]	[176.45]	(35.30)
Monthly earnings [if employed]	1056.50	1211.90	612.5	599.40
	[1237.05]	[1367.19]	[633.64]	(540.88)
Panel B: Entrepreneurship, Empowerment, and Life Satisfaction				
Own sewing machine	0.36	0.32	0.43	-0.11**
	[0.48]	[0.47]	[0.49]	(0.04)
ROSCA membership	0.113	0.115	0.106	0.009
	[0.31]	[0.32]	[0.31]	(0.03)
Happy at home	0.87	0.86	0.89	-0.03
	[0.33]	[0.34]	[0.31]	(0.03)
Panel C: Socioeconomic Characteristics				
Age	22.24	22.33	22.04	0.29
	[5.66]	[5.66]	[5.66]	(0.54)
Completed secondary schooling	0.44	0.449	0.433	0.016
	[0.49]	[0.49]	[0.49]	(0.047)
Experienced in stitching and tailoring	0.216	0.217	0.213	0.004
	[0.41]	[0.41]	[0.41]	(0.04)
Married	0.33	0.34	0.31	0.03
	[0.47]	[0.47]	[0.46]	(0.04)
SC	0.51	0.52	0.49	0.03
	[0.50]	[0.50]	[0.50]	(0.05)
Hindu	0.494	0.498	0.484	0.014
	[0.50]	[0.50]	[0.50]	(0.048)
Dependency ratio	0.257	0.262	0.248	0.014
	[0.48]	[0.49]	[0.45]	(0.04)
Test of Joint Significance†				1.34
Test of Joint Significance††				1.77
Sample Size	504	345	159	

Notes: Standard deviation reported in brackets and standard errors reported in parentheses. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$. † : excluding ownership of sewing machine. †† : including ownership of sewing machine.

Table A-3: Baseline Characteristics: 2010 – 2012 Panel

	Full Sample (1)	Treatment (2)	Control (3)	Difference (4 = 2 - 3)
Panel A: Labor Market Outcomes				
Casual/full-time employment	0.032	0.033	0.032	0.001
	[0.177]	[0.178]	[0.176]	(0.017)
Self-employment	0.024	0.024	0.025	-0.001
	[0.15]	[0.15]	[0.16]	(0.015)
Employed	0.048	0.047	0.051	-0.04
	[0.21]	[0.21]	[0.22]	(0.02)
Hours worked	0.93	0.95	0.85	0.10
	[5.45]	[5.61]	[5.10]	(0.53)
Hours worked [if employed]	18.96	20.06	16.75	3.31
	[16.63]	[17.12]	[16.49]	(7.32)
Monthly earnings	42.88	47.07	33.89	13.18
	[285.11]	[318.85]	[194.37]	(27.65)
Monthly earnings [if employed]	877.33	985.53	660.93	324.6
	[983.81]	[1130.16]	[602.43]	(430.04)
Panel B: Entrepreneurship, Empowerment, and Life Satisfaction				
Own sewing machine	0.35	0.31	0.44	-0.13***
	[0.47]	[0.46]	[0.49]	(0.04)
ROSCA membership	0.116	0.12	0.11	0.01
	[0.32]	[0.32]	[0.31]	(0.03)
Happy at home	0.87	0.86	0.89	-0.03
	[0.33]	[0.34]	[0.30]	(0.03)
Panel C: Socioeconomic Characteristics				
Age	22.28	22.27	22.31	-0.04
	[5.80]	[5.80]	[5.83]	(0.56)
Completed secondary schooling	0.45	0.44	0.46	-0.02
	[0.50]	[0.49]	[0.50]	(0.05)
Experienced in stitching and tailoring	0.213	0.211	0.217	-0.006
	[0.49]	[0.41]	[0.41]	(0.04)
Married	0.323	0.322	0.326	-0.004
	[0.47]	[0.46]	[0.47]	(0.04)
SC	0.521	0.522	0.519	0.003
	[0.40]	[0.50]	[0.50]	(0.05)
Hindu	0.49	0.48	0.52	-0.04
	[0.49]	[0.49]	[0.50]	(0.05)
Dependency ratio	0.25	0.26	0.23	0.03
	[0.48]	[0.49]	[0.45]	(0.04)
Test of Joint Significance†				1.44
Test of Joint Significance††				1.68
Sample Size	491	335	156	

Notes: Standard deviation reported in square brackets and standard errors reported in round brackets. $^{***}p < 0.01$,** $p < 0.05$,* $p < 0.1$. † : excluding ownership of sewing machine. †† : including ownership of sewing machine.

Table A-4: First-Stage Regression Results for SATYA-Pratham Program: 6-month effect

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.676***	0.689***	0.678***	0.677***	0.677***	0.661***	0.673***	0.676***
	(0.117)	(0.117)	(0.117)	(0.117)	(0.117)	(0.120)	(0.117)	(0.117)
Treatment × *Year*	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treatment × *Age*	-0.005	-0.006	-0.005	-0.005	-0.005	-0.005	-0.005	-0.005
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Year	0.000	0.000***	0.000	0.000	0.000	0.000	0.000	0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Experienced in	0.101**	0.095*	0.098**	0.102**	0.102**	0.116**	0.090*	0.103**
stitching and tailoring	(0.050)	(0.050)	(0.050)	(0.050)	(0.050)	(0.054)	(0.050)	(0.050)
Age	-0.002	-0.003	-0.003	-0.002	-0.002	-0.002	-0.002	-0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Completed secondary	0.166***	0.170***	0.164***	0.166***	0.165***	0.168***	0.161***	0.166***
schooling	(0.038)	(0.038)	(0.038)	(0.038)	(0.038)	(0.038)	(0.039)	(0.039)
Married	0.048	0.059	0.054	0.048	0.049	0.048	0.049	0.046
	(0.067)	(0.067)	(0.067)	(0.067)	(0.067)	(0.067)	(0.067)	(0.067)
Hindu	-0.043	-0.043	-0.045	-0.042	-0.045	-0.040	-0.044	-0.039
	(0.058)	(0.058)	(0.058)	(0.058)	(0.059)	(0.058)	(0.058)	(0.058)
SC	-0.016	-0.018	-0.016	-0.015	-0.015	-0.018	-0.020	-0.016
	(0.039)	(0.039)	(0.039)	(0.039)	(0.039)	(0.039)	(0.040)	(0.039)
Dependency ratio	0.035	0.033	0.033	0.035	0.034	0.035	0.038	0.036
	(0.046)	(0.047)	(0.046)	(0.046)	(0.047)	(0.046)	(0.046)	(0.046)
Constant	-0.145	-0.147	-0.138	-0.148	-0.145	-0.134	-0.140	-0.161
	(0.113)	(0.112)	(0.113)	(0.112)	(0.113)	(0.113)	(0.112)	(0.125)
Smple size	878	878	878	878	878	878	878	878

Notes: Dependent variable *TRAINED*. Regressions also include region of residence, center dummies, and the lagged outcome variable. Columns (1) - (8) present the first-stage regressions corresponding to the endogenous regressor *TRAINED* in 3. Robust standard errors clustered at the individual level in parentheses. $^{***}p < 0.01$,** $p < 0.05$,* $p < 0.1$. Estimation conducted on the 2010-2011-2012 sample. Excluded instruments are *Treatment*, *Treatment* × *Age*, and *Treatment* × *Year*.

Table A-5: First-Stage Regression Results for SATYA-Pratham Program: 18-month effect

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.058	0.064	0.059	0.058	0.058	0.050	0.057	0.058
	(0.057)	(0.057)	(0.057)	(0.057)	(0.057)	(0.058)	(0.057)	(0.057)
Treatment × *Year*	0.560***	0.560***	0.560***	0.560***	0.560***	0.560***	0.560***	0.560***
	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)
Treatment × *Age*	-0.003	-0.003	-0.003	-0.003	-0.003	-0.002	-0.003	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Year	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Experienced in stitching	0.050**	0.048*	0.049**	0.051**	0.051**	0.058**	0.045*	0.052**
and tailoring	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.027)	(0.025)	(0.025)
Age	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Completed secondary	0.083***	0.085***	0.082***	0.083***	0.083***	0.084***	0.080***	0.083***
schooling	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
Married	0.024	0.030	0.027	0.024	0.024	0.024	0.025	0.023
	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.033)	(0.034)	(0.034)
Hindu	-0.021	-0.021	-0.022	-0.021	-0.022	-0.020	-0.022	-0.020
	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)
SC	-0.008	-0.009	-0.008	-0.008	-0.007	-0.009	-0.010	-0.008
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
Dependency ratio	0.017	0.016	0.017	0.018	0.017	0.017	0.019	0.018
	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)
Constant	-0.072	-0.073	-0.069	-0.074	-0.072	-0.067	-0.070	-0.081
	(0.056)	(0.056)	(0.056)	(0.056)	(0.056)	(0.057)	(0.056)	(0.062)
Sample size	878	878	878	878	878	878	878	878

Notes: Dependent variable *TRAINED* × *YEAR*. Regressions also include region of residence, center dummies, and the lagged outcome variable. Columns (1) - (8) present the first-stage regressions corresponding to the endogenous regressor *TRAINED* × *YEAR* in 3. Robust standard errors clustered at the individual level in parentheses. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$. Estimation conducted on the 2010-2011-2012 sample. Excluded instruments are *Treatment*, *Treatment* × *Age*, and *Treatment* × *Year*.

Table A-6: First-Stage Regression Results for *Any* Program: 6-month Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.690***	0.694***	0.691***	0.692***	0.691***	0.679***	0.688***	0.690***
	(0.167)	(0.167)	(0.167)	(0.167)	(0.167)	(0.167)	(0.167)	(0.167)
Treatment × *Year*	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.00)	(0.000)	(0.000)	(0.00)	(0.00)	(0.000)	(0.00)	(0.00)
Treatment × *Age*	-0.007	-0.007	-0.007	-0.007	-0.007	-0.007	-0.007	-0.007
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)
Year	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.00)	(0.000)	(0.000)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Age	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Completed secondary	0.143***	0.146***	0.143***	0.143***	0.142***	0.145***	0.140***	0.147***
schooling	(0.041)	(0.042)	(0.041)	(0.041)	(0.041)	(0.041)	(0.042)	(0.042)
Married	0.062	0.063	0.065	0.062	0.063	0.061	0.061	0.060
	(0.074)	(0.075)	(0.074)	(0.074)	(0.074)	(0.074)	(0.074)	(0.074)
Hindu	-0.038	-0.036	-0.038	-0.039	-0.041	-0.035	-0.038	-0.037
	(0.065)	(0.065)	(0.065)	(0.065)	(0.066)	(0.065)	(0.065)	(0.065)
SC	-0.014	-0.015	-0.014	-0.012	-0.012	-0.016	-0.017	-0.014
	(0.042)	(0.042)	(0.042)	(0.042)	(0.042)	(0.042)	(0.043)	(0.042)
Experienced in stitching	0.116**	0.116**	0.115**	0.117**	0.117**	0.127**	0.109*	0.119**
and tailoring	(0.057)	(0.057)	(0.057)	(0.056)	(0.056)	(0.060)	(0.057)	(0.056)
Dependency ratio	0.008	0.009	0.008	0.009	0.007	0.009	0.011	0.009
	(0.051)	(0.051)	(0.051)	(0.051)	(0.051)	(0.050)	(0.050)	(0.051)
Constant	-0.089	-0.096	-0.089	-0.091	-0.088	-0.085	-0.089	-0.079
	(0.176)	(0.175)	(0.176)	(0.175)	(0.176)	(0.179)	(0.175)	(0.179)
Sample size	878	878	878	878	878	878	878	878

Notes: Dependent variable *TRAINED any*. Regressions also include region of residence, center dummies, and the lagged outcome variable. Columns (1) - (8) present the first-stage regressions corresponding to the endogenous regressor *TRAINED any*. Robust standard errors clustered at the individual level in parentheses. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$. Estimation conducted on the 2010-2011-2012 sample. Excluded instruments are *Treatment*, *Treatment* × *Age*, and *Treatment* × *Year*.

Table A-7: First-Stage Regression Results for *Any* Program: 18-month Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.083	0.085	0.084	0.084	0.084	0.078	0.082	0.083
	(0.083)	(0.083)	(0.082)	(0.082)	(0.082)	(0.085)	(0.083)	(0.082)
Treatment × *Year*	0.523***	0.523***	0.523***	0.523***	0.523***	0.523***	0.523***	0.523***
	(0.040)	(0.040)	(0.040)	(0.040)	(0.040)	(0.040)	(0.040)	(0.040)
Treatment × *Age*	-0.004	-0.004	-0.004	-0.004	-0.004	-0.003	-0.004	-0.004
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Year	0.128***	0.128***	0.128***	0.128***	0.128***	0.128***	0.128***	0.128***
	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
Age	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Completed secondary	0.071***	0.073***	0.071***	0.071***	0.071***	0.073***	0.070***	0.073***
schooling	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Married	0.031	0.032	0.032	0.031	0.031	0.030	0.031	0.030
	(0.037)	(0.037)	(0.037)	(0.037)	(0.037)	(0.037)	(0.037)	(0.037)
Hindu	-0.019	-0.018	-0.019	-0.019	-0.021	-0.017	-0.019	-0.019
	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)	(0.032)	(0.033)	(0.033)
SC	-0.007	-0.008	-0.007	-0.006	-0.006	-0.008	-0.009	-0.007
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Experienced in stitching	0.058**	0.058**	0.058**	0.058**	0.058**	0.064**	0.055*	0.059**
and tailoring	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.030)	(0.029)	(0.028)
Dependency ratio	0.004	0.005	0.004	0.004	0.004	0.005	0.006	0.005
	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)
Constant	-0.108	-0.112	-0.108	-0.109	-0.108	-0.107	-0.109	-0.103
	(0.088)	(0.087)	(0.087)	(0.087)	(0.087)	(0.089)	(0.087)	(0.089)
Sample size	878	878	878	878	878	878	878	878

Notes: Dependent variable *TRAINED*: completed *any* program × *YEAR*. Regressions also include region of residence, center dummies, and the lagged outcome variable. Columns (1) - (8) present the first-stage regressions corresponding to the endogenous regressor *TRAINED any* × *YEAR*. Robust standard errors clustered at the individual level in parentheses. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$. Estimation conducted on the 2010-2011-2012 sample. Excluded instruments are *Treatment*, *Treatment* × *Age*, and *Treatment* × *Year*.