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Learning and Earning: Evidence from a Randomized Evaluation in India

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Keywords

Vocational Training, Panel Data, India, Economic Returns, Field Experiment

Disciplines

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Comments

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JEL Classification: I21, J19, J24, 015

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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 most recent 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, pp 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 be sent back to school. Policy makers and international organizations such as the World Bank, USAID, and DFID consider vocational training to be one promising avenue through which young adults, particularly women, can acquire marketable skills that will enable them to secure employment.[1] Despite the large scale expansionary policies aimed at increasing access to vocational training programs, there is very little evidence on the returns to such training programs from developing countries. Experimental evidence is particularly scarce and furthermore, evidence on the medium-run effects of such programs from developing countries are non-existent.

This paper addresses crucial gaps in the existing literature, and to the best of our knowledge is the first to present both the short-and-medium-run impact estimates of participating in a subsidized vocational training program in a developing country. More specifically, this paper quantifies the returns from participating in a training program in stitching and tailoring, targeted at women aged 18 – 39 years, with at least 5 or more grades of schooling residing in poor slums of New Delhi in India. Applicants to this training program were randomly assigned to one of the two groups: treatment group (received access to the 6-month training) and the control group (did not receive access to the training). The short-run intent-to-treat (ITT) effects of the program indicate that within 6 months after the completion of the program, 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

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

than women in the control group. Using a second round of follow-up data collected 18 months after the completion of the program, we find that the effects of the program on employment, hours worked, and earnings are all sustained in the medium-run.

There now exists a fairly large literature that assesses the impact of participating in vocational training programs on earnings and employment opportunities using data from developed countries (see Ashenfelter, 1978; Ashenfelter and Card, 1985; Card and Sullivan, 1988; Hotz, Imbens, and Klerman, 2006). The general conclusions that arise from these US and European experiences is that the impact of vocational training programs is generally modest, at best, and that the effectiveness of the program varies with the characteristics of the participants and the type of training (see Heckman, Lalonde, and Smith, 1999; Kluve, 2006, for systematic reviews). However, applying these findings to developing countries is inappropriate for a number for reasons, most important of which is that trainees in developing countries are likely to start with very low levels of formal education, skills and full-time employment, compared to those in developed nations.

Evidence on the effectiveness of training programs in developing countries is however much more limited. For example, Betcherman, Olivas, and Dar (2004) in their review of 69 impact evaluations of unemployed and youth training programs find only 19 in developing countries. They find that impact estimates of training programs in developing countries are larger than estimates for training programs in the United States and Europe. Nopo and Saavedra (2003) in their review of training programs in Latin America arrive at the same conclusion. However, a large majority of the programs analyzed in these surveys are non-experimental in nature. Experimental evaluation of labor market training programs in developing countries is fairly rare – two exceptions are Card, Ibarraran, Regalia, Rosas, and Soares (2011) and Attanasio, Kugler, and Meghir (2011). The results from these two papers are quite mixed. Card, Ibarraran, Regalia, Rosas, and Soares (2011), using data from a government subsidized training program for low-income youth in urban areas of the Dominican Republic, find that the program only marginally improved hourly wages, and the probability of health insurance coverage, conditional on employment, and find no significant impact of the training program on the subsequent employability of trainees. On the other hand, Attanasio, Kugler, and Meghir (2011) are more positive – using data from a randomized vocational training program aimed at disadvantaged youth in Colombia in 2005, they find that the program raised earnings and employment for women.[2]

[2]Hicks, Kremer, Mbiti, and Miguel (2012) and Field, Linden, and Wang (2012) are currently conducting similar evaluations in Kenya and Mongolia respectively. The results of both these projects are as yet

This paper makes three distinct contributions to the literature. First, we are not only able estimate the short-run intent-to-treat effects of the program but are the first to capture the medium-run estimates of having access to and completing a vocational training program. Second, we pay substantial attention to barriers to program completion and not just program take-up. Third, none of the existing papers examine alternate "pathways" that can result in similar impact estimates. Our paper is successful in ruling out two possible alternate pathways – changes in participants behavior and signalling, both of which can bias the treatment effects. Our results suggest that women who participated in the program indeed reap benefits through skill accumulation.

We designed a field experiment in conjunction with two non-governmental organizations (NGOs): Pratham Delhi Education Initiative (henceforth Pratham) and Social Awakening Through Youth Action (henceforth Satya). These two NGOs together offered a subsidized six-month course in stitching and tailoring to women between ages 18 and 39 years, residing in poor slum communities of New Delhi, India. Eligible women were invited to apply to the program and from within the pool of eligible applicants, a lottery determined each woman's assignment into one of the following two groups: treatment group (received access to this course) and control group (did not receive access to this course). Baseline pre-intervention data was collected during July – August 2010, the first-round of follow-up (midline survey) was conducted in July – August 2011 and the second-round of follow-up (endline survey) was conducted in August – September 2012. We combine pre-intervention data with the two-rounds of follow-up data to evaluate the short-and-medium-run effects of participating in the program. There are significant program effects both in the short and the medium run. As 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 enable a better study of whether impacts continue to persist or not in the long-run. To our knowledge, this is the first paper to explicitly address this issue in the

unavailable. Fiala, Martinez, and Blattman (2011) examine the effectiveness of a cash transfer program in Uganda that provided young adults nearly unconditional, unsupervised cash transfers to pay for vocational training, tools, and business start-up costs. They find that despite a lack of central monitoring and accountability, most youth invest the transfer in vocational skills and tools. Second, the economic impacts of the transfer are large: hours of non-household employment double and cash earnings increase by nearly 50% relative to the control group. Macours, Premand, and Vakis (2012) find that in the context of Nicaragua access to vocational training in conjunction with a conditional cash transfer program enable households to insure against weather related shocks. They argue that combining safety nets with productive interventions can help households manage future weather risks and promote longer-term program impacts. Groh, Krishnan, McKenzie, and Vishwanath (2012) find that soft skills training program provided to female graduates in Jordan, aimed at improving their employability, has had very limited impact. Skills training is only one component of these impact evaluation studies and hence the results from these three papers are not directly comparable to what we do in this paper.

context of vocational training program in a developing country.

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, "*And 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 of the intervention, the data, and timing of events throughout the study period. The short-and-medium-run impact estimates of the training program are presented in Section 3. Section 4 discusses the barriers to program completion. A cost-benefit analysis of the program is discussed in Section 5. Alternative explanations that could potentially drive the results are discussed in Section 6. Finally, Section 7 ends with a conclusion and discussion.

2 Design

2.1 The Program

Theories of human capital accumulation suggest that limited income and wealth combined with credit market imperfections makes it difficult to finance investments in education even when the marginal returns from such investments may exceed the marginal costs (Becker 1967). To alleviate credit constraints faced by women in developing countries, we design an intervention in vocational training program in stitching and tailoring services in conjunction with two non-governmental organizations (NGOs): Pratham and Satya. Pratham is one of the largest NGO's in India reaching out to more than 3 million underprivileged children with their initiatives in pre-school education all over the nation. Satya, on the

other hand, is a small NGO which specializes in providing skill development programs to residents in poor communities. Pratham and Satya partnered to provide a rigorous six-month long training program in stitching and tailoring services with the aim of making women in these areas adept in making clothes for children, adult men and women. The program will henceforth be termed as the Satya/Pratham program.

In May 2010, a complete census was administered in the targeted areas in New Delhi. 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 recent expansion. 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 potential applicants were also informed (via the advertisement) 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). The English version of the advertisement for the program is presented in Figure 1.

All selected participants were required to deposit Rs 50 per month for continuing in the program. This required participants to be ready to commit a total of Rs 300 for the entire duration of the training 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 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. All eligible women were informed of this deposit requirement in the advertisement, prior to applying. Finally, the potential participants were also told that they would receive a certificate on completing the program. Satya and Pratham employees held joint information sessions, where interested women had the opportunity to meet with representatives from the two NGOs to discuss and clarify questions about the program. By the end of June 2010, Pratham received 658

applications.[3]

Randomization was stratified by location – North Shahdara and South Shahdara. Two-thirds of all applications from each area (that is, 164 of the 244 applicants from South Shahdara and 278 of the 414 applicants from North Shahdara) were assigned to the treatment group (women who were offered a spot in the program) and the remaining one-third were assigned to the control group (women who were not offered a place in the program). The randomization was conducted as follows. First, every applicant was given an ID number. These ID numbers were written in chits of paper, which 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 at the time this randomization exercise was conducted. The baseline survey was underway at the time of the randomization 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 and received more applications and had 3 training centers; the remaining 2 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 necessary. 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.

The actual program started during the second/third week of August 2010 and continued through to the last week of January 2011. The baseline survey was conducted during the period July - August 2010, the first follow-up (midline) survey during the same two months in 2011 and the second follow-up (endline) survey during August - September 2012. Figure 2 provides a schematic representation of the chronology of events.

[3]A small proportion of eligible women applied for the program, but the applicants are representative of the set of eligible women in the area in terms of both age and in terms of school completion – the two variables on which we collected data in our census.

2.2 Data - Baseline, Follow-up and Attrition

2.2.1 Baseline Data

The baseline socio-economic survey, conducted in July - August 2010 attempted to survey all 658 women who applied to the program. However, survey data could only be collected for 90 percent of the applicants due to respondent's unavailability and occasional refusal to participate in the survey. Our baseline data consists of 594 women, of whom 409 belong to the treatment group and the remaining 185 belong to the control group. The household questionnaire was designed to collect detailed information on household demographic characteristics, ownership of household assets, labor market outcomes, measure of bargaining power, and life satisfaction. The list of outcome variables is presented in Panels A and B of Table 1.

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 test this assumption we report pre-intervention averages of all variables used later in the regression analysis. Columns 2 and 3 of Table 2 report 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. There is no systematic difference in labor market outcomes presented in Panel A of Table 2 between the treatment and the control group. Women in the two groups also exhibit similar levels of happiness and bargaining power (empowerment), captured by ROSCA (Rotating Savings and Credit Association) membership as reported in Panel B of Table 2.[4] Overall the baseline outcome variables reported in Panels A and B of Table 2 and the covariates reported in Panel C of Table 2 are all balanced, except for ownership of sewing machine, which is higher for women in the control group. The overall joint F-test on the regression of the treatment dummy on all baseline covariates, shows that we cannot reject the null hypothesis that baseline characteristics of women did not predict assignment to treatment. 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

[4] Anderson and Baland (2002) propose an explanation of membership of ROSCAs in Kenya (similar to chit funds in India) based on conflictual interactions within the household. In their paper, participation in a ROSCA is a strategy a wife employs to protect her savings against claims by her husband for immediate consumption. So membership in a ROSCA could be viewed as a measure of bargaining power of the woman.

than fifty percent of the women belong to scheduled castes.[5] In our sample, labor market participation rates are quite low to begin with – in terms of employment, hours worked and also earnings.

2.2.2 Follow-up Data and Attrition

We conducted two rounds of follow-up data collection. The midline survey was conducted during July - August 2011, approximately 6 months after the training program was completed and measures the short-run effects of the program. The endline survey was conducted during July - August 2012, roughly 18 months after the training program was completed and measures the medium-run impacts of the program. Our goal during both rounds of follow-up data collection was to target and interview all 594 women surveyed in the baseline. Of the 594 women surveyed in 2010, 504 could be re-surveyed in 2011 (tracking rate of 85% between 2010 and 2011) and of these 504 women, 439 could also be followed through the 2012 round of the survey (tracking rate of 87% between 2011 and 2012). Our enumerators were also able to trace an additional 52 women during 2012, who were not traced in 2011 making a total of 491 women surveyed in 2010 and 2012 (tracking rate of 83% during 2010 - 2012 period). Our final estimation sample consists of the 439 women who could be traced and interviewed in all three years. We call this the 2010-2011-2012 sample.[6]

We examine the robustness of our impact estimates to two alternative subsamples – 2010-2011 sub-sample (504 women) and the 2010-2012 sub-sample (491 women). Table A-1 in the appendix shows that as with the 2010-2011-2012 sample (Table 2) the baseline covariates are balanced, with the exception of ownership of sewing machine.[7]

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

[6] The attrition rates found here are comparable to other papers in this literature. For example Attanasio, Kugler, and Meghir (2011) are unable to follow around 18.5 percent of their baseline sample about $13-15$ months after the conclusion of their program and Card, Ibarraran, Regalia, Rosas, and Soares (2011) are unable to track around 20 percent of their baseline sample $18-24$ months after their initial application into the program.

[7] In Table A-1 we report two sets of overall joint F-tests – one on the regression of the treatment dummy on all baseline covariates and the second on the regression of the treatment dummy on all baseline covariates excluding the ownership of sewing machine. We find that the joint F-test on the regression including all baseline covariates for the 2010-2011 sample is 1.78 ($p-value=0.03$) and the 2010-2012 sample is 1.58 ($p-value=0.07$). The corresponding F-test becomes smaller and the $p-value$ larger for the 2010-2011 sample as 1.34 ($p-value=0.18$) and the 2010-2012 sample as 1.34 ($p-value=0.17$) when we exclude the ownership of sewing machine confirming that the only source of possible imbalance in the baseline covariates is that owning to ownership of sewing machine.

Our identification strategy also assumes that there is no differential attrition between the treatment and the control group. To test this, we first examine how the baseline socioeconomic characteristics affect the likelihood of attrition. In Table 3, column 1 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. Notice 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.

Second, following Becketti, Gould, Lillard, and Welch (1988) we also regress all the different outcome variables of interest at the baseline on the 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). 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 4, columns 1 - 5 for the labor market outcomes at baseline; columns 6 - 8 for the entrepreneurship, empowerment and life satisfaction variables at baseline. The joint F statistics on the attrition dummy (*Attrite*) and all the interaction terms appended in Table 4 is never statistically 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.

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 (2010-2011-2012) 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 3, 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 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 affected by the exclusion of the temporary attritors from the estimating sample.

3 Program Impacts

3.1 Effect of *TRAINING*: ITT Estimates

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

$$Y_{it} = \beta_0 + \beta_1 Y_{i0} + \beta_2 TRAINING_i + \beta_3 YEAR_t + \beta_4 TRAINING_i \times YEAR_t + \sum_{j=1}^{K} \gamma_j \mathbf{X}_{ij} + \epsilon_{it} \quad (1)$$

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 between the treatment and control groups. 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 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 - this doesn't change over time. $YEAR$ 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.[8] Finally, ϵ_{it} is a random i.i.d. disturbance term. So β_2 gives us the short-run (i.e., over the period $2010 - 2011$) intent-to-treat 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 medium-run intent-to-treat effect of the program is then given by $\beta_2 + \beta_4$.

The set of pre-treatment (baseline) explanatory variables that we control for in the regressions include: *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*, a self-reported measure of prior experience in stitching and tai-

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

loring service (= 1 if the spent spent any time in the week prior to the survey stitching by hand; 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 common unobservables.

The short-and-medium-run intent-to-treat (ITT) effects of the vocational training program are reported in Table 5. The coefficient estimates presented in Panel A capture the short-and-medium-run intent-to-treat effects of being assigned to the treatment group (*TRAINING*) on labor market outcomes. In the short-run, the impact estimates reported in column 1, Table 5 suggest that during the post-training period, the likelihood of casual or permanent wage employment, self-employment, any employment, hours worked and monthly earnings are all statistically significantly higher for women who were offered the *TRAINING*. The program increases the likelihood of – casual or permanent wage employment by more than 5 percentage points, self-employment by almost 4 percentage points, and any employment by 6 percentage points. The program increases hours worked in the post-training period by around 2.5 hours, and monthly earnings by approximately Rs 150 in the short-run. Notice that for women who were not offered the *TRAINING*, the average hours worked is 1.12 and average monthly earnings is Rs 100. As a result, women in the treatment group work thrice as much and earn almost 150 percent more than women in the control group.

The medium-run effects presented in column (3) show that the effects have been sustained over the medium-run. The coefficient estimate on the interaction term (β_4) is always positive, though not statistically significant. While overall the average for the control group has also increased (hours worked increasing from 1.12 to 3.30 and monthly earnings from Rs 100 to Rs 252), 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 short-run the women who receive the *TRAINING* are 8 percentage points more likely to own a sewing machine (see Panel B in Table 5) – this effect is however not statistically significant ($p - value = 0.108$). In the medium-run, 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 ... 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 5), either in the short-run or the medium-run.

The overall proportion of employed (in any employment) 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 effects on labor market participation and hours worked that we obtain here are similar to the effects obtained for the female sample by Attanasio, Kugler, and Meghir (2011), particularly when we look at the effects on the probability of employment and on hours worked. However, we obtain much stronger effects on earnings. The effects are systematically higher compared to those obtained by Card, Ibarraran, Regalia, Rosas, and Soares (2011), who find very small effects on employment and only about a 10% increase in monthly earnings. Of course one must bear in mind that these programs are all very different – implemented in very different parts of the world, with very different target populations and so any direct comparison of the impacts is almost impossible.

To examine the robustness of our results, we estimate variants of equation (1) for the two subsamples (2010-2011 and 2010-2012). The results are presented in Table A-2 in the appendix. The results are very similar, both qualitatively and quantitatively, to those presented in Table 5 in that – both the short-run and the medium-run effects are positive and in general the medium-run effects are larger than the short-run effects. Achieving consistent results across sub-samples further alleviates concerns that attrition results in selection bias.

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 – and here we have 8 outcome variables. To rule this out we examine the ITT effects of the training program on summary indices using the approach outlined in

Kling, Liebman, and Katz (2007) and Karlan and Zinman (2009). We construct summary indices as follows: (*a*) an *all labor index* that includes any 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; (*d*) an *alternative all labor index* that includes any employment, hours worked, monthly earnings, and ownership of sewing machine; and finally (*e*) an *overall index* that includes any employment, hours worked, monthly earnings, ownership of sewing machine, happy at home, and rosca participation. The index method requires all variables to be converted into z-scores. The z-scores are constructed for each outcome variable using the mean and the standard deviation of the control group as the reference group. A higher value of the z-score implies an improvement. We take an equally weighted average of all the standardized outcomes within a domain to construct the indices.

We re-estimate equation (1) using the index measures as the outcome variables of interest. The short-run intent-to-treat estimates of assignment to *TRAINING* are presented in Panel C of Table 5). On an average, being offered the *TRAINING* improves labor market outcomes by almost 0.33 standard deviations. However, the training program has no impact on measures of empowerment and happiness. The index constructed over different labor market outcomes (*all labor index*, *self index*, and *alternative all labor index*) all reject the null hypothesis that the *TRAINING* has no effect on labor market outcomes in both the short-run and the medium-run at the 5% significance level. Rejection of the null here alleviates concerns relating to incorrect inference that comes with the use of multiple outcome variables as done in this paper. The *overall index* weighted over selected outcomes here also suggest that the program has positive effects, both in the short-run and the medium-run.

3.3 Effect of Program Completion: Treatment on the Treated (TOT)

Not everyone assigned to the treatment group completed the program and received the certificate at the end of the program. In our panel 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. 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. On an average program completers (hereafter

TRAINED) attended more than 70% of all classes in comparison to program drop-outs who attended only 4% percent of all classes during the training period. In panels A and B in Figure 3 we present the average monthly attendance for program completers and drop-outs respectively. Among program completers, average attendance is typically more than 70%, except in November when it falls to 60% due to the popular religious festival of Diwali. Average monthly attendance among program drop-outs starts out at around 20% in the beginning of the program in August 2010 and declines to 10% by September 2010 and remains at an average of 4% during November 2010 - January 2011. The majority of the drop-outs therefore occurred right at the beginning of the program.

Note that the Satya/Pratham program was not the only program available to women. There were other private providers, government training schools and other NGOs that offered similar courses in the city. We also asked the women whether they had completed *any* course in stitching/tailoring, irrespective of whether it was offered by Satya/Pratham or by any other government or private provider. An additional 9% of the women from the treatment group completed a course in stitching and tailoring that was not provided by Satya/Pratham. A slightly higher percentage of the women from the control group, 13% completed a course in stitching and tailoring that was not provided by Satya/Pratham. The difference is however not statistically significant ($p - value = 0.23$).

Panel A in Figure 3 depicts that the intensity of the training is likely to be considerably higher for those women who completed the training. The labor market, empowerment, entrepreneurship, and life satisfaction measures are also likely to vary by the intensity of training. To capture the returns from completing the vocational training course provided by *Satya/Pratham* and *any* program including Satya/Pratham, we estimate the following variant of equation (1) where the coefficient estimate on *TRAINED* and the interaction term *TRAINED* $\times$ *YEAR* respectively capture the short-and-medium-run impact of the treatment on the treated for both *Satya/Pratham* program and *any* program.

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

Since *TRAINED* and *TRAINED* $\times$ *YEAR* are both endogenously determined for both the *Satya/Pratham* and *any* program, we exploit random assignment to the treatment, that is,

being offered the training program along with its interaction with age and the year dummy to compute the TOT estimates for *TRAINED* and *TRAINED* $\times$ *YEAR*. The first-stage regression results for *TRAINED* and *TRAINED* $\times$ *YEAR* for the *Satya/Pratham* program are reported in Appendix Tables A-3 and A-4 respectively. The corresponding first-stage regression result for *TRAINED* and *TRAINED* $\times$ *YEAR* for *any* program are reported in Appendix Tables A-5 and A-6 respectively. The *treatment* indicator and the interaction term *Treatment* $\times$ *Year* reported in the first-stage regression results in Appendix Tables A-3 – A-6 suggest that access and credit constraints alleviated through assignment to the six-month subsidized training program indeed affects the likelihood of program completion. For instance, it shows 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. This difference in program completion rates between *Satya/Pratham* and *any* program suggest that a very small proportion of the sample women take-up courses offered by other providers confirming credit (and resource) constraints acting as a significant barrier to program participation and completion.

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 6 suggest that in the short-run, the *TRAINED* experience a 9 percentage point increase in the likelihood of obtaining casual or permanent wage employment, a 7 percentage point increase in the likelihood of obtaining self-employment, a 11 percentage point increase in the likelihood of obtaining any employment, work an additional 4.2 hours during the last week, earn an additional Rs 261 in monthly earnings and are 15 percentage point more likely to own a sewing machine, relative to the control group. The estimates reported in column (2) in Table 6 indicate that the short-run TOT effects persist into the medium-run and in almost all cases the difference effect is positive. This holds for both the labor market outcomes and entrepreneurship. The results for the short-and-medium-run effects for the Satya/Pratham program reported in columns 1 and 2 of Table 6 are very similar to the short-and-medium-run effects reported for *any* program reported in columns 4 and 5 of Table 6. Notice this result is not surprising since 80% of the women who completed the *any* program indeed completed the *Satya/Pratham* program.

Let us briefly summarize the results so far. First, we find positive association between access to the program and labor market outcomes in both the short-run and the medium-run – more importantly the short-run effects persist into the medium-run; we observe

no-decaying of program effects during the observed period. Second, the labor market effects are always positive, as are the effects on entrepreneurship.

4 Barriers to Program Completion

The results so far suggest that credit constraint is an important barrier to program participation and that women not assigned to the treatment group have very low rates of participation in *any* program. Even among women assigned to the treatment group, more than 40 percent do not complete the program. One of the reasons for low cost-effectiveness of the labor market training programs in developed countries and a potential challenge in low-and-middle income countries as well, has been the low rates of program completion. 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 - Germany (69%), Dominican Republic (60%), Uruguay (51%), and Peru (60%) (see Kluve, Card, Fertig, Gora, Jacobi, Jensen, Leetmaa, Nima, Patacchini, Schaffner, Schmidt, van der Klaauw, and Weber, 2007; Ibarraran and Rosas., 2009; Card, Ibarraran, Regalia, Rosas, and Soares, 2011).

Existing literature from developing countries fails to analyze drop-out behavior that can have significant implications for designing cost-effective labor market policies. We collect data on all program participants and non-participants and as a result can analyze the barriers to skill accumulation and program completion. We compute and present in column 1 of Table 7 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 more likely to be able to internalize the benefits of training and hence complete the program. Distance to the training center captured by the time taken to walk to the training center is a significant barrier to 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. We also use Figure 4 to show

that there is a strong negative relationship between the time taken to walk to the training center and the likelihood of program completion. 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.[9]

In column 2, Table 7 we present the marginal effects from a probit regression of *any* program completed, which include both the Satya/Pratham participants and others. Notice we cannot include distance to center as an explanatory variable as we do not have any measure of the distance to the training center for the other programs. We can include assignment to treatment (Satya/Pratham) 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 signifying the importance of access and credit constraints on program take-up and completion. The results are similar to those presented in column 1. In addition, we 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. Note that even for the Satya/Pratham program married women with co-habiting mother-in-laws are 18 percentage points more likely to complete the program, though this effect is not statistically significant at any conventional significance level. The fact that married women with co-resident mother-in-laws are more likely to complete the program is not surprising as co-resident mother-in-laws can provide low cost child care – this is a common arrangement in many developing countries. 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.

[9]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 the GPS instrument for measuring distance.

5 Cost-Benefit Analysis

To measure the cost-effectiveness of the underlying program, we present cost-benefit comparisons under two scenarios: first, for replicating the program at a different location and second, for continuation of the existing program. Under the first scenario, the NGO's total cost of the underlying vocational training program amounts to Rs 1810 per person[10], including both fixed (for example machines) and variable (for example teacher salary and rent) costs. In addition to this, the average program participant also incurs personal time costs, that is, the opportunity cost of participating in the training program which is proxied by the average monthly earnings of employed women in the treatment group during the pre-intervention period. In 2010, an average employed women from the treatment group earns Rs 985 in a month. The total time cost of the program for each participant then amounts to Rs 5910 (Rs 985 $\times$ 6 months) in terms of forgone earnings. As a result, the final cost per participant is Rs 7720 (Rs 5910 + Rs 1810), which is given by the sum of operating costs, fixed costs and opportunity costs computed over 6 months. The short-run ITT effects of the program reported in Table 5 indicate that the program increases annual earnings by Rs 1776 (Rs 148 $\times$ 12 months).

To compute the present discounted value of future income stream, we assume the following: (a) the working life of these women to be 40 years given that the average age of the respondent in our sample is 22 years, (b) 5 percent discount rate, and (c) no appreciation or depreciation in annual earnings.[11] Based on our ITT estimates and these assumptions, we obtain the present discounted value of future earnings stream for a participant to be Rs 31,998. This amounts to a net benefit of Rs 24,278 per participant. The total cost of the program can be recovered in less than four years. Even at a 10 percent discount rate, the total cost of the program can be recovered in less than five years.

The TOT estimates of the program are much larger and generate an income stream of Rs 56,429 over the participant's working life. Given that approximately 56 percent of all individuals who had access to the training program did not complete the program, the per unit cost of the program increases to Rs 9245 per person. The associated net benefit of the program remains substantially higher at Rs 47,185. However, it needs to be noted that that these estimates do not reflect general equilibrium costs and benefits of the vocational

[10] 1USD = Rs 50 (approximately).

[11] Note that our impact estimates suggest no depreciation in annual earnings and a marginal appreciation in annual earnings though not statistically significant making this assumption plausible.

training program. Incorporating the general equilibrium impacts are likely to change the returns, though it is not clear in which direction. On the one hand, as more and more women are trained and enter the labor market, the premium on training is likely to go down; on the other hand, if returns to training are convex, then not incorporating this kind of non-linearity implies that the returns to the program are likely to be under estimated.

Under the second scenario, the NGO only incurs variable cost such as teacher salary, rent and equipment maintenance; all of which sum up to Rs 1538 per person. Under these new cost calculations, the ITT estimates generate a net benefit of Rs 24,550 and the TOT estimates generate a net benefit of Rs 47,686. There are therefore considerable gains from both continuing the program in the same location and replicating the program in a different location.

Note additionally that the net benefits computed here possibly represent lower bounds for the benefits of the vocational-education program as they are based on short-run effects of the program, and do not account for gains from savings on clothing expenditure, and empowerment. Increase in women's labor force participation and earnings can have an impact on children's human capital, and these potential intergenerational effects have also not been accounted for in our computations.

6 Alternative Pathways?

Are the intent-to-treat effects observed here result of skill accumulation or "something else"? The intent-to-treat effects presented above capture the total impact of having access to this training program. It does not however, capture the intermediate channels such as skill accumulation, signalling, and or changes in behavior through which the training can potentially affect outcomes. In this section, we explore two alternative pathways. First, 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 intrinsic competitiveness which further explains for some of the observed difference in outcomes between the treatment and the control group. Second, 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).

6.1 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 a set of behavioral experiments prior to randomization, that is, before learning their treatment status and again 5 months after the training program. Due to operational constraints, the behavioral experiments could only be conducted in South Shahdara. The experiments were conducted in the Pratham office located in South Shahdara, a prominent and convenient place for all the participants. Pratham employees were hired to recruit for the behavioral experiments but the team of recruiters and indeed the officials of the two NGO's had no information about the content of these experiments. Of the 224 women residing in South Shahdara who applied for the program, 153 participated in these behavioral experiments in 2010. However, not all the women who participated in the behavioral experiments actually participated in the baseline survey and we have complete baseline data (both experimental and survey) for 135 women.

In May-June 2011, approximately five months after the training program was completed, we invited all the women who participated in the experiments in 2010 back to the Pratham office to participate in a similar set of experiments as in the previous year. Attempts were made to track and invite every woman who was in our final 2010 sample. Despite all effort, we were unable to trace around 15% of the participants in 2010. However, there are no systematic differences in the attrition rates across the two groups. We therefore have both pre and post-training data on 117 women.[12]

Each subject participated in two behavioral games. The basic structure of each game is similar to the games used in previous studies (see for example Gneezy, Leonard, and List, 2009). The first was the *Investment Game* where each player was given the option of investing any part of an initial endowment in a hypothetical risky project that had a 50-percent chance of tripling 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 intrinsic 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

[12]These artefactual field experiments or extra-lab experiments were conducted only at the baseline and the midline (that is, only in 2010 and 2011). Hence, we are unable to compute the medium run intent-to-treat effects of the training program on behavior.

had to choose one of two possible methods of compensation: a *piece-rate* or a *competition-rate* compensation method.[13] 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, Gangadharan, Maitra, Mani, and Subramanian (2012) for more details on the experiment. Panel C in Table 1 presents the list of behavioral outcome variables used in our analysis.

The sample characteristics at baseline are presented in Table 8. The sample here is restricted to women who participated in the behavioral experiments and were surveyed both in 2010 and in 2011. We call this the behavioral sample. At the baseline, there is no difference between the treatment and control group in terms of proportion of the endowment allocated to the risky option in the *Investment Game*, in terms of the choice of the wage scheme in the *Competition Game* and in terms of their self-assessment of the number of bags they expect to fill in the *Competition Game*. However at the baseline, women in the treatment group appear to be more self confident in a relative sense (captured by their relative rank) compared to the women in the control group. In terms of the socio-economic characteristics at the baseline (Panel B, Table 8), there are almost no differences in the sample averages between women in the two groups – the only exception being marital status: more women in the treatment group were married compared to the control group.

The primary question that we examine is: Does *TRAINING* cause changes in the behavioral/intrinsic characteristics of the women? As before, the panel dimension of the data on behavioral characteristics along with a randomized evaluation design implemented here allows us to measure the short-run ITT effects of the vocational training program on behavioral outcomes. We estimate a simple difference-in-difference specification.[14]

$$B_{it} = \beta_0 + \beta_1 TRAINING_i + \beta_2 YEAR_t + \beta_3 TRAINING_i \times YEAR_t + \sum_{j=1}^{K} \gamma_j \mathbf{X}_{ij} + \epsilon_{it} \quad (3)$$

B_{it} captures the decisions made by woman i in year t in the different tasks in the behavioral experiment. *TRAINING* is a dummy variable that takes the value of 1 if the woman is

[13] In the *piece-rate* compensation method, the woman's earnings depended solely on her own performance. In the *competition-rate* compensation method, her earnings depended on how she performed relative to a randomly chosen subject in the same session.

[14] Here we do not include baseline behavioral characteristics in the right hand side as we are concerned by the small size of the overall sample.

assigned to the treatment group, 0 otherwise. $YEAR$ is a dummy variable that takes the value 1 in 2011, 0 otherwise. The coefficient estimate, β_3 on the interaction term $TRAINING \times YEAR$ gives us the causal effect of being offered the *TRAINING* program on behavioral outcomes.

Table 9 reports the difference-in-difference estimates of assignment to *TRAINING* on the intrinsic characteristics (that is, the coefficients of β_3). As the regression results in Table 9 show, assignment to *TRAINING* has no significant effect on intrinsic characteristics confirming that the ITT effects of the program on labor market outcomes is not caused by changes in participants' intrinsic characteristics over time. We can therefore conclude that assignment to the program appears to have had no impact on the participants' intrinsic characteristics at least, in the short-run.

6.2 Certificate Effect

The second alternative 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, that is, is this simply a *certificate effect*?

To examine this we estimate the following equation:

$$Y_{it} = \beta_0 + \beta_1 Y_{i0} + \beta_2 ATTENDANCE_i + \sum_{j=1}^{K} \gamma_j \mathbf{X}_{ij} + \epsilon_{it} \tag{4}$$

We estimate equation (4) separately for $2010 - 2011$ and $2010 - 2012$ of the estimating panel sample $2010 - 2011 - 2012$ to capture both the short-run and medium-run TOT impacts of attendance. The coefficient estimate on attendance, β_2 gives the effect of increased attendance (increased intensity of training) in both the short-and-medium-run for the relevant subsamples. Since attendance is endogenous, once again we use assignment to the treatment status (*TRAINING*) and its interaction with age as instruments for *ATTENDANCE*.[15]

[15]The first-stage regression results are available upon request.

Comparing the implied impacts of increased intensity of training (*ATTENDANCE*) reported in columns (1) and (2) in Table 10 and program completion (*TRAINED*) presented in Table 6 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 monthly earnings. The average program completer attended 70 percent of total classes. For the average program completer, in the short-run the increase in monthly earning is Rs 256 (3.66×70). Compare this to the increase of Rs 261.39 for the *TRAINED* (see Table 6). So approximately 98 percent of the effect for the program completers is explained by intensity of training (and attendance). This figure is slightly lower in the medium-run at 90 percent. The results therefore suggest that it is attendance and skill accumulation that is driving the results and not the *certificate effect* or *signalling*.

7 Discussion

Youth underemployment, especially among less educated populations perpetuates poverty. The situation is particularly dire for women in low income households, the 2013 World Development notes, "Worldwide, fewer than half of women have jobs, compared with almost four-fifths of men" (WDR, 2013, pp 25). 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 potential 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, Duflo, Glennerster, and Kinnan, 2013). Using a field experiment in Sri Lanka de Mel, McKenzie, and Woodruff (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. Similar gender biased results are obtained by Fafchamps, McKenzie, Quinn, and Woodruff (2011) and Berge, Bjorvatn, and Tungodden (2011). 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 create successful small businesses. One additional advantage to 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 countries in several developing nations, the economic benefits from participating in vocational training programs is relatively unknown.

This paper makes three distinct contributions to the literature. First, we are not only able estimate the short-run intent-to-treat effects of participating in a vocational training program on labor market outcomes, measures of empowerment, entrepreneurship, and life satisfaction but are the first to capture the medium-run effects of having access to and completing a vocational training program. The program effects presented in this paper are extremely encouraging. We find that the program in a very short time has generated substantial improvement in labor market outcomes for these women. In approximately 6 months after program completion, women who were randomly offered the training program are almost six percentage points more likely to be employed and on an average work two and a half additional hours compared to those who were not offered the training. We also find that during the post-training period, women in the treatment group earn 150 percent more than women in the control group. The program impacts are much larger for women who completed the training program. The short-run effects that we obtain here are much larger than those observed in developed countries and are consistent with the rather small but growing literature on vocational training and labor market outcomes in developing countries. We also find that the short-run impact estimates of the labor market training program considered here continue to at least persist in the medium-run – and for almost all outcome variables the additional effect is positive. This result is of significant

importance from a policy stand-point especially when we want to invest in programs that can be beneficial in the long-run and do not fade over time.

Second, we pay substantial attention to barriers of program completion and not just take-up. High rates of program completion also makes programs more cost-effective. Programs where there are low rates of drop-out tend to have large number of program beneficiaries in the long-run compared to those that have high rates of drop-out. Vocational training programs face high rates of drop-outs and factors identifying drop-out behavior can be useful for improving completion rates. In our analysis, we are able to identify two significant barriers to program completion for women – distance to training center and lack of available child care support in the household. These barriers to program completion suggest that active labor market training policies in the future will need to accommodate these constraints to design more cost-effective labor market policies.

Finally, none of the existing papers emphasize on alternate "pathways" that can result in similar impact estimates. Our paper is successful in ruling out two possible pathways – changes in participants' behavior and signalling, both of which can affect treatment effects. We find that the program does not change participants' intrinsic characteristics such as preference for risk, competition and confidence ruling out behavioral changes resulting in treatment effects. We also rule out signalling as the primary driver of the labor market impacts. All this suggests that women who participate in the training program indeed gain through skill accumulation.

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, credit constraints, local access, and availability of child care support in the household 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 low-and-middle income countries in Asia and sub-Saharan Africa, which experience similar challenges in attaining economic growth, development and gender equality.

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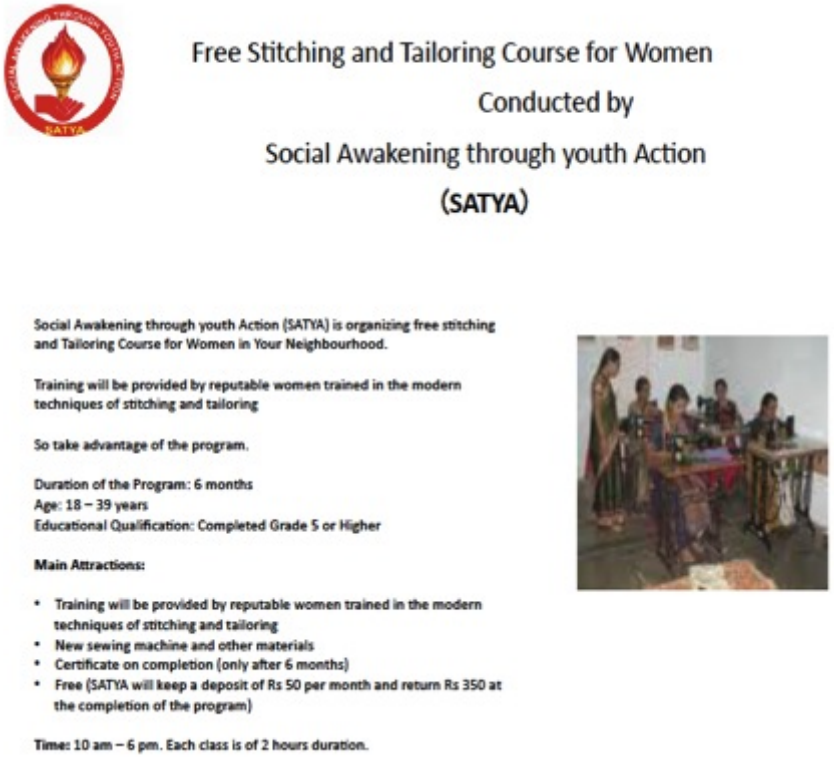
Figure 1: The Advertisement Campaign of the Program



Free Stitching and Tailoring Course for Women

Conducted by

Social Awakening through youth Action

(SATYA)

Social Awakening through youth Action (SATYA) is organizing free stitching and Tailoring Course for Women in Your Neighbourhood.

Training will be provided by reputable women trained in the modern techniques of stitching and tailoring

So take advantage of the program.

Duration of the Program: 6 months
Age: 18 – 39 years
Educational Qualification: Completed Grade 5 or Higher

Main Attractions:

- Training will be provided by reputable women trained in the modern techniques of stitching and tailoring
- New sewing machine and other materials
- Certificate on completion (only after 6 months)
- Free (SATYA will keep a deposit of Rs 50 per month and return Rs 350 at the completion of the program)

Time: 10 am – 6 pm. Each class is of 2 hours duration.

Figure 2: Chronology of Events

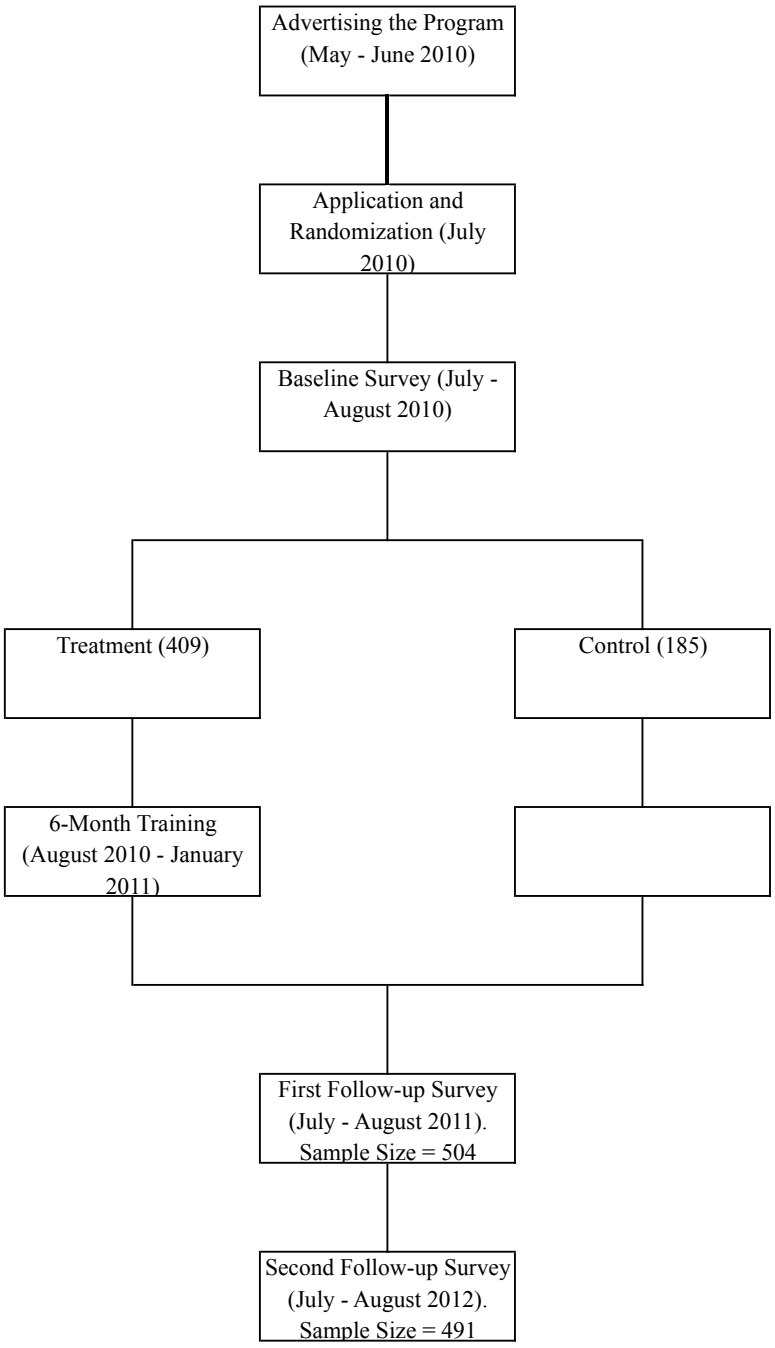


Figure 3: Average Monthly Attendance

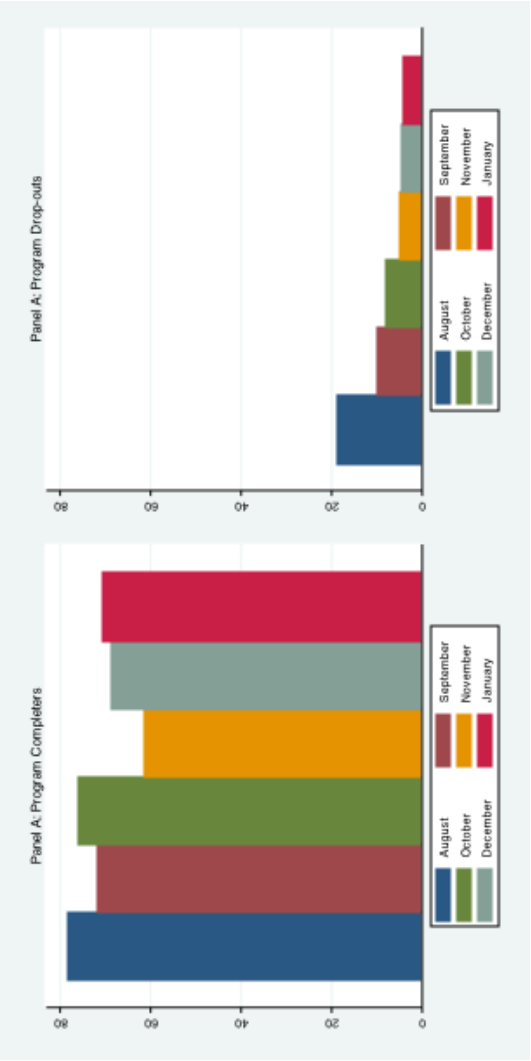


Figure 4: Walking time to training center and Program Completion

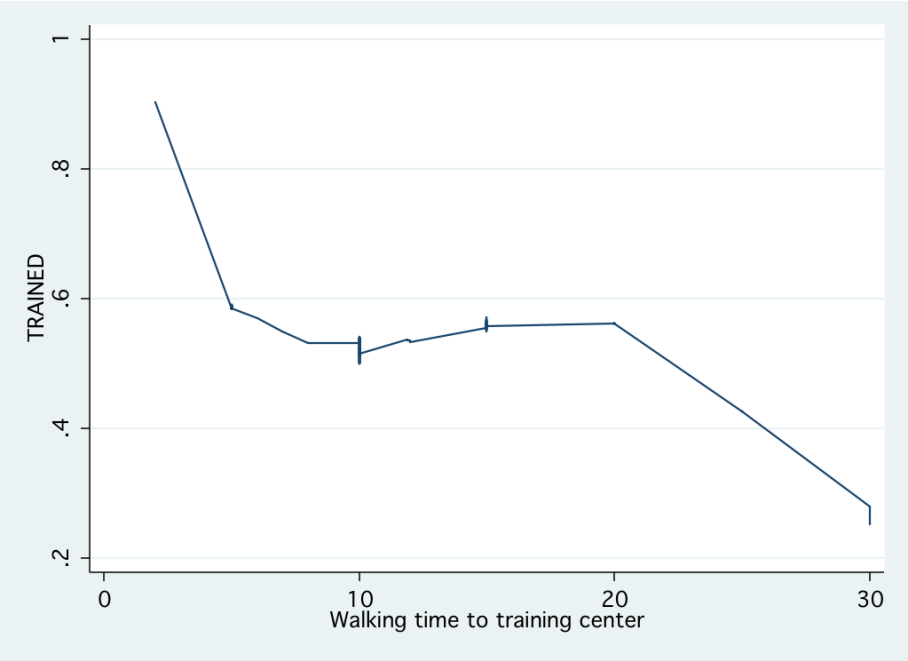


Table 1: Outcome variables included in the analysis

Panel A: Labor Market Outcomes	
Casual or Permanent employment:	= 1 if the respondent is employed for casual wage or is in permanent employment
Self employment:	= 1 if the respondent is self-employed
Any employment:	= 1 if the respondent is employed (casual, full-time, and or self)
Hours worked:	number of hours worked during the last week, where hours worked is a continuous variable
Monthly earnings†:	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
Rosca membership:	= 1 if the respondent is a member of a Rotating Savings and Credit Association (ROSCA)/chit fund
Happy at home:	A categorical variable taking the following four values: 4 if very satisfied; 3 if moderately satisfied; 2 if moderately dissatisfied; and 1 if not satisfied
Panel C: Behavioral/Intrinsic Characteristics	
Proportion allocated to the risky option	proportion allocated to the risky option in the investment game
Competitive wage scheme	= 1 if the woman choses the competition wage scheme in the competition game
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:
†: Monthly earnings is top coded at Rs 10, 000.

Table 2: Baseline Characteristics

	Full Sample (1)	Treatment (2)	Control (3)	Difference (4 = 2-3)
Panel A: Labor Market Outcomes				
Casual/Permanent employment	0.034	0.037	0.028	0.009
	[0.182]	[0.189]	[0.167]	(0.018)
Self employment	0.027	0.027	0.028	-0.001
	[0.163]	[0.162]	[0.167]	(0.016)
Any employment	0.052	0.054	0.050	0.004
	[0.223]	[0.226]	[0.218]	(0.022)
Hours worked	0.995	1.077	0.823	0.254
	[5.699]	[5.943]	[5.161]	(0.58)
Monthly earnings	45.68	52.19	30.40	22.50
	[297.70]	[337.67]	[187.17]	(30.44)
Hours worked (if any employment=1)	19.000	20.063	16.571	3.492
	[17.000]	[17.121]	(17.803)	(7.84)
Monthly earnings (If any employment=1)	872.00	985.53	612.5	373.03
	[1005.56]	[1130.16]	[633.65]	(459.25)
Panel B: Entrepreneurship, Empowerment and Life Satisfaction				
Own sewing machine	0.364	0.326	0.447	-0.121**
	[0.482]	[0.469]	[0.499]	(0.05)
Rosca membership	0.114	0.117	0.106	0.011
	[0.318]	[0.322]	[0.309]	(0.03)
Happy at home	3.419	3.413	3.433	-0.020
	[0.806]	[0.829]	[0.759]	(0.08)
Panel C: Socio-economic Characteristics				
Age	22.335	22.339	22.326	0.013
	[5.820]	[5.806]	[5.871]	(0.59)
Completed secondary schooling	0.449	0.450	0.447	0.003
	[0.498]	[0.498]	[0.499]	(0.05)
Experienced in stitching and tailoring	0.225	0.221	0.234	-0.013
	[0.418]	[0.415]	[0.424]	(0.04)
Married	0.330	0.332	0.326	-0.004
	[0.471]	[0.472]	[0.471]	(0.05)
SC	0.522	0.527	0.511	0.016
	[0.500]	[0.500]	[0.502]	(0.05)
Hindu	0.510	0.503	0.525	-0.022
	[0.500]	[0.501]	[0.501]	(0.051)
Dependency ratio	0.250	0.256	0.238	0.018
	[0.476]	[0.489]	[0.451]	(0.05)
F-test of joint significance				1.43
$(p - value)$				(0.13)
Sample size	439	298	141	

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

Table 3: 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 4: Are Attriting Women Different?

	Casual/ Permanent employment (1)	Self employment (2)	Any employment (3)	Hours worked (4)	Monthly earnings (5)	Own sewing machine (6)	ROSCA membership (7)	Happy at Home (8)
Treatment	0.011	0.0002	0.008	0.301	24.201	-0.111**	0.015	-0.026
	(0.018)	(0.017)	(0.023)	(0.575)	(26.248)	(0.046)	(0.032)	(0.079)
Attrite	-0.010	0.071	-0.024	-3.582	-244.614	-0.446	0.235	-0.190
	(0.150)	(0.082)	(0.163)	(4.950)	(340.172)	(0.295)	(0.180)	(0.574)
Attrite ×	-0.014	0.012	-0.003	0.018	0.627	-0.031	-0.027	0.102
Treatment	(0.034)	(0.020)	(0.037)	(0.878)	(53.052)	(0.095)	(0.066)	(0.159)
Experienced	0.036	0.043	0.049	0.733	38.284	0.476***	0.165***	-0.051
	(0.028)	(0.026)	(0.035)	(0.742)	(34.946)	(0.057)	(0.047)	(0.099)
Age	0.004**	0.004**	0.006**	0.110*	5.459*	-0.003	0.003	-0.002
	(0.002)	(0.002)	(0.003)	(0.061)	(2.979)	(0.006)	(0.004)	(0.010)
Secondary	0.026	-0.012	0.027	0.798	46.873*	0.026	0.074**	0.205***
school	(0.017)	(0.014)	(0.021)	(0.544)	(27.704)	(0.043)	(0.031)	(0.079)
Married	-0.034	-0.069***	-0.077**	-0.863	-60.045	0.073	-0.038	0.133
	(0.026)	(0.026)	(0.033)	(0.834)	(48.894)	(0.080)	(0.042)	(0.134)
Hindu	0.035	0.013	0.044	1.404	112.200	0.038	0.044	-0.339***
	(0.027)	(0.019)	(0.032)	(1.183)	(87.049)	(0.063)	(0.044)	(0.128)
SC	-0.010	0.013	-0.003	-0.726	-44.249	-0.044	0.045	0.005
	(0.018)	(0.015)	(0.022)	(0.582)	(30.563)	(0.044)	(0.030)	(0.079)
Dependency ratio	0.019	0.018	0.024	0.538	47.937	-0.028	-0.024	-0.080
	(0.025)	(0.022)	(0.026)	(0.788)	(44.654)	(0.054)	(0.029)	(0.097)
Attrite ×	-0.002	-0.007**	-0.005	0.059	3.810	0.015	-0.009	-0.023
age	(0.005)	(0.003)	(0.006)	(0.182)	(9.413)	(0.012)	(0.007)	(0.022)
Attrite ×	0.024	-0.007	0.013	1.213	113.041	0.101	-0.022	0.091
Secondary Schooling	(0.037)	(0.020)	(0.040)	(1.318)	(116.193)	(0.081)	(0.062)	(0.152)
Attrite ×	0.087	0.081**	0.118	2.090	221.348	-0.108	0.065	0.091
married	(0.070)	(0.036)	(0.073)	(2.147)	(170.575)	(0.149)	(0.096)	(0.253)
Attrite ×	0.020	0.045	0.067	-0.114	-101.027	0.003	0.041	0.274
hindu	(0.066)	(0.062)	(0.089)	(2.067)	(116.644)	(0.141)	(0.099)	(0.253)
Attrite ×	0.021	-0.012	0.027	1.478	152.558	0.156*	0.007	-0.072
SC	(0.035)	(0.021)	(0.038)	(1.323)	(122.457)	(0.085)	(0.062)	(0.165)
Attrite ×	0.035	-0.010	0.018	1.938	257.276	0.087	0.033	0.359**
experienced	(0.074)	(0.043)	(0.078)	(2.950)	(289.666)	(0.110)	(0.107)	(0.179)
Attrite ×	-0.057	-0.009	-0.065	-2.136	-193.185	-0.132	0.033	0.087
dependency ratio	(0.040)	(0.025)	(0.042)	(1.388)	(119.416)	(0.102)	(0.053)	(0.159)
Constant	-0.114*	-0.073	-0.151**	-3.008	-203.004	0.285**	-0.132	3.747***
	(0.058)	(0.046)	(0.069)	(1.964)	(125.870)	(0.143)	(0.094)	(0.241)
F-test of joint significance	0.38	1.22	0.96	0.43	0.40	0.83	0.41	1.11
$(p - value)$	(0.94)	(0.27)	(0.48)	(0.93)	(0.94)	(0.60)	(0.93)	(0.35)
Sample size	594	594	594	594	594	594	594	594

Notes:
F-test on the joint significance of the attrition dummy and all its interaction terms are appended at the end of the table. Regressions include region of residence and center dummies. Robust standard errors in parentheses. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$. Experienced denotes prior experience in stitching and tailoring (self reported).

Table 5: ITT effects of *TRAINING*

	Short-run Effect (1)	Mean Control (2011) (2)	Medium-run Effect (3)	Mean Control (2012) (4)
Panel A: Labor Market Outcomes				
Casual/Permanent employment	0.052**	0.05	0.064**	0.08
	(0.02)		(0.03)	
Self employment	0.039**	0.014	0.039**	0.014
	(0.016)		(0.016)	
Any employment	0.06**	0.063	0.081**	0.099
	(0.03)		(0.03)	
Hours worked	2.44***	1.12	3.41***	3.30
	(0.80)		(1.31)	
Monthly earnings	148.35**	100	241.80**	251.77
	(75.14)		(115.73)	
Panel B: Entrepreneurship, Empowerment and Life Satisfaction				
Own sewing machine	0.08	0.53	0.13***	0.59
	(0.05)		(0.05)	
Rosca membership	-0.0008	0.042	0.0002	0.085
	(0.021)		(0.03)	
Happy at home	-0.08	3.34	-0.09	3.33
	(0.07)		(0.06)	
Panel C: Index Measures				
All labor index	0.33***		0.27**	
	(0.12)		(0.11)	
Self index	0.26***		0.30***	
	(0.09)		(0.09)	
All other outcome index	-0.06		-0.07	
	(0.057)		(0.06)	
Alternative all labor index	0.29***		0.27***	
	(0.09)		(0.08)	
Overall index	0.17***		0.16**	
	(0.05)		(0.05)	
Sample size		878		

Notes:
Regressions control for a full set of pre-intervention socio-economic 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 includes all women who have been surveyed in all 3 rounds (2010-2011-2012 sample). Robust standard errors in parentheses. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$.

Table 6: Effect of Program Completion

	Satya/Pratham Program			Any Program		
	Short-run Effect	Medium-run Effect	Hansen J-statistic	Short-run Effect	Medium-run Effect	Hansen J-statistic
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Labor Market Outcomes						
Casual/Permanent employment	0.093**	0.114**	0.03	0.10**	0.121**	0.056
	(0.043)	(0.054)	[0.86]	(0.04)	(0.06)	[0.81]
Self employment	0.068**	0.068**	1.80	0.072**	0.072**	1.97
	(0.03)	(0.03)	[0.18]	(0.03)	(0.03)	[0.16]
Any employment	0.107**	0.145**	0.006	0.11**	0.15**	0.023
	(0.048)	(0.059)	[0.94]	(0.05)	(0.063)	[0.88]
Hours worked	4.22***	5.95***	3.10*	4.38***	6.24**	3.25*
	(1.43)	(2.31)	[0.08]	(1.52)	(2.48)	[0.08]
Monthly earnings	261.39**	428.16**	0.39	273.50**	452.07**	0.47
	(132.13)	(204.45)	[0.53]	(139.25)	(218.65)	[0.49]
Panel B: Entrepreneurship, Empowerment and Life Satisfaction						
Own sewing machine	0.15*	0.233***	1.22	0.16*	0.25***	1.07
	(0.087)	(0.087)	[0.27]	(0.09)	(0.09)	[0.30]
Rosca membership	-0.0009	0.0009	0.06	-0.0008	0.0012	0.06
	(0.037)	(0.05)	[0.80]	(0.039)	(0.05)	[0.80]
Happy at home	-0.136	-0.164	0.81	-0.14	-0.17	0.87
	(0.12)	(0.12)	[0.37]	(0.130)	(0.127)	[0.35]
Sample size		878			878	

Notes:
Regressions control for a full set of pre-intervention socio-economic 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 are Excluded instruments are *Treatment*, *Treatment* $\times$ *Age* and *Treatment* $\times$ *Year*. Sample includes all women who have been surveyed in all 3 rounds. Robust standard errors in round brackets and p-values for the Hansen-J statistic is reported in square brackets. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.
† : The Kleibergen-Paap rk Wald F statistic is the same across the different outcome variables of interest as the only source of variation comes from the lagged outcome variable, which has little explanatory power.

Table 7: 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 status. 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 in only column 1. Robust standard errors in parentheses. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$.

Table 8: Baseline Characteristics: Behavioral Sample

	Full Sample (1)	Treatment (2)	Control (3)	Difference (4 = 2 - 3)
Panel A: Intrinsic Characteristics				
Proportion allocated to the	52.308	53.077	50.769	2.308
risky option	[19.973]	[20.908]	[18.120]	(3.92)
Competitive wage	0.393	0.410	0.359	0.051
scheme	[0.491]	[0.495]	[0.486]	(0.096)
Self Assessment	4.435	4.376	4.551	-0.175
	[2.079]	[1.878]	[2.454]	(0.409)
Relative Rank	4.077	4.206	3.821	0.385**
	[0.984]	[0.888]	[1.121]	(0.19)
Panel B: Socio-economic Characteristics				
Age	23.727	24.333	22.513	1.821
	[5.998]	[6.057]	[5.762]	(1.16)
Completed secondary schooling	0.462	0.436	0.513	-0.077
	[0.501]	[0.499]	[0.506]	(0.098)
Experienced in stitching and tailoring	0.521	0.525	0.512	0.013
	[0.502]	[0.502]	[0.505]	(0.098)
Married	0.470	0.526	0.359	0.167*
	[0.501]	[0.503]	[0.486]	(0.09)
SC	0.615	0.615	0.615	0.000
	[0.489]	[0.490]	[0.493]	(0.096)
Hindu	0.974	0.974	0.974	0.000
	[0.159]	[0.159]	[0.160]	(0.031)
Dependency ratio	0.359	0.376	0.325	0.051
	[0.591]	[0.632]	[0.506]	(0.11)
F-test of joint significance†				0.83
$(p - value)$				[0.61]
Sample size	117	78	39	

Notes:
Standard deviation reported in square brackets and standard error reported in round brackets. Sample Includes all women who were surveyed in 2010 and 2011. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$

Table 9: Effect of *TRAINING* on Intrinsic Characteristics

	ITT Estimates (1)	Mean Control (2011) (2)
Proportion allocated to the risky option	-8.769	54.56
	(5.942)	
Competitive wage scheme	0.064	0.38
	(0.131)	
Self assessment	0.611	4.10
	(0.595)	
Relative rank	0.115	3.51
	(0.307)	
Sample size		234

Notes:
Column (1) presents the ITT estimates of *TRAINING* on intrinsic characteristics. Sample includes women who participated in the behavioral experiments and were surveyed both in 2010 and 2011. Regressions control for a full set of pre-intervention socio-economic characteristics (age, completed secondary schooling, married, experienced in stitching and tailoring, hindu, SC, dependency ratio), region of residence and center dummies. Robust standard errors in parentheses. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$.

Table 10: Certificate Effect?

	Short-run effect (1)	Medium-run effect (2)
Panel A: Labor Market Outcomes		
Casual/permanent employment	0.0011**	0.0015**
	(0.0005)	(0.0007)
Self employment	0.0009**	0.0009***
	(0.0003)	(0.0003)
Any employment	0.0013**	0.0019**
	(0.0006)	(0.0007)
Hours worked	0.056***	0.081***
	(0.019)	(0.03)
Monthly earnings	3.66**	5.45**
	(1.77)	(2.64)
Panel B: Entrepreneurship, Empowerment, and Life Satisfaction		
Own sewing machine	0.002*	0.0028**
	(0.001)	(0.0011)
Rosca membership	0.00004	-0.00006
	(0.0005)	(0.0006)
Happy at home	-0.0018	-0.0022
	(0.0015)	(0.001)
Sample size	439	

Notes:
$2010 - 2011 - 2012$ sample used.
ATTENDANCE is instrumented with *Treatment* and *Treatment* $\times$ *Age*. Regressions control for a full set of pre-intervention socio-economic 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 in parentheses. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$.

Table A-1: Baseline Characteristics (Sub samples)

	Full Sample	Treatment	Control	Difference	Full Sample	Treatment	Control	Difference
		2010-2011 Sample				2010-2012 Sample		
	(1)	(2)	(3)	(4 = 2 - 3)	(5)	(6)	(7)	(8 = 5 - 6)
Panel A: Labor Market Outcomes								
Casual/permanent wage employment	0.037	0.043	0.025	0.018	0.032	0.033	0.032	0.001
	[0.19]	[0.20]	[0.15]	(0.018)	[0.177]	[0.178]	[0.176]	(0.017)
Self-employment	0.025	0.026	0.025	0.001	0.024	0.023	0.025	-0.002
	[0.16]	[0.16]	[0.15]	(0.015)	[0.15]	[0.15]	[0.15]	(0.014)
Any employment	0.053	0.057	0.044	0.013	0.048	0.047	0.051	0.04
	[0.22]	[0.23]	[0.20]	(0.021)	[0.21]	[0.21]	[0.22]	(0.02)
Hours worked	1.12	1.31	0.73	0.58	0.93	0.95	0.85	0.10
	[6.15]	[6.66]	[4.86]	(0.59)	[5.45]	[5.61]	[5.10]	(0.53)
Conditional hours worked	21.07	22.65	16.57	6.08	19	20.06	16.75	3.31
	[17.22]	[17.20]	[17.80]	(7.61)	[7.32]	[17.12]	[16.49]	(7.32)
Monthly earnings	56.59	70.25	26.96	43.28	35.76	38.38	30.12	8.25
	[368.51]	[428.58]	[176.45]	(35.30)	[274.57]	[306.54]	[189.19]	(26.63)
Conditional monthly earnings	1056.50	1211.90	612.5	599.40	731.66	803.75	587.5	216.25
	[1237.05]	[1367.19]	[633.64]	(540.88)	[1036.75]	[1198.42]	[646.83]	(456.69)
Panel B: Entrepreneurship, Empowerment, and Life Satisfaction								
Own sewing machine	0.36	0.32	0.43	-0.10**	0.35	0.31	0.44	-0.13***
	[0.48]	[0.46]	[0.49]	(0.04)	[0.47]	[0.46]	[0.49]	(0.04)
Rosca participation	0.113	0.115	0.106	0.009	0.116	0.11	0.10	0.01
	[0.31]	[0.32]	[0.31]	(0.03)	[0.32]	[0.32]	[0.31]	(0.03)
Happy at home	3.43	3.437	3.434	0.003	3.42	3.41	3.43	0.02
	[0.80]	[0.82]	[0.75]	(0.076)	[0.79]	[0.82]	[0.74]	(0.07)

Continued . . .

Table A-1 Continued: Baseline Characteristics (Sub samples)

	Full Sample	Treatment	Control	Difference	Full Sample	Treatment	Control	Difference
		2010-2011 Sample				2010-2012 Sample		
	(1)	(2)	(3)	(4 = 2 - 3)	(5)	(6)	(7)	(8 = 5 - 6)
Panel C: Socioeconomic Characteristics								
Age	22.24	22.33	22.04	0.29	22.28	22.27	22.31	0.04
	[5.66]	[5.78]	[5.77]	(0.54)	[5.80]	[5.80]	[5.83]	(0.56)
Completed secondary schooling	0.44	0.449	0.433	0.016	0.45	0.44	0.46	-0.02
	[0.49]	[0.49]	[0.49]	(0.047)	[0.50]	[0.49]	[0.50]	(0.048)
Experience in stitching/tailoring	0.216	0.217	0.213	0.003	0.213	0.211	0.217	-0.006
	[0.45]	[0.43]	[0.48]	(0.04)	[0.49]	[0.42]	[0.48]	(0.04)
Married	0.335	0.34	0.31	0.03	0.323	0.322	0.326	0.004
	[0.47]	[0.47]	[0.46]	(0.04)	[0.47]	[0.46]	[0.47]	(0.04)
SC	0.51	0.52	0.49	0.02	0.521	0.522	0.519	0.003
	[0.40]	[0.50]	[0.50]	(0.04)	[0.40]	[0.50]	[0.50]	(0.05)
Hindu	0.494	0.498	0.484	0.014	0.49	0.48	0.52	0.04
	[0.50]	[0.50]	[0.50]	(0.048)	[0.49]	[0.49]	[0.50]	(0.05)
Dependency ratio	0.257	0.262	0.248	0.014	0.25	0.26	0.23	0.03
	[0.48]	[0.49]	[0.45]	(0.04)	[0.48]	[0.49]	[0.45]	(0.04)
F-test of joint significance†				1.34				1.34
$(p - value)$				(0.18)				(0.18)
F-test of joint significance††				1.78				1.58
$(p - value)$				(0.03)				(0.08)
Sample size	504	345	159		491	335	156	

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

Table A-2: ITT effects of *TRAINING*. Regressions using different subsamples.

	Short-run Effects: 2010-2011 sample		Medium-run Effects: 2010-2012 sample	
	Coefficient	Mean Control (2011)	Coefficient	Mean Control (2012)
	(1)	(2)	(3)	(4)
Panel A: Labor Market Outcomes				
Casual/Permanent employment	0.043*	0.05	0.072**	0.076
	(0.022)		(0.028)	
Self employment	0.045***	0.012	0.041***	0.013
	(0.015)		(0.015)	
Any employment	0.057**	0.06	0.09***	0.096
	(0.02)		(0.03)	
Hours worked	2.06***	1.17	3.56***	3.25
	(0.74)		(1.25)	
Monthly earnings	149.22**	95.84	210.10*	291.66
	(67.44)		(122.7)	
Panel B: Entrepreneurship, Empowerment and Life Satisfaction				
Own sewing machine	0.07	0.54	0.12***	0.59
	(0.046)		(0.04)	
Rosca membership	0.010	0.037	0.003	0.076
	(0.019)		(0.025)	
Happy at home	-0.072	3.35	-0.06	3.30
	(0.06)		(0.06)	
Sample size	504		491	

Notes:
Regressions control for a full set of pre-intervention socio-economic 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 in parentheses. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$.

Table A-3: First-Stage Regression Results for Satya/Pratham Program: Short-run Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.676***	0.689***	0.678***	0.677***	0.677***	0.661***	0.673***	0.679***
	(0.085)	(0.085)	(0.085)	(0.085)	(0.085)	(0.087)	(0.085)	(0.086)
Treatment × *Year*	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)
Treatment × *Age*	-0.005	-0.006	-0.005	-0.005	-0.005	-0.005	-0.005	-0.005
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Year	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.012)	(0.013)	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)	(0.012)
Age	-0.002	-0.003	-0.003	-0.002	-0.002	-0.002	-0.002	-0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Completed secondary schooling	0.166***	0.170***	0.164***	0.166***	0.165***	0.168***	0.161***	0.160***
	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
Married	0.048	0.059	0.054	0.048	0.049	0.048	0.049	0.042
	(0.047)	(0.048)	(0.048)	(0.047)	(0.048)	(0.047)	(0.048)	(0.047)
Hindu	-0.043	-0.043	-0.045	-0.042	-0.045	-0.040	-0.044	-0.029
	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)
SC	-0.016	-0.018	-0.016	-0.015	-0.015	-0.018	-0.020	-0.016
	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
Experienced in stitching and tailoring	0.101***	0.095***	0.098***	0.102***	0.102***	0.116***	0.090**	0.105***
	(0.035)	(0.035)	(0.035)	(0.035)	(0.035)	(0.038)	(0.036)	(0.035)
Dependency ratio	0.035	0.033	0.033	0.035	0.034	0.035	0.038	0.039
	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)	(0.032)
Constant	-0.145*	-0.147*	-0.138*	-0.148*	-0.145*	-0.134*	-0.140*	-0.278***
	(0.080)	(0.079)	(0.080)	(0.080)	(0.080)	(0.080)	(0.080)	(0.105)
Sample size	878	878	878	878	878	878	878	878
F-statistic on the excluded instruments†	245.44	245.58	245.37	245.49	244.41	234.37	242.17	248.06

Notes:
Dependent variable *TRAINED*. Regressions also include region of residence, center dummies and lagged outcome variables. Columns (1) - (8) present the first-stage regressions correspond to the short-run effects presented in column 1, Table 6. Robust standard errors in parenthesis. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$. Estimation conducted on the 2010-2011-2012 sample.
†: Excluded instruments are *Treatment*, *Treatment* × *Age* and *Treatment* × *Year*.

Table A-4: First-Stage Regression Results for Satya/Pratham Program: Medium-run Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.058	0.064	0.059	0.058	0.058	0.050	0.057	0.059
	(0.058)	(0.058)	(0.058)	(0.058)	(0.058)	(0.060)	(0.058)	(0.058)
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
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
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 schooling	0.083***	0.085***	0.082***	0.083***	0.083***	0.084***	0.080***	0.080***
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
Married	0.024	0.030	0.027	0.024	0.024	0.024	0.025	0.021
	(0.035)	(0.035)	(0.035)	(0.035)	(0.035)	(0.035)	(0.035)	(0.035)
Hindu	-0.021	-0.021	-0.022	-0.021	-0.022	-0.020	-0.022	-0.014
	(0.029)	(0.029)	(0.030)	(0.030)	(0.030)	(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)
Experienced in stitching and tailoring	0.050**	0.048*	0.049*	0.051**	0.051**	0.058**	0.045*	0.052**
	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.028)	(0.026)	(0.026)
Dependency ratio	0.017	0.016	0.017	0.018	0.017	0.017	0.019	0.019
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)
Constant	-0.072	-0.073	-0.069	-0.074	-0.072	-0.067	-0.070	-0.139*
	(0.056)	(0.055)	(0.056)	(0.055)	(0.056)	(0.056)	(0.056)	(0.075)
Sample size	878	878	878	878	878	878	878	878
F-statistic on the excluded instruments†	129.28	129.73	129.41	129.29	129.10	127.94	129.03	130.03

Notes:
Dependent variable *TRAINED*. Regressions also include region of residence, center dummies and lagged outcome variables. Columns (1) - (8) present the first-stage regressions correspond to the short-run effects presented in column 2, Table 6. Robust standard errors in parenthesis. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$. 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 Any Program: Short-run Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.690***	0.694***	0.691***	0.692***	0.691***	0.679***	0.688***	0.691***
	(0.121)	(0.122)	(0.121)	(0.121)	(0.121)	(0.125)	(0.121)	(0.121)
Treatment × *Year*	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.056)	(0.056)	(0.056)	(0.056)	(0.056)	(0.056)	(0.056)	(0.056)
Treatment × *Age*	-0.007	-0.007	-0.007	-0.007	-0.007	-0.007	-0.007	-0.007
	(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
	(0.041)	(0.042)	(0.041)	(0.041)	(0.041)	(0.041)	(0.042)	(0.042)
Age	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Completed secondary schooling	0.143***	0.146***	0.143***	0.143***	0.142***	0.145***	0.140***	0.143***
	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.030)	(0.030)
Married	0.062	0.063	0.065	0.062	0.063	0.061	0.061	0.058
	(0.052)	(0.053)	(0.053)	(0.052)	(0.052)	(0.052)	(0.052)	(0.052)
Hindu	-0.038	-0.036	-0.038	-0.039	-0.041	-0.035	-0.038	-0.031
	(0.046)	(0.046)	(0.046)	(0.046)	(0.047)	(0.046)	(0.046)	(0.046)
SC	-0.014	-0.015	-0.014	-0.012	-0.012	-0.016	-0.017	-0.015
	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)
Experienced in stitching and tailoring	0.116***	0.116***	0.115***	0.117***	0.117***	0.127***	0.109***	0.119***
	(0.040)	(0.040)	(0.040)	(0.040)	(0.040)	(0.043)	(0.040)	(0.040)
Dependency ratio	0.008	0.009	0.008	0.009	0.007	0.009	0.011	0.011
	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)
Constant	-0.089	-0.096	-0.089	-0.091	-0.088	-0.085	-0.089	-0.138
	(0.126)	(0.125)	(0.126)	(0.126)	(0.126)	(0.128)	(0.126)	(0.141)
Sample size	878	878	878	878	878	878	878	878
F-statistic on the excluded instruments†	119.70	119.82	119.97	119.91	119.21	114.97	119.32	120.13

Notes:
Dependent variable *TRAINED*: completed *any* program. Regressions also include region of residence and lagged outcome variables. Columns (1) - (8) present the first-stage regressions correspond to the short-run effects presented in column 4, Table 6. Robust standard errors in parenthesis. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$. 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: Medium-run Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.083	0.085	0.084	0.084	0.084	0.078	0.082	0.084
	(0.082)	(0.082)	(0.082)	(0.082)	(0.082)	(0.084)	(0.082)	(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***
	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)
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***	0.071***
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	0.029
	(0.038)	(0.039)	(0.038)	(0.038)	(0.038)	(0.038)	(0.038)	(0.038)
Hindu	-0.019	-0.018	-0.019	-0.019	-0.021	-0.017	-0.019	-0.016
	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)
SC	-0.007	-0.008	-0.007	-0.006	-0.006	-0.008	-0.009	-0.007
	(0.022)	(0.022)	(0.022)	(0.021)	(0.022)	(0.022)	(0.022)	(0.022)
Experienced in stitching	0.058**	0.058**	0.058**	0.058**	0.058**	0.064**	0.055*	0.060**
and tailoring	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.031)	(0.029)	(0.029)
Dependency ratio	0.004	0.005	0.004	0.004	0.004	0.005	0.006	0.005
	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)
Constant	-0.108	-0.112	-0.108	-0.109	-0.108	-0.107	-0.109	-0.133
	(0.086)	(0.085)	(0.086)	(0.085)	(0.086)	(0.087)	(0.085)	(0.097)
Sample size	878	878	878	878	878	878	878	878
F-statistic on the excluded instruments†	60.50	60.54	60.58	60.57	60.40	59.58	60.45	60.61

Notes:
Dependent variable *TRAINED*: completed *any* program. Regressions also include region of residence and lagged outcome variables. Columns (1) - (8) present the first-stage regressions correspond to the short-run effects presented in column 5, Table 6. Robust standard errors in parenthesis. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$. Estimation conducted on the 2010-2011-2012 sample.
†: Excluded instruments are *Treatment*, *Treatment* × *Age* and *Treatment* × *Year*.