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Author
Das, Narayan Chandra

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Essays on Labor and Credit Markets in Bangladesh

by

Narayan Chandra Das

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Committee in charge:

Professor Alain de Janvry, Chair
Professor Elisabeth Sadoulet
Professor Paul Gertler

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Abstract

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Rising youth unemployment is a key concern for many poor countries, where school dropout rates at the primary or secondary level are very high. The relationship between skills training and youth employment is thus a critical question of interest to economists. In the first chapter of this dissertation, I examine this relationship in the context of Bangladesh, a lower-middle income country. The relationship between credit and agricultural development is also an important question of interest to economists, as agricultural development is directly linked with food security and poverty. The theoretical literature shows that credit can positively affect agricultural development through, among others, decreasing sharecropping relative to fixed rental contracts. The second chapter of this dissertation empirically tests this proposition. The third chapter of this dissertation examines whether providing potential migrants with information along with administrative and community support reduces the risk of international migration.

In the first chapter, I estimate the effects of a youth training program in Bangladesh on labor market outcomes. The program provides on-the-job and classroom training to disadvantaged and unemployed youths. On-the-job training is provided through apprenticeship under a local master crafts person. Classroom training curriculum includes theoretical training on specific trades as well as soft-skills training. The program is implemented by BRAC, the largest NGO in the world. Using the data generated by BRAC's internal research unit, I show that six months after the intervention, on-the-job training increases labor market participation of program participants by 22.6 percentage points, total time devoted to earning activities by 59%, and earnings by 44%. It increases both self- and wage employment. The effect on employment is found to be larger for females. Additional effects of classroom training over on-the-job training on overall employment and earnings are small in magnitude. Results, however, indicate that if classroom training is added to on-the-job training, the effects shift from self- to wage employment. Results also show that employment in firms where the apprenticeship took place is a channel for the effect on wage employment. The benefit-cost ratio for on-the-job training is estimated to be 6.34, demonstrating high returns of the investment made under this initiative. I also show that, at the scale at which the program was implemented, employment effects for beneficiaries were not achieved through displacement of non-beneficiaries.

Choice of a land rental contract has figured prominently in the theory of industrial organization. While a share contract is inefficient in a first-best world, it may be the preferred option under second-best conditions. Theory has thus predicted the existence of sharecropping as the potentially preferred contract under liquidity constraint, insurance market failure, and market failure for non-contractible inputs not owned by the tenant. Rigorous empirical evidence is, however, still lacking on this basic tenet of theory. In the second chapter, co-authored with Alain de Janvry and Elisabeth Sadoulet, we study a randomized experiment in a credit program for landless workers and marginal farmers organized by BRAC in Bangladesh to show that access to credit has a large positive effect on the choice of fixed rent over share rent contract, both in terms of number of contracts and area contracted. As predicted by theory, the magnitude of this shift is enhanced when the tenant is less exposed to risk. However, we do not find any conclusive evidence that the shift is increased by differential possession of non-contractible farm management experience. Our results suggest that development programs that give access to credit to potential tenants can help them move away from inefficient land rental contracts.

In the third chapter, coauthored with Alain de Janvry and Elisabeth Sadoulet, we investigate whether providing information along with administrative and community support to aspiring migrants reduces the risk of migration. The risks we focus on include: (i) the risk of failure to depart the home country after having spent resources during the migration process; and (ii) the risk of not finding a job abroad with the expected salary. BRAC implemented the intervention in Bangladesh. It recruited community volunteers to implement the intervention. We randomized the intervention at the union level - the lowest administrative unit in the country. Results show that the intervention has no statistically significant effect on the overall migration success or on migration failure. Similarly, there is no significant effect on the salary received abroad. However, in areas where poverty rates are relatively high, the intervention reduces migration failure. The program also reduces the cost of failure, as well as migration costs among individuals who attempt to migrate through informal channels. Additional results suggest that the program is more likely to decrease migration failure in areas where the levels of education of volunteers are higher. We also find that treatment effect on migration success is larger in areas where the proportion of volunteers who are returning migrants or migrant family members is higher. These results suggest that the program might be more effective if it engages more educated individuals and/or more returnee migrants/migrant family members as volunteers.

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Chapter 1

Training the Disadvantaged Youth and Labor Market Outcomes: Evidence from Bangladesh

1.1 Introduction

Lack of skills is considered to be one of the main determinants of unemployment and poverty. As a consequence, programs that reduce the cost of education have been key policies for poor countries (Attanasio *et al.*, 2011). Despite these policies, school dropout rates in developing countries are very high. In South and West Asia, for example, 26% of adolescents of lower secondary age were out of school in 2013 (UNESCO, 2010).[1] The majority of these young school dropouts end up unemployed or in low quality jobs that offer limited socio-economic opportunities.[2] Training can be a potential solution to address the problem of rising youth unemployment in poor countries.

A large amount of literature examines the impacts of training programs (consisting in apprenticeship or classroom vocational training or a combination of both) in developing countries.[3] McKenzie (2017) reviews the results from 12 program evaluations in 8 developing countries.[4] Two of these studies focus on apprenticeship under a master crafts person and the other 10 on classroom training with/without internship. He finds that only 3 studies show a statistically significant impact on employment and 2 studies on earnings.[5] While the review shows modest effects of training programs in developing countries, two recent papers that were not covered by McKenzie (2017) document quite large effects of apprenticeship or classroom training (Alfonsi *et al.*, 2017; Hardy and McCasland, 2015). Specifically, Alfonsi *et al.* (2017) find that apprenticeship training increases employment by 7 percentage points and classroom vocational training by 11 percentage points.[6] Hardy and McCasland (2015) find that each apprentice placement in small firms in Ghana increases firm size by 0.5 workers after six months.

[1] Globally, the rate is 17.3%; for Bangladesh and India, it is around 22%.

[2] In Bangladesh, the context of this study, 10% of youth aged 15-19 years are unemployed (BBS, 2017).

[3] Some of these programs provide internship instead of apprenticeship.

[4] Studies reviewed by McKenzie (2017) include Hirshleifer *et al.* (2016), Alzúa *et al.* (2016), Attanasio *et al.* (2011, 2015), Card *et al.* (2011), Ibarrarán *et al.* (2014), Ibarrarán *et al.* (2015), Maitra and Mani (2016), Honorati (2015), Cho *et al.* (2013), and Diaz and Rosas (2016).

[5] With regard to impact heterogeneity, among the studies reviewed, Attanasio *et al.* (2011, 2015) find significant impacts on employment for women but not for men, but they do not formally test for difference in impact by gender. McKenzie (2017) also show that studies that formally test for equality by gender can either not reject that impacts are similar for men and women, or find significantly higher impacts for men.

[6] These are average effects for three post-intervention survey waves conducted 1, 2 and 3 years after training completion. Short run effect (after one year) is similar. They, however, show that the effect of apprenticeship training on treatment individuals is likely to be achieved through displacement of non-participants.

Acevedo *et al.* (2017) examine whether higher expectation of training participants is a reason for the mixed results of training programs. They find that, after training, employment expectation of male participants increases while they learn little from the program. They eventually end up unemployed as their expectations are not met by the labor market. For females, on the other hand, they find that training leads to an increase in both their skills and expectations, and expectations are met by the labor market. In effect, females experience positive impacts on employment, at least in the short-run. Another reason for training to achieve low results is that labor markets in developing countries, particularly in urban areas, work better; firms are able to fill vacancies quickly, and workers turn down many job opportunities and quit jobs frequently in pursuit of better opportunities (McKenzie, 2017). However, studying apprenticeship placement in small firms in Ghana (with placement randomized at the firm level), Hardy and McCasland (2015) find that it significantly increases firms' profit and employment. Their findings are thus unlikely to support the observation by McKenzie (2017) that firms in developing countries are able to fill vacancies quickly.

In general, existing evidence on the effectiveness of training programs in developing countries is mixed, with large effects documented by Hardy and McCasland (2015) and Alfonsi *et al.,* (2017), and modest or no effects documented by the studies reviewed in McKenzie (2017). Furthermore, although training programs typically combine different skills training (classroom vocational training, soft skills, and apprenticeship), evidence on the additional effect of one component over another is scarce.[7]

In this paper, I study a training program that provides on-the-job (apprenticeship) and classroom training to disadvantaged and unemployed/under-employed youth in Bangladesh, a lower-middle income country with high rate of secondary school dropout and youth unemployment. The program I study was implemented by BRAC, the largest NGO in the world. On-the-job training is provided through apprenticeship under a local Master Crafts Person (MCP). Classroom training curriculum includes theoretical training on specific trades, and soft-skills training such as financial literacy, market assessment, and basic communicative English. The program also provides post-training support. Once training is completed, program officers link participants with potential employers for wage employment. And for those who are keen on self-employment, they offer information, guidance, and technical assistance. Evaluating this program, I investigate the following research questions: (1) what is the effect of on-the-job training on labor market outcomes (employment and earnings)? and (2) how does this effect vary if classroom training is compounded with on-the-job training (i.e., what is the additional effect of classroom training)?

The data used in this study were generated by BRAC's Research and Evaluation Division (henceforth, BRAC-RED). For evaluating the program, BRAC-RED implemented a randomized controlled trial with the cohort of 2016. At the first stage, it purposefully selected sixty branch offices from the list of offices where the 2016 cohort was targeted.[8] In randomly selected half of

[7] To the best of my knowledge, one study (Acevedo *et al.*, 2017) estimates the additional effect of classroom vocational training.

[8] The training program is implemented through local BRAC office, known as branch office. A branch office typically covers a geographical area of 5-7 km radius.

these branch offices only on-the-job training was implemented while in the other half, both on-the-job and classroom training were implemented. From each of the 60 sample branch offices, about 56 eligible youths were randomly chosen, and randomly selected half of them were assigned to the treatment group and the rest to control (i.e., no training). BRAC-RED conducted a baseline survey in June, 2016 covering 3,186 youths and their families. After the baseline survey, BRAC started training in July, 2016 which ended in December, 2016. Sixty one percent of the youth assigned to the treatment group participated in the program. Six months after completion of training phase, BRAC-RED conducted a follow up survey, successfully reaching 2,946 youths.

I estimate short-run impacts of the intervention using data on 2,946 youths. Results show that on-the-job training (i.e., apprenticeship training) has positive effects on employment and earnings. The magnitudes of the effects are large. Specifically, it increases labor market participation of program participants by 22.6 percentage points (59%), hours of work by 59%, and earnings by 44% (TOT effects). Additional effect of classroom training is found to be modest. Specifically the effects are statistically insignificant for all outcomes except for hours of work in self-employment and earnings from this employment. Point estimates of the additional effects of the classroom training, however, indicate that if classroom training is added to on-the-job training the effects shift from self- to wage employment. By examining heterogeneity of the effects of training with respect to gender, I find that the effect on employment is larger for females, and the difference is statistically significant. With regard to earnings, females also experience larger impacts but the difference is not statistically significant. Further results show that females experience positive impacts on both wage and self-employment while males only experience positive effects on wage employment. Results also show that the program is more effective for unmarried females compared to their married counterparts. It is found that employment in firms where the apprenticeship took place (i.e., in the MCPs' firms) is a key channel for wage employment effect.

Findings also show that the intervention increases welfare substantially. Treatment individuals are more likely to own personal cell phone, and have more dresses (shirts/pants) and shoes compared to control individuals. They are also likely to report higher level of psychological wellbeing. A cost-benefit analysis of on-the-job training component of the program shows that it is highly cost effective. Assuming that the life of benefit is 44 years, and that benefits do not change overtime, benefit-cost ratio for on-the-job training is estimated to be 6.34. Using variation across branches in the intensity of treatment, I show that, at the scale at which the program was implemented, employment effects for beneficiaries were not achieved through displacement of non-beneficiaries.

This paper makes a number of contributions to the literature on training programs. First, it is the first to estimate the additional impact of classroom training (theoretical plus soft skills training) over on-the-job training. To the best of my knowledge, Acevedo *et al.* (2017) is the only study that estimates the additional effect of classroom training but they focus on the vocational (i.e., theoretical/hard skills) component of classroom training. Since classroom training accounts for a significant portion of resources under training programs, the findings of this study regarding this have important implications for policy.

Second, this study contributes to the literature on gender differences in the effects of training programs (Cho *et al.*, 2016; Attanasio *et al.*, 2011). As already mentioned, existing evidence on difference in training impacts by gender is mixed. Hence, this study advances our knowledge by expanding the existing set of results on training and gender.

As already mentioned, the training program I study in this paper using an experimental design is implemented by BRAC in Bangladesh. Earlier, Bhattacherjee and Kamruzzaman (2016) and Rahman *et al.* (2017) evaluated the pilot of this program.[9] These studies, however, suffer from methodological shortcomings as they use a non-experimental design where near eligible individuals were compared with program participants. Further, they do not separate out the effects of different training components of the program.

The remainder of the paper is organized as follows. After the introduction, section 1.2 describes the context and the training program while section 1.3 formulates testable hypotheses. In section 1.4, I discuss the evaluation design and the data used in this study while section 1.5 provides descriptive statistics including balancing test of randomization. Section 1.6 presents the results. Finally, section 1.7 concludes the paper.

1.2 The Context and BRAC's Training Program

1.2.1 The Context

Bangladesh is a lower middle-income country with a population of 160 million.[10] As of 2010, 31.5% of its population live in poverty (BBS, 2012). The Constitution of Bangladesh has guaranteed the rights to primary education for all, and the amount of public investment in education is substantial. About 12.27% of total public expenditures (i.e., 2.5% of GDP) is allocated for the education sector, of which almost half is for primary and mass education.[11] Further, several NGOs invest substantial amount of resources in education for children from poor families.[12] Despite all these initiatives, school dropout rate remains high. According to BANBEIS (2015) school dropout rates at the primary and secondary level are 21% and 43.18%, respectively.

Many school dropout youth or adolescents in Bangladesh end up unemployed. A recent study using data from 35 slums from urban areas shows that 32% of youth aged 15-19 years are neither enrolled in school nor in the job market (Chowdhury *et al.*, 2017).[13] National level data, on the other hand, show that 9.9% of youth aged 15-19 years were unemployed in 2015-16 (BBS, 2017). The rate is higher among females (11.5%) compared to males (9.3%). Poverty is reported to be a proximate cause of school dropout in Bangladesh (Sabates *et al.,* 2010). Hence, programs that address youth unemployment are likely to contribute to poverty reduction.

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[9] Rahman *et al.* (2017) show that the program increases employment by about 46 percentage points and income by BDT 1028.

[10] https://datahelpdesk.worldbank.org/knowledgebase/articles/906519-world-bank-country-and-lending-groups

[11] http://www.mof.gov.bd/en/budget/16_17/speech/BS_Bangla_Final_1.6.16.pdf

[12] For example, BRAC provided education to poor children through 20,776 primary schools in 2005 (Nath, 2006).

[13] School enrolment rate among youth aged 15-19 years is found to be 24%.

Informal sectors constitute majority of the job opportunities (around 80%) in Bangladesh (ADB and BBS, 2012). Low productivity and abundance of unskilled labor are some of the general characteristics of the informal sector in Bangladesh. Titumir and Hossain (2003) show that the low level of skills is likely to be responsible for low productivity in most sectors of Bangladesh including informal ones. In the absence of proper training facilities majority of the informal sector workers learn specific trades through apprenticeship. However, most apprenticeship arrangements in informal markets are unpaid where the apprentice needs to work fulltime with little or no employment benefit (Maligalig *et al.,* 2009).

In Bangladesh, the legal age of marriage is 18 and 21 years for females and males, respectively. But statistics show that more than half of girls are married off before reaching 18 years (UNICEF, 2016). School dropout and unemployment are reported to be the proximate causes of child marriage (Kamal *et al.*, 2015). It is thus possible that development programs that increase the employment of female youth are likely to also reduce child marriage.

1.2.2 BRAC's Training Program

BRAC started a training program titled 'Skill Training for Advancing Resources (henceforth, STAR)' in 2012. The objective of the program is to produce a well-trained and empowered manpower among youth and thus enhance employment. The program targets individuals aged 14-18 years from poor households, who are out of school for at least a year.[14] For disable participants, however, age range is from 15-21 years.[15] Maximum grade passed by the target group is 8. In addition, per capita monthly income of their families must be less than BDT 3,000 (i.e., $1.12 per person per day at nominal exchange rate or $3.42 per person per day at 2015 PPP exchange rate).[16] The program is operated through BRAC field office, known as branch office. BRAC staffs make door-to-door visits in communities/villages surrounding the branch office to identify eligible participants. They use a small questionnaire, containing questions on the eligibility criteria. Initially, the program was developed by BRAC together with ILO and UNICEF in order to support the Bureau of Non-Formal Education (BNFE)'s 2nd phase of the Basic Education for Hard to Reach Urban Working Children (BEHTRUWC) project. The model later continued serving the poor and disadvantaged school dropouts from both rural and urban areas.

Program participants are provided with both classroom and on-the job training. The duration of on-the-job training is six months with five days a week. The classroom training, by contrast, is provided for a period of six months with three and a half hours a week. The program cycle is completed in six months. Classroom training curriculum includes theoretical training on specific

[14] The pilot cohort targeted individuals aged 13-17 years but since 2014 the program has been targeting individuals aged 14-18 years. It needs to be mentioned here that as per child labor law in Bangladesh individuals aged 14 years are eligible to work (https://childlabourlawbd.blogspot.com/2017/).
[15] Typically, the program targets 10% youth with disabilities.
[16] The Bangladesh Bureau of Statistics (BBS) publishes the official poverty estimates for Bangladesh using the Cost of Basic Needs (CBN) methodology. Those that earn BDT 2,587 or less per capita per month (calculated based on information from BBS, 2012) are considered as poor, indicating that BRAC targets youth from poor households.

trades, and soft-skills training (financial literacy, market assessment and basic communicative English). The theoretical training is provided by trainers from local government and non-government training institutions. A trainee is provided training on a single trade. Typically, the program provides training on tailoring, fridge/AC repairing, embroidering, electronic device repairing, wooden furniture making, beauty parlour training, and graphics designing. Selection of trade for a participant depends on his/her interest. Once training is completed, BRAC links the participants with potential employers for wage employments. And for those keen on self-employment, BRAC offers information, guidance and technical assistance.

On-the-job training is provided through apprenticeship under a local Master Crafts Person (MCP). Main characteristics of the MCPs selected by BRAC are as follows: (i) the owner of small firms in the local markets; (ii) experienced as a skilled crafts person in the particular trade at least for 5 years; (iii) availability of sufficient space in his/her workplace to accommodate apprentice; (iv) previous successful experience in managing apprentices; (v) education level at least grade five; (vi) workplaces is located within the eight km radius of BRAC field office; and (vii) availability of toilet facilities at the work place.[17] The MCPs are provided orientation on: (i) objectives of the STAR program; (ii) their responsibility to apprentice; and (iii) decent working environment.

As already mentioned, on-the-job training is provided through placing trainees with local MCPs. Hence, the total number of participants targeted from each branch office is contingent upon the number of MCPs available. It also depends on administrative issues like staffing as each field staff has to manage about 50-70 participants. The program is subsidized. As travel allowance, each trainee is provided with BDT 1,200 ($15) per month. On the other hand, the MCPs are provided with an allowance of BDT 2,000 ($25) per month for each apprentice placement. For 2016 cohort, total costs per participant were BDT 2,900. I discuss the costs of the program in detail in section 1.6.5.

The pilot phase of the program was implemented in 2012-2013, covering 1,000 youths from the divisional cities.[18] The program targeted an equal number of male and female participants during the pilot. In 2016, it targeted 7,500 youths (60% females) from both urban (including both district and divisional cities) and rural areas. Around 10% of program participants were youth with disabilities (mild to moderate disability).

[17] The MCPs can be workers in small firms in the local markets. But priority is given to those that are owners.
[18] There are seven administrative divisions in Bangladesh.

1.3 Testable Hypotheses

There are several channels through which training may affect employability and earnings. Investment in human capital can be the key to macroeconomic growth (Krueger and Lindahl, 2001). Human capital, in the form of observable skills associated with investments in education and training, can raise productivity, wage and employability (Card, 1999). Since apprenticeship training imparts practical skills, it may increase trainees' human capital and productivity. Similarly, classroom training that provides theoretical training on specific trade may impart technical skills, thereby increasing earnings and employment.

Training under MCPs' mentorship allows the participants to reveal their "type" (effort, skills and talents) to potential employers, thereby increasing employment. MCPs may have vacant positions because study shows that there are substantial labor market frictions in developing countries (Hardy and McCasland, 2015). Therefore, it is expected that apprenticeship training would increase employment through filling vacant positions (if any) in MCPs' firms.

Theoretically, workers that are well connected are likely to fare better than those that are poorly connected (Montgomery, 1991). Working directly with MCPs, the participants are able to connect not only to one potential employer but also to the network of employers through recommendations (Owolabi and Pal, 2011). Hence, they are likely to have a strong network with employees and employers, which may eventually help them enter job markets. Network coupled with technical and practical skills may also increase employment through migration because literature shows that skilled workers are more likely to migrate (Chiquiar and Hanson, 2005).[19] Similarly, those with strong community networks are more likely to migrate (Munshi, 2003).

Training may also impart general skills on how to start and operate a business, which could spur entrepreneurship (Cho *et. al.*, 2016). Hence, training could also increase self-employment.

To summarize:

(1) Apprenticeship training may increase employment and earnings.
(2) Additional effect of classroom training may be positive
(3) Employment in MCPs' firms and migration can be possible channels for the effects.

1.4 Evaluation Design and Data

This study is based on the data generated by BRAC-RED, a multi-disciplinary independent research unit within the framework of BRAC. BRAC-RED plays an integral role in designing BRAC's development interventions, monitoring progress, documenting achievements, and undertaking impact assessment studies.[20] For the purpose of impact evaluation of the STAR program, it conducted a baseline survey in June, 2016. The sample individuals were followed up in June-July, 2017, generating panel data on 2,946 eligible youths.

[19] Chiquiar and Hanson (2005) show that among men, those with intermediate levels of skill migrate most, and among women, on the other hand, those with the highest levels of skill migrate most.
[20] http://research.brac.net/new/about/whoweare.

1.4.1 Evaluation Design

To evaluate the STAR program, BRAC-RED adopted a Randomized Controlled Trial (RCT) design for the cohort of 2016. For implementing the 2016 cohort of the program, BRAC selected 120 branch offices. For each branch office, a tentative target of beneficiaries was set based on MCPs availability in that particular area, and administrative issues, as discussed earlier. The target varied between 56-131 beneficiaries across the branch offices. In some of these branch offices, the program was also implemented before 2016. BRAC-RED restricted study sample to 60 branch offices where the program was not implemented before 2016 or if any, the intensity of coverage in earlier year(s) was relatively low. These 60 branch offices are located in 34 districts (there are 64 districts in Bangladesh). Randomization of the intervention then proceeded as follows (Figure 1.1 shows each steps of the randomization):

1. Randomly selected half of the 60 branch offices were assigned to on-the-job training only (treatment arm 1) and the other half to combined classroom and on-the-job training (treatment arm 2).

2. In randomly selected half of the 30 branch offices assigned to treatment arm 1, program's planned target was reduced by 10%. Same sampling strategy was followed for the branch offices assigned to treatment arm 2. The random variation (10%) in program target was made to examine the displacement effect of the intervention.[21]

3. For each of the 30 branch offices assigned to treatment arm 1, about 56 eligible youths were randomly chosen from the list of youth selected by the program, and randomly selected half of them were assigned to treatment (on-the-job training only) and the rest to control.[22] In other words, although 30 branch offices were assigned to treatment arm 1 (on-the-job training), within each of these branch offices, 50% of the sample eligible youth were assigned to this treatment and the rest to control group (i.e., no support). Similarly, for each of the 30 branch offices assigned to treatment arm 2, about 56 eligible youths were randomly chosen, and randomly selected half of them were assigned to treatment (combined on-the-job and classroom training) and the rest to control group.

The sample was not stratified by gender. But, as the program targeted 60% female participants, it was expected that close to half the sample youth would be males.

[21] This small variation, however, may not give enough statistical power to detect the displacement effect of the intervention, if any.

[22] Originally, BRAC-RED planned to select 56 youths per branch office (i.e., 3,360 youths from 60 branch offices). Some youth, however, declined to participate in the survey or were absent during household visit. It could not replace all the non-responses by other eligible youth from respective branch offices, because there were not additional selected youth in some branch offices. Hence, in some branches, based on the availability, more than 56 youths were surveyed, while in others 56 or fewer youths were surveyed. Appropriate sampling weight was assigned for data analysis. Neither the program staff (i.e., those who implemented the intervention at the field level) nor the selected youth were informed about who is in the treatment or control group until completion of the baseline survey.

1.4.2 Data

A baseline survey was conducted in June, 2016. The survey attempted to cover 3,360 youths, but it could successfully interview 3,186. The baseline survey collected information on the demographic characteristics of the sample eligible youth and their household members, including age, marital status and education. It also collected information on the employment status and earnings of the sample youth as well as other household members aged 6 years or above for the last one-year of survey. In addition, it collected information on savings, credit, drug abuse, reverse digit test score, personality test, empowerment and confidence level (household decision making) of the youth. At the household level, information on asset holdings (productive and durable assets), and food and non-food expenditures was collected.

BRAC-RED conducted a follow up survey during June-July, 2017, about six months after completion of the training. The follow up survey attempted to visit all the youth covered by the baseline survey.

The follow up survey collected all the information contained in the baseline survey. In addition, it collected information on the migration history of the sample eligible youth and other adult members of their households. Employment module for the eligible youth collected detailed information on employment and earnings. It collected detailed information on employment, time devoted to each activity, and earnings for the last one month of the survey. The survey also collected information on employment status in each month from January-June, 2017. These data allow me to analyze the effect of the intervention on employment dynamics.

If any sample youth was not available at home during household visit for the follow up survey, survey enumerators were instructed to interview them through phone calls. Among the 3,186 eligible youths covered by the baseline survey, 2,946 were successfully interviewed by the follow up survey. Of these 2,946 youths, 2100 were interviewed in person and the rest (846 youths) through phone calls. Those that were interviewed through phone calls were asked limited set of questions (labor market participation, earnings, migration, savings and credit) to shorten the duration of phone call.

Information reported in appendix Table A1.1 shows that, of the 2,946 eligible youth covered by the follow up survey, 1,745 (59%) are females, which is consistent with the fact that in 2016 the program targeted 60% female participants. Overall attrition rate in the follow up survey was 7.65% (7.5% for treatment and 7.8% for control group). Attrition rate in my sample seems to be lower compared to most of the existing studies on training programs.[23] In Table A1.2, I test whether attrition rates are different between the treatment and control groups. I also test whether baseline characteristics are correlated with attrition. Results show that attrition rates do not differ between treatment and control groups (column 1). Similarly, none of the baseline characteristics reported in Table A1.2 is correlated with attrition (column 2). Results also show that there is no

[23] McKenzie (2017) reviewing 12 evaluations on training programs in developing countries shows that attrition rate is 18% or higher for all except one study.

differential attrition by these baseline characteristics between treatment and control individuals (column 3).

Table A1.3 reports program participation rate of the treatment group. Overall, 61% of those assigned to the treatment participated in the program. Participation rate in on-the-job training was higher than that of combined classroom and on-the-job training. Gender disaggregated analysis shows that males were less likely to participate in the training, particularly in the combined classroom and on-the-job training. These statistics are likely to indicate that males are less interested in classroom training. Further analysis (not shown in Table) shows that among those that participated in the program, 87% completed the training course (85% for males and 89% for females). These findings contrast with findings by Cho *et al.* (2016) who show that females are more irregular in attending training program. Among those that did not complete the training, 65% attended for a least one month. For estimating the effect of the intervention, I consider these individuals as participants as they might still be affected by the intervention.

1.4.3 MCP survey

In addition to the surveys on youth and their families, BRAC-RED conducted a baseline survey on MCPs. The purpose of this survey was to assess whether placement of trainees with MCPs affects the firm level outcomes: employments, profits, revenue, sales and business size. In other words, the purpose of the MCP survey was to investigate whether the firms to which apprentices were placed are labor constrained. The survey covered two groups of MCPs: MCPs to whom apprentices were placed by BRAC (henceforth, participant MCPs), and MCPs to whom no apprentice was placed by BRAC (henceforth, non-participant MCPs). The non-participant MCPs are those that were operating similar enterprises in the same market as the participant MCPs but their firms were not placed any trainee by BRAC either because they were not interested in taking trainees or they did not have spaces to accommodate trainees or BRAC did not approach to them. The MCP baseline survey was conducted in the same branch offices where the youth baseline survey took place. From each of the 60 sample branch offices, five participant MCPs and another five non-participant MCPs were randomly selected for the baseline survey. The survey, however, successfully visited 586 MCPs of whom 295 were participant MCPs and the rest 291 were non-participants. The survey collected detailed information on sales, profits, capital stock, and investments. It also collected information on the number of employees, working environment and safety measures of the firms. In addition, it collected information on the demographic characteristics and work experience of the MCPs. I use these data to characterize the MCPs.

1.5 Descriptive Statistics and Balancing Test

This study is based on data on 2,946 youths. Table 1.1 presents the gender disaggregated baseline characteristics of the youth and their households. At baseline, the average age of male youth was 16.2 years against 16.9 for females. Information also shows that 29% and 19% of male and female youth, respectively were employed at baseline. Further analysis (not shown in

Table), however, shows that 89% of the employed males devoted less than an hour per day to earning activities. For females, the proportion is 99%. At the national level, unemployment rate among comparable age group (15-19 years) is 9.9% (BBS, 2017). These statistics indicate that the program was successful in targeting the unemployed or under-employed youth. Twenty one percent of the sample female youth were married at baseline. For males, by contrast, the rate is only 2%. Further analysis (not shown in Table) shows that about 84% of the married females were married off before reaching 18 years, indicating considerable child marriage among the sample females.[24] The households of the sample male youth on average owned 18 decimals of land, 0.51 cows, and 0.42 goats; for females, corresponding figures are 15, 0.40, and 0.40, respectively.[25] Land holding is a strong correlate of poverty in Bangladesh, particularly for rural households (World Bank, 2013). These statistics thus indicate that the targeted youth were from asset poor households. The heads of the sample youth' households have little education (on average 2.11 and 2.25 years for males and females, respectively). Statistics also show that 90% of the sample youth were out of school at baseline. By and large, statistics reported in Table 1.1 indicate that the program successfully targeted the disadvantaged youth population.

Table 1.2 reports results from balancing test of the randomization. The analysis does not distinguish between males and females because the randomization was not stratified by gender. The table presents the results of a regression of the dependent variable listed in the first column on an indicator variable for on-the-job training (1 if assigned to on-the-job training and zero if otherwise), an indicator variable for both types of training (1 if assigned to combined on-the-job and classroom training and zero if otherwise) and branch fixed effects. Results reported in column 2 of Table 1.2 show that, among the 12 variables reported, two (education and gender) show statistically significant differences between the treatment and control samples for areas where on-the-job training was offered. Similarly, for branch offices where combined on-the-job and classroom training was offered, two variables (education and age) show statistically significant differences between the treatment and control groups (column 4). Overall, results in Table 1.2 are likely to indicate some minor imbalances of the treatment and control samples, but it could be that these differences are due to chance. Nonetheless, I control for these baseline imbalances for estimating the effect of the intervention.

Table 1.3 reports statistics on some socio-economic characteristics of the surveyed MCPs. As can be seen, the MCPs operated small firms, with average firm employing about 2.1 workers. Mean amount of capital of the firms is BDT 243,032 ($3,037). The level of education of the MCPs is 8 years on average.

[24] Among the sample females aged 18 years or above, child marriage rate is 37%.
[25] In Bangladesh, households owning less than 50 decimals of lands (0.20 hectares) are considered to be functionally landless (Scott and Islam, 2008).

1.6 Results and Discussion

1.6.1 Main Results

In this section, I estimate the effects of the intervention on labor market outcomes. The key outcome variables of interest for this study are employment, hours of work and earnings. The analysis distinguishes between self- and wage employment. First, I estimate intention-to-treat (ITT) effects by comparing the outcomes for individuals randomly assigned to the treatment and control groups disregarding their compliance with the treatment status. I use the sample of individuals who responded to both baseline and follow up surveys. Using follow up survey data, I simply run OLS regression of outcome on treatment indicators. Branch fixed effects are included as the randomization was stratified at the branch level. I also include baseline characteristics to control for minor imbalances in the randomization. Inclusion of these controls is also likely to gain precision in the estimates (Duflo *et al.*, 2008). Specifically, I estimate the following equation:

$$y_{ib} = \alpha_1 + \alpha_2 JTraining_{ib} + \alpha_3 BTraining_{ib} + X_{ib}\omega + \eta_b + \vartheta_{ib} \qquad (1.1)$$

Where y_{ib} is the outcome variable of interest for individual *i* from branch office b; $JTraining_{ib}$ is an indicator variable taking the value of 1 if individual *i* is assigned to on-the-job training and 0 if otherwise; $BTraining_{ib}$ is an indicator variable taking the value of 1 if individual *i* is assigned to both types of training (on-the-job plus classroom training) and 0 if otherwise; X_{ib} is a set of youth's baseline characteristics (marital status, age, gender, education, employment status, time devoted to earning activity and earnings); η_b are branch office fixed effects; and ϑ_{ib} is an error term. α_2 and α_3 are the ITT effects of on-the-job, and combined classroom and on-the-job training, respectively. $(\alpha_3 - \alpha_2)$ is the additional effect of classroom training.

I also estimate TOT effects using an instrumental variable approach. The second stage equation for IV is as follows:

$$y_{ib} = \beta_1 + \beta_2 PJTraining_{ib} + \beta_3 PBTraining_{ib} + X_{ib}\pi + \tau_b + \nu_{ib} \qquad (1.2)$$

Where $PJTraining_{ib}$ takes the value 1 if individual *i* has participated in on-the-job training and 0 if otherwise; $PBTraining_{ib}$ takes the value of 1 if individual *i* has participated in both classroom and on-the-job training and 0 if otherwise; τ_b are branch office fixed effects and ν_{ib} is an error term. Other variables are as defined earlier.

$PJTraining_{ib}$ and $PBTraining_{ib}$ in equation (1.2) are endogenous because not all those assigned to treatment participated. Hence, they are instrumented on $JTraining_{ib}$ and $BTraining_{ib}$. The first stage equations are as follows:

$$PJTraining_{ib} = \theta_1 + \theta_2 JTraining_{ib} + \theta_3 BTraining_{ib} + X_{ib}\rho + \varsigma_b + \varpi_{ib} \qquad (1.3)$$

$$PBTraining_{ib} = \gamma_1 + \gamma_2 JTraining_{ib} + \gamma_3 BTraining_{ib} + X_{ib}\sigma + \iota_b + \mu_{ib} \qquad (1.4)$$

where ϖ_{ib} and μ_{ib} are error terms, and ς_b and ι_b are branch office fixed effects.

Table 1.4 reports the regression results of estimating equations (1.1)-(1.4). Panel A presents ITT effects while panel B reports TOT effects. The first stage results from IV are presented in appendix Table A1.4. Columns 1-6 of Table 1.4 report the effects on employability, columns 7-12 on hours of work (per day) and columns 13-18 on earnings. Earnings information was collected for the last month of survey. I report monthly earnings in Bangladeshi currency (BDT).[26] The odd-numbered columns of Table 1.4 report regression results without controlling for baseline characteristics while the even-numbered columns report those with controlling for baseline characteristics. The first and second rows of panel A report the estimates of α_2 and α_3, respectively while the third row presents the estimate of $(\alpha_3 - \alpha_2)$. The first and second rows of panel B, on the other hand, report the estimates of β_2 and β_3, respectively while the third row presents the estimate of $(\beta_3 - \beta_2)$.

Results show that treatment effects are generally robust after controlling for baseline characteristics except for some minor changes in the point estimates of the effects. For instance, it is seen that with baseline controls the additional effect (ITT effect) of classroom training on time devoted to self-employment is 0.22 hours decrease (not significant) while without controls the corresponding effect is 0.26 hours decrease (significant at the 10% level) (columns 7 and 8 of Table 1.4). Nevertheless, in what follows the discussion mainly focuses on the results without baseline controls.

ITT estimates show that on-the-job training increases employment by 14 percentage points (column 5 of Table 1.4). The effect of this training on wage employment (9.5 percentage points increase) is larger than that of self-employment (5.3 percentage points increase) (columns 1 and 3 of panel A). Results reported in columns 7 and 9 show that on-the-job training also increases hours of work in both self- and wage employment. But the magnitude of the effect on hours of work in wage employment is lager than that of self-employment. As can be seen from the results reported in column 17, ITT effect of on-the-job training on total earnings (BDT/month) is positive and statistically significant. Similarly, the effect on earnings from wage employment is positive (statistically significant at the 5% level) (column 15). But the effect on earnings from self-employment is not statistically significant though point estimate is positive.

Results reported in the second row of panel A (Table 1.4) show that the ITT effect of combined classroom and on-the-job training is positive for both wage and self-employment but the effect for the latter outcome is not statistically significant (columns 1 and 3). Similarly, the result on the hours of work in wage employment is positive and statistically significant (column 9 of panel A). The impact of the combined classroom and on-the-job training on total earnings is also positive but it is statistically insignificant (column 17). In the 3rd row of panel A, I show the additional effect of classroom training. As can be seen, the additional effect of classroom training is statistically significant (at the 10% level) only for time devoted to self-employment and earnings from this employment (columns 7 and 13 of panel A). These effects are negative. Additional effects of classroom training on wage employment and time devoted to this

[26] Exchange of rate of USD in terms of BDT is roughly 80.

employment are positive but they are statistically insignificant. Taken together, results are likely to indicate that the additional effect of classroom training on employment is little but there is some weak evidence that when classroom training is compounded with on-the-job training, the effects shift from self- to wage employment. This may be due to the fact that training that combines theoretical learning and apprenticeship is more likely to improve participants' technical skills compared to training provided through apprenticeship only. The former thus increases the chance of wage employment. Therefore, among individuals who have the option to choose wage employment, some may engage in wage work leaving self-employment because of the risks associated with self-employment.[27]

Looking at the results presented in panel B of Table 1.4, it can be seen that the TOT effects are larger than the ITT effects for all the outcomes reported. This is expected because not all those assigned to the treatment participated. Comparing the TOT effects of on-the-job and both types of training, it is found that the difference in impacts between the two types of treatments is statistically insignificant for all the outcomes expect for time devoted to self-employment and earnings from this employment. Nonetheless, the TOT estimates for on-the-job training show that it increases employability by 22.6 percentage points (59% increase relative to control group mean), hours of work per day by 1.41 (59% increase relative to control mean) and total earnings by BDT 784 (44% increase relative to control group mean), indicating large positive effects of on-the-job training (columns 5, 11 an 17 of Table 1.4).

Table 1.4 presents results suggesting large effects of the training program on employment and earnings. A natural question is whether the program increases employment and earnings of those that were employed at baseline or of those that were unemployed or of both groups. To examine this, in Table 1.5, I report the impacts (ITT estimates) of the program on employability, hours of work and earnings at the intensive and extensive margins. The intensive margin is characterized by individuals that were employed (or under-employed) at baseline. The extensive margin, by contrast, is characterized by individuals that were unemployed at baseline. I estimate a slightly different version of equation (1.1); instead of two treatment indicator variables, I use one treatment indicator that takes the value of 1 if individual *i* is assigned to treatment (any type of treatments) and 0 if otherwise. So, the coefficient on this treatment indicator measures the average effect of the two types of treatments (on-the-job, and combined on-the-job and classroom training). I estimate the regression separately for the two groups of individuals. Findings show that the program increases employment and hours of work mainly for individuals that were unemployed at baseline (i.e., at the extensive margin). Moreover, it significantly increases earnings at the extensive margin. At the intensive margin, the effect on employment is positive and statistically significant at the 10% level if baseline characteristics are not controlled. The effects on hours of work and earnings at the intensive margins, by contrast, are all statistically insignificant. These results indicate that the training program is likely to be more effective for unemployed individuals compared to their employed counterparts.

Impact estimates reported in Tables 1.4 and 1.5 are for the last month of the follow-up survey. Employment information is also available for each of the last six months (January-June,

[27] Studies show that self-employment is risker than wage employment (Knight, 1921 as cited in Parker, 1997), and that the least risk averse chooses self-employment over paid employment (Kihlstrom and Laffont, 1979).

2017) of the follow up survey.[28] I analyze these data to examine whether the effect of the program on employment persists overtime. Using full sample data I estimate treatment effect for each of these months by regressing outcome (indicator variable for employability) on treatment indicator (it takes the value of 1 if individual *i* is assigned to treatment (any type of treatments)) and branch fixed effects. Figure 1.2 graphs the point estimates (ITT estimates). As can be seen, the point estimates hover around 0.14, showing that the effect of the program on employment does not decline overtime. These results also indicate that the program generates impacts immediately after completion of the training (note that the training was completed in December, 2016).

Several papers on apprenticeship training document positive effects on employment and earnings (e.g., Honorati, 2015; Alfonsi *et al.*, 2017).[29] Therefore, the findings of my study echo the positive results from these studies. However, the magnitude of the effect on employment I document in this study seems to be larger than those documented by Honorati (2015) and Alfonsi *et al.* (2017). The training program studied by Honorati (2015) increases employment by about 5.5 percentage points (ITT effect, simple un-weighted average across genders), and that of Alfonsi *et al.* (2017) by 7 percentage points (ITT effect, averaged over three year period but short-run effect is similar). By contrast, I find that apprenticeship training increases employment by 14 percentage points (ITT effect).

1.6.2 Heterogeneity of Effects with Respect to Gender

As mentioned earlier, existing evidence regarding gender differences in training effects is mixed. I thus examine gender differences in the effects of the STAR program. I do not, however, separate out the effects of on-the-job, and combined on-the-job and classroom training because disaggregated analysis by gender as well as treatment types might not have enough power to detect statistically significant effects. I estimate ITT effects using the following equation:

$$y_{ib} = \delta_1 + \delta_2 Training_{ib} + \delta_3 Training_{ib} * Gender_{ib} + X_{ib}\phi + \rho_b + \varepsilon_{ib} \quad \text{.....(1.5)}$$

where $Training_{ib}$ takes the value of 1 if individual i is assigned to treatment (any type of treatments); $Gender_{ib}$ is an indicator variable taking the value of 1 if individual i is male and 0 if female; ρ_b are branch fixed effects; X_{ib} is a set of youth's baseline characteristics including, among others, $Gender_{ib}$; and ε_{ib} is an error term. δ_2 identifies the effect of the training for females, $\delta_2 + \delta_3$ the effect for males, and δ_3 the additional effect for males.

Table 1.6 reports the regression results of equation (1.5). First row presents the estimated effects for females and second row for males. Third row, by contrast, reports the additional effects for males. The odd-numbered columns present results without baseline controls while the

[28] Since the survey was started in June, 2017, data for this month are not available for some individuals. Number of observations for this month is 1800.

[29] Similarly, Hardy and McCasland (2015) document large effect of apprenticeship training but they examine the effects at the firm level. Twenty one percent of the participants in the program studied by Honorati (2015) received apprenticeship training under master crafts persons along with technical training. They rest were provided technical training and internship.

even-numbered columns show those with baseline controls. As can be seen, results are generally robust after controlling for baseline characteristics. It is found that the impact of the intervention on overall employment, hours of work and earnings are larger for females compared to their male counterparts although the differences between the two groups of individuals for the latter two outcomes are not statistically significant (columns 5, 6, 11, 12, 17 and 18). Specifically, as per the results without baseline controls, the intervention increases females' employability by 17.7 percentage points (ITT effects) compared to 5.8 percentage points for males and the difference is statistically significant at the 1% level (column 5). Results also show that the intervention increases both wage and self-employment for females. For males, by contrast, it has statistically significant effect on wage employment only. Further results from impact analysis for specific occupations under self-employment show that the intervention increases females' time for tailoring and small businesses (columns 13-16 of panel B of Table A1.5 in appendix). Results for specific occupations under wage employment, by contrast, show that the intervention increases females' time for working as beautician and tailor (columns 3-6 of panel A of Table A1.5). Estimates reported in panel A also show that treatment males devoted more time to mobile phone servicing and wooden furniture making related works compared to their control counterparts. Importantly, it is found that females decrease time devoted to work for readymade garment (RMG), a major source of employment for female workers in Bangladesh (panel A). This sector employs about 4 million people, 80 percent of whom are females (BSR, 2014). My findings thus indicate that skilled workers are less likely to work for RMG sector.

Why is the program less effective for males? Evidence shows that this may be because males learn little from training (Acevedo *et al.*, 2017). Also, it may be because baseline unemployment rate among males (29%) was lager compared to females (19%). As shown earlier, the program generates lager impacts for the unemployed youth compared to the employed. Analyzing the data on control group's employment status at the baseline and follow-up, I find that employment rate among males increased from 29% to 65%. For females, by contrast, the proportion remained almost the same (21.6% at baseline and 21.4% at follow up). These statistics indicate that females in Bangladesh are likely to be constrained to enter labor market. Evidence shows that Bangladeshi women's participation in market employment is constrained due to their traditional gender roles that require them to bear the main responsibility of household work on a daily and generational basis (Kabeer, 2003). My findings thus indicate that training perhaps helps women overcome this constraint, thereby generating large effects for them.

Since the program significantly increases employment and earnings among female youth, most of whom were unemployed and unmarried at baseline, it has important implications on their economic and social empowerment. It has also implication on child marriage because empirical evidence shows that child marriage among females in Bangladesh is higher for unemployed or unskilled workers (Kamal *et al*., 2015). However, results presented so far in this paper do not provide evidence on whether the intervention increases employment among unmarried females in the sample. To examine this, I estimate the heterogeneity of females' employment effects with respect to marital status. Training might have larger effects on employment for unmarried females, particularly on wage employment, compared to their married counterparts, because evidence shows that married females have to spend more time on housework (World Bank, 2011). This is also evident from employment data on control females

in my study sample. The data show that, at follow-up, married females in the control sample devoted 1.6 hours per day to household chores compared to 0.79 hours among unmarried females.

Appendix Table A1.6 reports the results of impact heterogeneity with respect to marital status. They are estimated using female sample only. I regress each outcome variable on treatment indicator, marital status (indicator variable taking the value of 1 if married and zero if unmarried) and interaction of treatment indicator with marital status. Branch fixed effects are also controlled. Additional results controlling for baseline characteristics are reported for robustness check. Findings show that both married and unmarried females experience positive effects on self-employment and hours of work in this employment but the effects for married females are statistically insignificant. It is also found that the effect on wage employment is very small and statistically insignificant for married females but unmarried females experience positive and statistically significant effect on this employment. Similarly, the program increases the earnings of married females. Overall, results reported in Table A1.6 indicate that the effect of training program may be smaller for married females compared to their unmarried counterparts. Yet, training has important implication for married individuals because they see some positive effect (though statistically insignificant, may be because of low statistical power) on self-employment despite the fact that they have to devote substantial amount of time to household chores.

1.6.3 Channels for Employment Effects

What are the mechanisms leading to the results? As mentioned earlier, employment in the MCPs' firms (i.e., the firms where the apprenticeships took place) can be a channel for the effects. Data show (not reported in table) that among those assigned to treatment (either on the job training or both types of training), 11.5% were employed in the MCPs' firms. For the control group, the corresponding proportion is 2.8%. This suggests that employment via MCPs is a channel for the effect on wage employment. In Table 1.7, I test this proposition using a regression framework. I estimate the effect of the intervention on the following three outcomes: wage employment anywhere, wage employment in MCPs' firms, and wage employment anywhere except in MCPs' firms. If employment in MCPs' firms is a mechanism for wage employment effects, then the effect on wage employment anywhere would be larger than the effect on wage employment anywhere except in MCPs' firms. Results show that the intervention increases wage employment anywhere by about 10.4 percentage points (ITT effects) (column 1 of Table 1.7). By contrast, it increases employment in MCPs' firms by 8.4 percentage points (column 3), suggesting that the effect on overall wage employment (i.e., wage employment anywhere) is predominantly derived by employment in MCPs' firms. As a result, the effect on wage employment anywhere except in MCPs' firms is very small and statistically insignificant (column 5 of Table 1.7). These results are likely to suggest that a reason why the BRAC program has large effects on wage employment is due to the choice of enterprises with MCPs and their interest in using apprentices as a channel to hiring.

As mentioned earlier, another channel for the effects on employment can be migration. In Table 1.8, I examine whether the program affects migration (internal or international

migration).[30] Migration is typically practiced by males in Bangladesh (Bryan *et al.,* 2014); hence, I estimate gender-disaggregated effects. Columns 1 and 2 of Table 1.8 report the estimated effects on migration using specification (1.5). Results show that point estimates are positive for males and negative for females. But they are not statistically significant effect. The reason for the little effect on migration may be due to the fact that most of the sample individuals were less than 18 years old at baseline (i.e., less than 19 years at follow up). It is perhaps less likely that these individuals would be away from their families. To test this, in columns 3 and 4 of Table 1.8, I show the impact on migration for relatively older individuals in the sample (i.e., those that were 18 years or above at baseline). Results confirm that the program has positive and statistically significant effect on migration for males from this age group. But there is no statistically significant effect for females. Data show (not reported in Table) that 87% of migrants in the sample were employed (mostly wage employed) at follow up compared to 45% among non-migrants, indicating that migration is likely to be a channel for employment effect for male individuals aged more than 17 years. But these individuals represent only 8.8% of the full sample. Taken together, results suggest that migration does not seem to be an important channel for the overall employment effect documented in this study.

1.6.4 Effects on Welfare and Asset Accumulation

Since the program significantly increases the employment and earnings of disadvantaged youth, it is likely that it would have positive effects on their welfare. In this section, I examine the effects of the intervention on welfare and asset accumulation. The follow-up survey asked several questions related to wellbeing. With regard to psychological wellbeing, it asked the sample youth six questions related to happiness, stress, anger and overall difficulties. The answers to these questions were recorded as yes or no. Using these indicators, I construct a psychological wellbeing index. First, I code a "yes" as 1 and a "no" as 0. Then each variable is standardized using control group's mean and standard deviation. Afterwards, I take an average of these standardized variables. The average is again standardized using control group's mean and standard deviation. The survey also collected information on the number of shoes and dresses, and cell phone ownership of the sample youth. I further analyze these outcomes as welfare indicators. The survey collected information on physical asset holding at the household level. Among physical assets, ownership of a sewing machine is particularly notable since the program increases self-employment in tailoring. So, the program may impact ownership of this asset. I thus analyze the effect of the intervention on sewing machines. Finally, I also analyze the effect on youth's savings.

Estimated effects on welfare and asset holdings are presented in Table 1.9. They are estimated by regressing each outcome on treatment indicator (it takes the value of 1 if individual *i* is assigned to any type of treatments), and branch office fixed effects. Additional results controlling for baseline characteristics are also reported to see robustness of the results. Results show that the intervention increases psychological wellbeing by 0.10 standard deviations and the effect is statistically significant at the 5% level (column 1 of Table 1.9). The program also increases the number of shirts/dresses and pairs of shoes owned. These effects are statistically

[30] Data show that 80% of migration in my sample was internal.

significant if baseline characteristics are controlled. Results also show that treatment individuals are 4.5 percentage points more likely to own personal cell phones compared to their control counterparts, and this effect is statistically significant at the10% level (column 7 of Table 1.9). Further, the intervention increases the ownership of sewing machines which are important for self-employment. The effect on savings is positive but not statistically significant.

1.6.5 Cost-benefit Analysis

In this section, I provide a cost-benefit analysis. As already discussed, the additional effects of the classroom training are small and statistically insignificant for most of the outcomes of interest for this study whereas the effects of on-the-job training are large in magnitude. These results are likely to indicate that on-the-job training can be scaled up in a cost-effective manner. Hence, I conduct a cost-benefit analysis for on-the-job training only. For benefit calculation, I follow Attanasio *et al.* (2011). They consider two cases: (i) gains are permanent but do not grow over time, and (ii) a 10 percent annual depreciation of gains. I also consider these two cases. Since the average age of the program participants is around 16, it is assumed that their working life is another 44 years. Results reported in Table 1.4 show that on-the-job training increases earnings of participants by BDT 784 per month (i.e., BDT 9,408 per year). Using this estimate, and assuming that gains are permanent but do not grow over time, total gain for 44 years with 5% discount rate is BDT 166,171. Since the duration of the training is six months, it is likely that participants did not earn income during these months. At baseline, the sample youth on average earned BDT 62.2 per month (i.e., BDT 373 for six months). This amount can be considered as the opportunity cost of attending the six-month training program. Therefore, the gain net of the opportunity costs of attending training is BDT 165,798. Average cost per participant of on-the-job training (2016 cohort) was BDT 26,116.[31] These costs include administrative costs, and allowances to trainees and MCPs. Note that the allowance provided to participants has not been considered as benefits because the amount is for travel purposes. Comparing the gains with the costs, the benefit-cost ratio is estimated to be 6.34, indicating substantial gains from on-the-job training relative to its costs.

Under a conservative scenario where benefits depreciate at a rate of 10% each year, total gain with 5% discount rate over a life cycle of 44 years is BDT 62,649. After deducting the opportunity cost of attending training, the figure stands at BDT 62,276. Benefit-cost ratio under this scenario is estimated to be 2.38. Taken together, results indicate substantial gains from apprenticeship training. These gains are, however, under-estimated if the MCPs are somehow positively affected by the intervention (e.g., if profits increase).

1.6.6 Robustness Check

As mentioned earlier, the random assignment to treatment or control group was done at the individual level. A natural question is thus whether the control individuals are affected by the

[31]For full package of training, on the other hand, costs per participant were BDT 29,000. For 2014 cohort, average costs per participant (full package of training) were BDT 33,000 against BDT 29,000 for 2016 cohort. Note that the program covered 1,000 and 7,500 youths in 2014 and 2016, respectively. These statistics indicate that marginal costs are likely to be lower than average costs.

intervention because training programs are likely to have displacement effects (i.e., treatment individuals take the job by displacing control individuals) (Crépon *et al.*, 2012; Johnson, 1979). The displaced individuals, if any, may become unemployed or accept lower wage jobs and their earnings may fall (Friedlander *et al*., 1997).[32] However, for poor country like Bangladesh, training programs may not have large displacement effects because evidence shows that there are substantial labor market frictions in developing countries (Hardy and McCasland, 2015).

In this section, I attempt to examine whether the control individuals in the sample are affected by the intervention. For this purpose, I exploit the variation in program coverage across the sample branch offices. As mentioned in section 1.2.2, BRAC set out a planned target for each branch office. The target varied significantly across branch offices (ranging from 50 to 120 participants). Furthermore, BRAC-RED reduced the planned target by 10% in a randomly selected half of the sample branch offices, creating further variation in the target across the sample branch offices. Figure 1.3 shows the distribution of BRAC's final target as a proportion of total eligible youth (unemployed youth aged 14-18 years) in the respective areas. The number of unemployed youth is calculated using information on the total population covered by each branch office and youth (14-18 years old) unemployment rates across administrative divisions.[33] BRAC has a total of 2000 branch offices across the country.[34] BRAC's development interventions cover a population of about 138 million in Bangladesh.[35] Hence, in the coverage area of a branch office, there are about 69,000 people, of whom around 6,555 (9.5%) are in the age group of 14-18 years.[36] Unemployment rate among youth aged 14-18 years varies from 7 to 11% across the administrative divisions (BBS, 2017). Using this information, I calculate the total number of eligible youth for each of the study branch offices.

Figure 1.3 shows that program coverage (i.e., final target) as a proportion of total eligible youth varies significantly across the study branch offices. On average the program covered 8.2% of total eligible youths from each branch office. Since the proportion is very small, it is perhaps less likely that the intervention would have a significant effect on control group's employment. However, I exploit this variation to formally examine if the intervention has any effect on employment for the control group. The variation in program coverage shown on Figure 1.3 may be endogenous to labor market outcomes. I investigate whether this variation is correlated with the baseline characteristics of the sample individuals. Table A1.7 (in appendix) reports the estimated coefficients of a regression of the variation in program coverage on youth's age, employment, earnings, education, gender, and marital status at baseline. The regression also includes district fixed effects and an indicator variable for urban area. Results indicate that some of the characteristics are correlated with the variation in program coverage. I control for these characteristics to examine the effect of variation in program coverage on control's employment. Specifically, I estimate the following equation using the full sample (i.e., 2,946 youths):

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[32] Several arguments, however, suggest that displacement if any may not seriously undermine training program effectiveness. Cohen (1969) and Johnson (1979) argue that, if training program participants are less likely to seek employment during training period than they otherwise would have been, then more jobs will be open to nonparticipants, at least temporarily.

[33] There are seven administrative divisions in Bangladesh, and the sample of this study covered all the divisions.

[34] http://www.brac.net/microfinance-programme/item/855-overview

[35] http://www.brac.net/sites/default/files/ataglance/at-a-glance-December-2014.pdf

[36] According to HIES 2010 (BBS, 2012), 9.5% of Bangladesh's population are in the age group of 15-19 years.

$$y_{ibld} = b_1 + b_2 Training_{ibld} + b_3 Training_{ibld} * Percentcov_{bld} + b_4 Percentcov_{bld} + b_4 Urban_{ld} + X_{ibld}\Phi + \zeta_d + e_{ibld} \quad (1.6)$$

where y_{ibld} is the outcome variable of interest (employment) for individual *i* from branch office b in location *l* (urban or rural) from district *d*; $Training_{ibld}$ is an indicator variable taking the value of 1 if individual *i* is assigned to treatment (any type of treatment), and 0 if control; $Percentcov_{bld}$ is the program's total coverage as percentage of the total number of eligible youth; $Urban_{ld}$ is an indicator variable taking the value of 1 if the location is urban and 0 if rural (program coverage is typically higher in the branch office located in urban areas; hence, dummy variable for urban is included); X_{ibld} is a set of youth's baseline characteristics; ζ_d are districts fixed effects; e_{ibld} is an error term. Standard errors are clustered at the branch office level. b_2+b_3 measures the effect of the intervention at a given value of $Percentcov_{bd}$. b_4 is the key parameter of interest, which measures the effect of the intervention on employment for control.

Table 1.10 reports the regression results of equation (1.6). Results reported in column 1 show that if $Percentcov_{bld}$ and $Training_{ibld} * Percentcov_{bld}$ are not controlled, the point estimate of the coefficient on $Training_{ibld}$ is 0.130 (significant at the 1% level). If $Percentcov_{bld}$ is controlled, the point estimate of the coefficient on $Training_{ibld}$ remains unchanged. On the other hand, the point estimate of the coefficient on $Percentcov_{bld}$ is small and statistically insignificant. Finally, if both $Percentcov_{bld}$ and $Training_{ibld} * Percentcov_{bld}$ are controlled, the point estimate of the coefficient on $Training_{ibld}$ declines to a large extent, mainly because the estimated coefficient on $Training_{ibld} * Percentcov_{bld}$ is positive (column 3). However, the estimate of b_4 (i.e., coefficient on $Percentcov_{bld}$), the key parameter of interest, is found to be statistically insignificant, suggesting that the program has perhaps no effect on the employment of control individuals. Point estimates reported in column 3, however, indicate that at the mean level of coverage by the program (i.e., 8.2%), the effect on control group's employment is a 3 percentage points decrease (8.2*0.0037=0.0303) while the ITT effect of the program on treatment individuals' employment is 12.2 percentage points increase (0.0749 + (0.0058*8.2)=0.122). Taken together, results indicate that the intervention has no significant effect on the control; if any, the magnitude of the effect is small relative to effect on the treatment group. Hence, at the scale at which the program was implemented, employment effects for beneficiaries were not achieved through displacement of non-beneficiaries.

1.7 Conclusion

Rising youth unemployment is a key concern for many poor countries. Skills training can be a potential solution to this problem but existing evidence on the effectiveness of training programs in developing countries is mixed. This paper extends the existing set of results by studying a training program in Bangladesh, a lower middle-income country with high rate of youth unemployment. The program provides on-the-job and classroom training to disadvantaged and unemployed/under-employed youth from both rural and urban areas. On-the-job training is provided through apprenticeship under a local Master Crafts Person (MCP). Classroom training curriculum includes theoretical training on specific trades as well as soft-skills training. The data used in this study were generated by BRAC's Research and Evaluation Division (BRAC-RED).

BRAC-RED conducted a randomized controlled trial across branches with treatments consisting of on-the-job training and a combined on-the-job training with classroom training in trades and soft skills. A baseline survey was conducted in June, 2016, and a follow up survey in June-July, 2017, about six months after completion of the training. Using these data, I investigate the following research questions: (1) what is the effect of on-the-job training on labor market outcomes (employment and earnings)? and (2) how does this effect vary if classroom training is compounded with on-the-job training (i.e., what is the additional effect of classroom training)? I also analyze effects on wage versus self-employment, migration decisions, wellbeing, and asset ownership. The particular role of employment in MCPs' firms is also studied. Heterogeneity of impacts is analyzed across genders.

I estimate the short-run impacts of the intervention. Results show that on-the-job training, which was provided through apprenticeship, has positive effects on employment and earnings. Specifically, it increases labor market participation of the program participants by 22.6 percentage points, total time devoted to earning activities by 59%, and earnings by 44%. Additional effects of the classroom training on overall employment and earnings are statistically insignificant. Further results, however, indicate that if classroom training is added to on-the-job training, the effects shift from self- to wage employment. By examining heterogeneity of the effects with respect to gender, I find that the effect on employment is larger for females. Furthermore, females experience positive impacts on both wage and self-employment while males only experience positive effects on wage employment. The program has thus important implications for the economic and social empowerment of the disadvantaged females. Expectedly, the effects on employment and earnings are larger effects for those that were unemployed at baseline. The intervention increases welfare substantially. Treatment individuals are more likely to own personal cell phones, and to have dresses (shirts/pants) and shoes compared to their control counterparts. They are also likely to report a higher level of psychological wellbeing. Benefit/cost ratio for on-the-job training is estimated at 6.34, suggesting that it can be scaled up with a cost-effective manner.

Using variation across branches in the intensity of treatment, I show that, at the scale at which the program was implemented, employment effects for beneficiaries were not achieved through displacement of non-beneficiaries. It is found that employment in firms where apprenticeship took place is the key channel for the effects on wage employment. I believe that a reason why the BRAC program has large effects on wage employment is due to the choice of enterprises with MCPs and their interest in using apprentices as a channel to hiring. This has important implications for the external validity of the results presented in this paper and comparison with results obtained by others.

The main limitation of this study is that it estimates the short-run impacts of the intervention. The question is thus whether these effects would dissipate in the long-run. Analyzing employment dynamics over a six-month period after the intervention, I find that the program generates impacts immediately after completion of the training phase, and that the effects do not decline within the six-month period. However, further study needs to be conducted to examine whether the impacts sustain the long-run. These results need to be assessed in relation to the effective way in which BRAC recruited MCPs.

1.8 Figures

Figure 1.1: Steps of randomization of BRAC Training Program

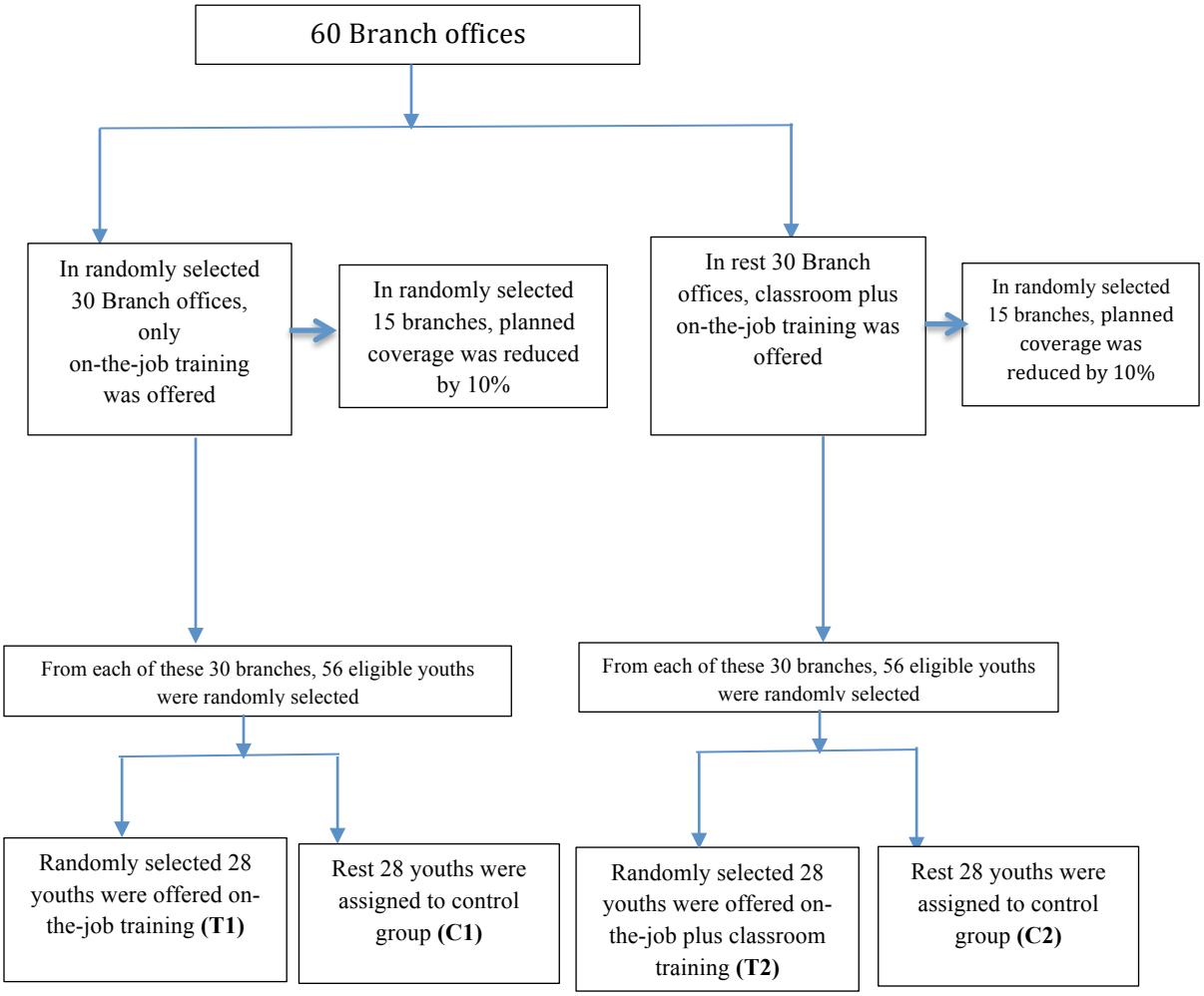
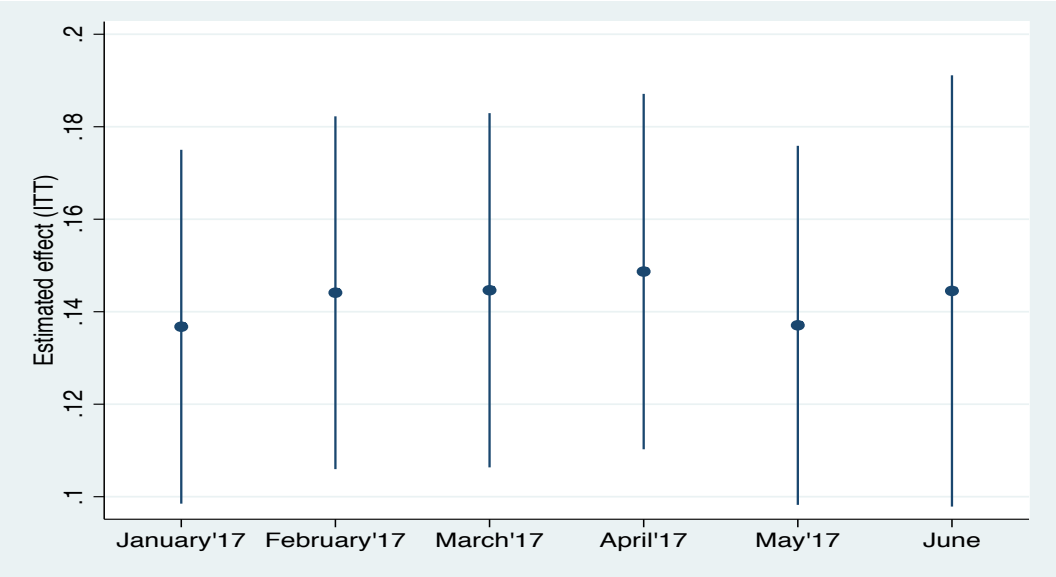
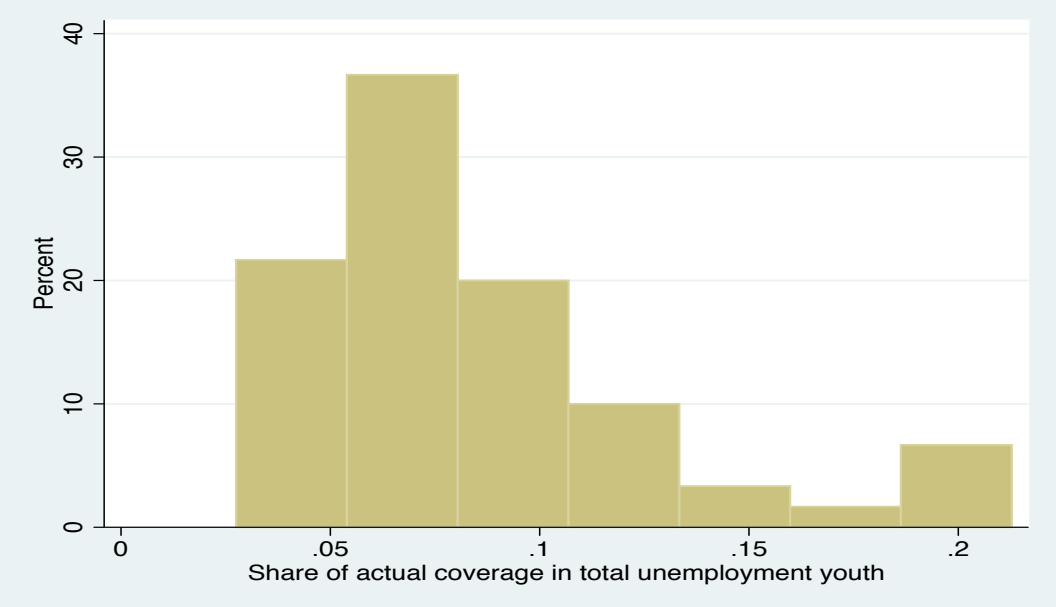


Figure 1.2: Dynamics of employment effects (ITT effects)



Note: Each dot represents point estimate for respect month. Vertical line shows 95% confidence interval.

Figure 1.3: Distribution branch offices by share of program coverage (final coverage)



1.9 Tables

Table 1.1: Baseline characteristics of sample youth

Characteristics	(1)	(2)
	Males	Females
Owed land (decimals)	17.90	14.54
No. of cows owned	0.51	0.40
No. of goats owned	0.42	0.40
Household head's education (years)	2.11	2.25
Household head's gender (male=1; female=0)	0.88	0.87
Household head's age (years)	47.23	47.24
Youth is unmarried (yes=1, no=0)	0.02	0.21
Youth is enrolled in school (yes=1, no=0)	0.10	0.10
Youth's education (years)	4.99	5.72
Youth's age (years)	16.18	16.94
Youth is employed (yes=1, no=0)	0.29	0.19
Youth's hours of work per day (unconditional)	0.21	0.04
Youth's earnings (BDT/month)	132.04	13.74
N	1,201	1,745

Note: asset ownership is at the household level.

Table 1.2: Balancing test of randomization

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Baseline characteristics	Assigned to on-the-job training (yes=1, no=0)	p-value	Assigned to combined on-the-job and classroom training (yes=1, no=0)	p-value	Constant	N	Branch fixed effects	R-sq
Owned land (decimal)	-2.109	0.40	0.793	0.616	15.14***	2946	Yes	0.096
	(2.508)		(1.583)		(3.929)			
No. of cows owned	0.0196	0.713	-0.0447	0.475	0.586***	2946	Yes	0.057
	(0.0531)		(0.0626)		(0.170)			
Household head's education (years)	0.0123	0.944	0.165	0.34	1.037**	2946	Yes	0.059
	(0.176)		(0.176)		(0.426)			
Household head's age (years)	-0.376	0.511	-0.271	0.67	50.14***	2946	Yes	0.044
	(0.573)		(0.648)		(1.381)			
Youth has savings (yes=1, no=0)	0.0124	0.304	0.0127	0.328	0.0245	2946	Yes	0.031
	(0.0121)		(0.013)		(0.0312)			
Youth is married (yes=1, no=0)	0.00169	0.919	0.00656	0.751	0.278***	2946	Yes	0.063
	(0.0166)		(0.0207)		(0.0813)			
Youth's education (years)	-0.309**	0.029	-0.358**	0.016	4.628***	2946	Yes	0.108
	(0.142)		(0.149)		(0.359)			
Youth's age (years)	-0.155	0.246	-0.260**	0.048	16.79***	2946	Yes	0.078
	(0.134)		(0.131)		(0.337)			
Youth's gender (male=1, female=0)	0.0551**	0.035	-0.0175	0.539	0.322***	2946	Yes	0.05
	(0.0262)		(0.0285)		(0.0842)			
Youth is employed ((yes=1, no=0)	-0.0193	0.334	-0.0325	0.126	0.0798*	2946	Yes	0.262
	(0.02)		(0.0212)		(0.0447)			
Youth's hours of work (per day)	-0.0355	0.104	-0.0195	0.374	0.0358	2946	Yes	0.09
	(0.0218)		(0.0219)		(0.026)			
Youth's earnings (BDT, per month)	12	0.619	-18.91	0.162	19.16*	2946	Yes	0.043
	(24.09)		(13.53)		(9.89)			

* p<0.10, ** p<0.05, *** p<0.01

Table 1.3: Characteristics of MCPs

Variable	Mean
Total business capital (BDT)	243,032
Firm's space (square feet)	218.2
Education (years)	7.9
Age (years)	34.1
No. of rooms used by the firm	1.2
Employees (male)	0.4
Employees (female)	1.7
Total employees	2.1
No. of observations	586

Table 1.4: Effects of on-the-job vs combined classroom and on-the-job training

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Self-employed (yes=1, no=0)		Wage employed (yes=1, no=0)		Employed (yes=1, no=0)		Hours of work (per day) in self-employment	
Panel A: ITT effects								
Effect of on-the-job training	0.0525***	0.0564***	0.0951***	0.0746***	0.140***	0.124***	0.242**	0.235**
	(0.0200)	(0.0198)	(0.0242)	(0.0223)	(0.0260)	(0.0237)	(0.109)	(0.108)
Effect of combined classroom and on-the job training	0.0233	0.0311*	0.112***	0.116***	0.131***	0.143***	-0.0179	0.0177
	(0.0186)	(0.0182)	(0.0276)	(0.0256)	(0.0288)	(0.0269)	(0.0838)	(0.0829)
Additional effect of classroom training	-0.0292	-0.02521	0.0172	0.04138	-0.0090	0.0180	-0.259*	-0.2170
	(0.0272)	(0.0268)	(0.0360)	(0.0330)	(0.0388)	(0.0357)	(0.1378)	(0.1354)
R-squared	0.097	0.120	0.063	0.210	0.089	0.229	0.050	0.073
Panel B: TOT effects (IV results)								
Effect of on-the-job training	0.0846***	0.0907***	0.153***	0.120***	0.226***	0.200***	0.389**	0.378**
	(0.0319)	(0.0315)	(0.0383)	(0.0352)	(0.0410)	(0.0372)	(0.175)	(0.173)
Effect of combined classroom and on-the job training	0.0381	0.0512*	0.183***	0.190***	0.214***	0.234***	-0.0293	0.0300
	(0.0301)	(0.0295)	(0.0437)	(0.0403)	(0.0461)	(0.0427)	(0.135)	(0.134)
Additional effect of classroom training	-0.0465	-0.0394	0.03013	0.07017	-0.0118	0.0335	-0.418*	-0.3478
	(0.0438)	(0.0430)	(0.0581)	(0.0532)	(0.0616)	(0.0560)	(0.2208)	(0.2160)
R-squared	0.097	0.120	0.078	0.229	0.105	0.251	0.049	0.074
Branch fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2946	2946	2946	2946	2946	2946	2946	2946
Control group mean at follow up	0.13	0.13	0.26	0.26	0.38	0.38	0.5	0.5

Standard errors in parentheses. Baseline characteristics include marital status, age, gender, education, employment status, time devoted to earning activity, and earnings.
* p<0.10, ** p<0.05, *** p<0.01

Table 1.4: Effects of on-the-job vs combined classroom and on-the job training (contd.)

	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	Hours of work (per day) in wage employment		Total hours of work (per day)		Earnings from self-employment (BDT/month)		Earnings from wage employment (BDT/month)		Total earnings (BDT/month)	
Panel A: ITT effects										
Effect of on-the-job training	0.64***	0.47***	0.88***	0.71***	126	118	361**	261*	487***	379**
	(0.185)	(0.171)	(0.197)	(0.177)	(102.4)	(102.4)	(151.2)	(139)	(169.8)	(153.8)
Effect of combined classroom and on-the job training	0.64***	0.65***	0.62***	0.67***	-124.8	-83.34	212.3	265.1	87.54	181.7
	(0.220)	(0.204)	(0.221)	(0.203)	(86.07)	(82.20)	(203.2)	(187)	(212.7)	(190.7)
Additional effect of classroom training	0.0006	0.1806	-0.2589	-0.0363	-251.2*	-201.5	-148.35	4.54	-399.5	-196.9
	(0.287)	(0.266)	(0.2960)	(0.2694)	(133.7)	(129.03)	(253.24)	(232.9)	(272.2)	(244.2)
R-squared	0.058	0.193	0.061	0.222	0.038	0.076	0.045	0.175	0.052	0.220
Panel B: TOT effects (IV results)										
Effect of on-the-job training	1.02***	0.76***	1.41***	1.13***	204	190	581**	419*	784***	609**
	(0.293)	(0.271)	(0.311)	(0.279)	(163)	(162.8)	(240.3)	(221)	(269.6)	(243.5)
Effect of combined classroom and on-the-job training	1.04***	1.07***	1.01***	1.10***	-203.6	-135.9	346.3	434.8	142.8	298.9
	(0.349)	(0.323)	(0.353)	(0.323)	(138.9)	(133.0)	(326.9)	(300.2)	(342.9)	(307.3)
Additional effect of classroom training	0.0142	0.3109	-0.4041	-0.0368	-407.1*	-326.1	-234.5	15.85	-641.6	-310.3
	(0.456)	(0.421)	(0.471)	(0.425)	(214.2)	(206.2)	(405.7)	(373)	(436.2)	(390.4)
R-squared	0.073	0.210	0.077	0.242	0.038	0.076	0.047	0.180	0.054	0.225
Branch fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2946	2946	2946	2946	2946	2946	2946	2946	2946	2946
Control group mean at follow up	1.87	1.87	2.37	2.37	416	416	1369	1369	1786	1786

Standard errors in parentheses. Baseline characteristics include marital status, age, gender, education, employment status, time devoted to earning activity, and earnings.
* p<0.10, ** p<0.05, *** p<0.01

Table 1.5: Effects of training at the extensive and intensive margins (ITT effects)

	(1)	(2)	(3)	(4)	(5)	(6)
	Employed (yes=1, no=0)		Hours of work (per day)		Total earnings (BDT/month)	
Panel A: Intensive margin						
Effect of training	0.0692*	0.0568	0.313	0.161	14.20	-54.71
	(0.0392)	(0.0361)	(0.307)	(0.275)	(328.2)	(290.3)
R-squared	0.187	0.346	0.152	0.321	0.193	0.349
Observations	718	718	718	718	718	718
Control group mean at follow up	0.53	0.53	3.24	3.24	2795.75	2795.75
Panel B: Extensive margin						
Effect of training	0.155***	0.150***	0.872***	0.812***	389.2***	359.9***
	(0.0225)	(0.0210)	(0.171)	(0.158)	(143.0)	(131.0)
R-squared	0.092	0.201	0.077	0.212	0.062	0.190
Observations	2228	2228	2228	2228	2228	2228
Control group mean at follow up	0.34	0.34	2.09	2.09	1459.96	1459.96
Branch fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Baseline characteristics	No	Yes	No	Yes	No	Yes

Standard errors in parentheses. Baseline characteristics include marital status, age, gender, education, employment status, time devoted to earning activity, and earnings.
* p<0.10, ** p<0.05, *** p<0.01

Table 1.6: Gender-disaggregated effects of training (ITT effects)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Self-employed (yes=1, no=0)		Wage-employed (yes=1, no=0)		Employed (yes=1, no=0)		Hours of work (per day) in self-employment	
Effect for females	0.0665***	0.0734***	0.113***	0.111***	0.177***	0.182***	0.25***	0.276***
	(0.0175)	(0.0172)	(0.0203)	(0.0203)	(0.0236)	(0.0234)	(0.0626)	(0.0637)
Effect for males	-0.0067	-0.00127	0.0746**	0.0737**	0.0580**	0.0622**	-0.1195	-0.104
	(0.02185)	(0.0216)	(0.0299)	(0.0295)	(0.0282)	(0.0278)	(0.1434)	(0.1423)
Additional effect for males	-0.0733***	-0.0746***	-0.0381	-0.0368	-0.119***	-0.119***	-0.370**	-0.380**
	(0.0280)	(0.0276)	(0.0361)	(0.0358)	(0.0368)	(0.0363)	(0.157)	(0.156)
Branch fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline characteristics	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2946	2946	2946	2946	2946	2946	2946	2946
R-squared	0.099	0.122	0.199	0.210	0.214	0.232	0.060	0.075
Control group mean at follow up (females)	0.11	0.11	0.11	0.11	0.22	0.22	0.28	0.28
Control group mean at follow up (males)	0.17	0.17	0.50	0.50	0.65	0.65	0.83	0.83

Standard errors in parentheses. Baseline characteristics include marital status, age, gender, education, employment status, time devoted to earning activity, and earnings. Regression equation for results reported in odd-numbered columns includes an indicator variable for gender.
* p<0.10, ** p<0.05, *** p<0.01

Table 1.6: Gender-disaggregated effects of training (ITT effects) (contd.)

	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	Hours of work (per day) in wage employment		Total hours of work (per day)		Earnings from self-employment (BDT/month)		Earnings from wage employment (BDT/month)		Total earnings (BDT/month)	
Effect for females	0.60***	0.56***	0.85***	0.84***	175***	205***	219**	239**	394***	443***
	(0.148)	(0.148)	(0.152)	(0.152)	(53.2)	(56.0)	(102.4)	(103.2)	(108.8)	(110.5)
Effect for males	0.575**	0.560**	0.4559*	0.4560*	-289**	-269*	289	299	0.333	30.4
	(0.253)	(0.2502)	(0.2522)	(0.2500)	(146.6)	(144.3)	(263.9)	(260.5)	(275.9)	(270.3)
Additional effect for males	-0.0202	-0.0039	-0.390	-0.384	-464***	-473***	70.8	60.4	-394	-413
	(0.294)	(0.291)	(0.295)	(0.293)	(156.7)	(155.7)	(288.0)	(287.3)	(301.0)	(298.3)
Branch fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline characteristics	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2946	2946	2946	2946	2946	2946	2946	2946	2946	2946
R-squared	0.182	0.193	0.212	0.223	0.057	0.079	0.160	0.175	0.194	0.220
Control group mean at follow up (females)	0.81	0.81	1.09	1.09	156.42	156.42	577.76	577.76	734.19	734.19
Control group mean at follow up (males)	3.54	3.54	4.38	4.38	822.22	822.22	2606.84	2606.84	3429.06	3429.1

Standard errors in parentheses. Baseline characteristics include marital status, age, gender, education, employment status, time devoted to earning activity, and earnings. Regression equation for results reported in odd-numbered columns includes an indicator variable for gender.
* p<0.10, ** p<0.05, *** p<0.01

Table 1.7: Effects of training on employment in MCPs' firms (ITT effects)

	(1)	(2)	(3)	(4)	(5)	(6)
	Wage employment (anywhere)		Wage employment (in MCPs' firm)		Wage employment (anywhere except in MCPs' firm)	
Effect of training	0.104***	0.0958***	0.0840***	0.0821***	0.0199	0.0137
	(0.0184)	(0.0171)	(0.0102)	(0.0102)	(0.0173)	(0.0160)
Branch fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Baseline characteristics	No	Yes	No	Yes	No	Yes
Observations	2946	2946	2946	2946	2946	2946
R-squared	0.062	0.210	0.090	0.099	0.045	0.186
Control group mean at follow up	0.262	0.262	0.0286	0.0286	0.233	0.233

Standard errors in parentheses. Baseline characteristics include marital status, age, gender, education, employment status, time devoted to earning activity, and earnings.
* p<0.10, ** p<0.05, *** p<0.01

Table 1.8: Effects of training on migration (ITT effects)

	Migrated (yes=1, no=0)			
	(1)	(2)	(3)	(4)
	Full sample youth		Youth aged 17 years or more	
Effect for females	-0.00292	-0.00312	0.00990	0.0112
	(0.00719)	(0.00718)	(0.0138)	(0.0149)
Effect for males	0.00307	0.0027	0.0552*	0.0579*
	(0.012)	(0.012)	(0.0302)	(0.0309)
Additional effect for males	0.00600	0.00587	0.0453	0.0468
	(0.0140)	(0.0139)	(0.0323)	(0.0331)
Branch fixed effects	Yes	Yes	Yes	Yes
Baseline characteristics	No	Yes	No	Yes
Observations	2946	2946	881	881
R-squared	0.042	0.046	0.120	0.122
Control group mean at follow up (males	0.0447	0.0447	0.0372	0.0372
Control group mean at follow up (females)	0.020	0.020	0.0235	0.0235

Standard errors in parentheses. Baseline characteristics include marital status, age, gender, education, employment status, time devoted to earning activity, and earnings. Regression equation for results reported in odd-numbered columns includes an indicator variable for gender.
* p<0.10, ** p<0.05, *** p<0.01

Table 1.9: Effects on wellbeing and asset accumulation (ITT effects)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Psychological wellbeing index		No. of shirts/ dress#		Pairs of shoes#		Own cell phone (Yes=1, No=0)		Own sewing machine (Yes=1, No=0)		Savings (BDT)	
Effect of training	0.10**	0.11**	0.12	0.16*	0.045	0.06**	0.045*	0.055***	0.024*	0.032**	599	697
	(0.045)	(0.045)	(0.087)	(0.086)	(0.033)	(0.032)	(0.023)	(0.021)	(0.013)	(0.01)	(758)	(782)
Branch fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline characteristics	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2100	2100	2077	2077	2074	2074	2100	2100	2946	2946	2946	2946
R-squared	0.124	0.147	0.213	0.240	0.120	0.164	0.065	0.225	0.047	0.077	0.017	0.019
Control group mean at follow up	0	0	4.77	4.77	1.97	1.97	0.39	0.39	0.113	0.113	991.9	991.9

Standard errors in parentheses. Baseline characteristics include marital status, age, gender, education, employment status, time devoted to earning activity, and earnings.
#Top 1% observations dropped. Results reported in columns 1-8 are based on information collected from the youth interviewed in person at follow up survey.
* p<0.10, ** p<0.05, *** p<0.01

Table 1.10: Effects of training on control group's employment

	(1)	(2)	(3)
	Employed (Yes=1, No=0)		
Training_{ibld}	0.130***	0.129***	0.0749**
	(0.0216)	(0.0215)	(0.0353)
$Percentcov_{bld}$		-0.00121	-0.00378
		(0.0043)	(0.0046)
$\text{Training}_{ibld} * Percentcov_{bld}$			0.00584
			(0.0039)
Urban dummy	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes
Baseline characteristics	Yes	Yes	Yes
Observations	2946	2946	2946
R-squared	0.207	0.207	0.208
Mean coverage (%)	8.2	8.2	8.2
Control group mean at follow up	0.132	0.262	0.385

Standard errors in parentheses, clustered at the branch office level. * p<0.10, ** p<0.05, *** p<0.01

1.10 Appendix Tables

Table A1.1: Sample size and attrition

	Baseline		Follow up		Attrition (%)	
	Treatment	Control	Treatment	Control	Treatment	Control
Males	653	644	607	594	7.04	7.76
Females	930	962	858	887	7.74	7.80
Total	1583	1606	1465	1481	7.45	7.78

Table A1.2: Correlates of attrition

	(1)	(2)	(3)
	Attrition (yes=1, no=0)		
Treatment (yes=1, no=0)	-0.00846	-0.00944	0.0270
	(0.0106)	(0.0106)	(0.0828)
Age of youth (years)		-0.00290	-0.00154
		(0.00260)	(0.00372)
Education of youth (years)		-0.000692	-0.00130
		(0.00154)	(0.00235)
Education of household head (years)		-0.000295	-0.000530
		(0.000874)	(0.000971)
Youth is employed (yes=1, no=0)		-0.00819	-0.0165
		(0.0151)	(0.0196)
Hours of work of youth (per day)		0.00873	-0.00332
		(0.0145)	(0.00873)
Gender of youth (male=1, female=0)		0.00140	-0.00157
		(0.0114)	(0.0159)
Marital status of youth (married=1, unmarried=0)		0.0103	0.0206
		(0.0191)	(0.0285)
Treatment*Age of youth			-0.00317
			(0.00502)
Treatment*Education of youth			0.00122
			(0.00288)
Treatment*Education of household head			0.00111
			(0.00196)
Treatment*Youth is employed			0.0182
			(0.0257)
Treatment*Hours of work of youth			0.0267
			(0.0300)
Treatment*Gender of youth			0.00559
			(0.0229)
Treatment*Marital status of youth			-0.0198
			(0.0375)
Constant	0.0389	0.0957*	0.0800
	(0.0250)	(0.0507)	(0.0668)
Branch fixed effects	Yes	Yes	Yes
Observations	3186	3186	3186
R-squared	0.105	0.106	0.107

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.01

Table A1.3: Program participation rate (%)

	Males	Females	All
On-the-job training	63.1	64.8	64.1
Combined on-the-job and classroom training	52.2	61.9	58.1
All	58.3	63.4	61.3

Table A1.4: First stage results

	(1)	(2)	(3)	(4)
	Participated in on-the-job training (Yes=1, No=0)		Participated in combined on-the-job and classroom training (Yes=1, No=0)	
Assigned to on-the-job training	0.621***	0.6210***	0.00033	0
	(0.0183)	(.0183)	(0.0015)	(3.57e-10)
Assigned to combined on-the-job and classroom training	-0.0015	1.38e-1***	0.6110***	(0.6130***
	(0.0013)	(3.00e-1)	(0.0193)	(.0195)
Branch level fixed effects	Yes	Yes	Yes	Yes
Baseline characteristics	Yes	No	Yes	No

Standard errors in parentheses. Baseline characteristics include marital status, age, gender, education, employment status, time devoted to earning activity, and earnings.
* p<0.10, ** p<0.05, *** p<0.01

Table A1.5: Effects of training on time (hours/day) devoted to earning activities (disaggregated analysis, ITT effects)

	Panel A: Hours of work (per day) in wage employment							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Mobile phone servicing		Tailoring		Working as beautician		Mason/wooden furniture making	
Effect for females	0.019	0.0161	0.573***	0.562***	0.129**	0.132**	0.0221	0.006
	(0.017)	(0.0179)	(0.0899)	(0.0882)	(0.063)	(0.0632)	(0.0230)	(0.025)
Effect for males	0.29***	0.29***	0.181	0.1750	0.0056	0.0065	0.3330**	0.33**
	(0.088)	(0.088)	(0.123)	(0.1239)	(0.012)	(0.0132)	(0.155)	(0.1525)
Additional effect for males	0.27***	0.27***	-0.39**	-0.387**	-0.12**	-0.125**	0.311**	0.324**
	(0.091)	(0.0919)	(0.154)	(0.153)	(0.062)	(0.0621)	(0.156)	(0.155)
Branch fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Characteristics	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2946	2946	2946	2946	2946	2946	2946	2946
R-squared	0.048	0.049	0.067	0.069	0.047	0.050	0.141	0.154

Standard errors in parentheses. Baseline characteristics include marital status, age, gender, education, employment status, time devoted to earning activity, and earnings. Regression equation for results reported in odd-numbered columns includes an indicator variable for gender.
* p<0.10, ** p<0.05, *** p<0.01

Table A1.5: Effects of training on time (hours/day) devoted to earning activities (disaggregated analysis, ITT effects) (contd.)

	Panel A: Hours of work (per day) in wage employment (contd.)				Panel B: Hours of work (per day) in self-employment					
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	Working for Garments industry		Others		Tailoring		Small business		Others	
Effect for females	-0.17**	-0.18**	0.0286	0.0316	0.253***	0.262***	0.027*	0.031**	-0.0300	-0.0169
	(0.08)	(0.08)	(0.073)	(0.074)	(0.0460)	(0.0460)	(0.013)	(0.0149)	(0.0423)	(0.0440)
Effect for males	-0.095	-0.096	-0.145	-0.149	-0.00075	0.0031	-0.0816	-0.0838	-0.0371	-0.0232
	(0.08)	(0.08)	(0.211)	(0.211)	(0.0096)	(0.0101)	(0.070)	(0.0708)	(0.1276)	(0.1268)
Additional effect for males	0.0811	0.0864	-0.174	-0.181	-0.25***	-0.25***	-0.109	-0.115	-0.00716	-0.00639
	(0.121)	(0.12)	(0.222)	(0.222)	(0.0473)	(0.0473)	(0.073)	(0.0732)	(0.135)	(0.134)
Branch fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline characteristics	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2946	2946	2946	2946	2946	2946	2946	2946	2946	2946
R-squared	0.069	0.080	0.112	0.114	0.060	0.063	0.034	0.045	0.070	0.075

Standard errors in parentheses. Baseline characteristics include marital status, age, gender, education, employment status, time devoted to earning activity, and earnings. Regression equation for results reported in odd-numbered columns includes an indicator variable for gender.
* p<0.10, ** p<0.05, *** p<0.01

Table A1.6: Heterogeneity of training effects for females with respect to marital status (ITT effects)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Self-employed (yes=1, no=0)		Wage employed (yes=1, no=0)		Employed (yes=1, no=0)		Hours of work (per day) in self-employment	
Effect for unmarried females	0.0704***	0.0763***	0.144***	0.145***	0.211***	0.218***	0.277***	0.290***
	(0.0183)	(0.0182)	(0.0222)	(0.0221)	(0.0257)	(0.0254)	(0.0637)	(0.0638)
Effect for married females	0.0536	0.0625	-0.005	-0.0056	0.0460	0.0543	0.1519	0.17564
	(0.0426)	(0.0424)	(0.0448)	(0.04536)	(0.0537)	(0.0543)	(0.1666)	(0.1694)
Additional effect for married females	-0.0168	-0.0137	-0.149***	-0.151***	-0.165***	-0.164***	-0.125	-0.114
	(0.0455)	(0.0453)	(0.0492)	(0.0497)	(0.0589)	(0.0594)	(0.181)	(0.180)
Branch fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline characteristics	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1745	1745	1745	1745	1745	1745	1745	1745
R-squared	0.114	0.123	0.089	0.094	0.137	0.147	0.083	0.088
Control group mean at follow up (unmarried)	0.08	0.08	0.09	0.09	0.18	0.18	0.2	0.2
Control group mean at follow up (married)	0.19	0.19	0.19	0.19	0.38	0.38	0.61	0.61

Standard errors in parentheses. Baseline characteristics include marital status, age, gender, education, employment status, time devoted to earning activity, and earnings. Regression equation for results reported in odd-numbered columns includes an indicator variable for marital status.
* p<0.10, ** p<0.05, *** p<0.01

Table A1.6: Heterogeneity of training effects for females with respect to marital status (ITT effects) (contd.)

	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	Hours of work (per day) in wage employment		Total hours of work (per day)		Earnings from self-employment (BDT/Month)		Earnings from wage employment (BDT/Month)		Total earnings (BDT/Month)	
Effect for unmarried Females	0.81***	0.81***	1.09***	1.10***	191***	197***	326***	343***	518***	540***
	(0.158)	(0.156)	(0.162)	(0.160)	(48.65)	(48.20)	(101.6)	(98.89)	(108.2)	(105.3)
Effect for married Females	-0.2173	-0.2251	-0.065	-0.0495	174.25	187.991	-229.44	-210.5	-55.182	-22.522
	(0.361)	(0.363)	(0.369)	(0.372)	(175.4)	(182.88)	(246.4)	(249.78)	(280.8)	(287.2)
Additional effect for married females	-1.03***	-1.04***	-1.2***	-1.2***	-16.9	-8.8	-556**	-554**	-573*	-563*
	(0.389)	(0.392)	(0.401)	(0.403)	(187.2)	(187.2)	(267.1)	(269.7)	(304.6)	(306.8)
Branch fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline characteristics	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1745	1745	1745	1745	1745	1745	1745	1745	1745	1745
R-squared	0.083	0.086	0.104	0.109	0.069	0.073	0.091	0.098	0.115	0.126
Control group mean at follow up (unmarried)	0.64	0.64	0.84	0.84	106.19	106.19	464.3	464.3	570.5	570.5
Control group mean at follow up (married)	1.46	1.46	2.08	2.08	356.97	356.97	1030.8	1030.8	1387.8	1387.8

Standard errors in parentheses. Baseline characteristics include marital status, age, gender, education, employment status, time devoted to earning activity, and earnings. Regression equation for results reported in odd-numbered columns includes an indicator variable for marital status.

* p<0.10, ** p<0.05, *** p<0.01

Table A1.7: Correlates of share of program coverage

Regressors (youth's baseline characteristics)	Dependent variable: Share (%) of program coverage in total unemployed youth
Marital status (married=1, unmarried=0)	-0.330*
	(0.191)
Education (years)	-0.0678***
	(0.0227)
Age (years)	-0.0237
	(0.0243)
Gender (male=1; female=0)	-0.267**
	(0.125)
Employed (yes=1, no=0)	0.388**
	(0.164)
Hours of work (per day)	-0.0155
	(0.129)
Earnings (BDT/month)	-0.00007
	(0.00008)
Baseline characteristics	Yes
Dummy for urban	Yes
Districts fixed effects	Yes
Observations	2946
R-squared	0.763

Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Chapter 2

Credit and Contracting: A Test of Theory[1]

2.1 Introduction

A share contract is inferior to a fixed rent contract under first-best conditions, a phenomenon famously known as the Marshallian inefficiency of sharecropping.[2] A voluminous literature provides theoretical explanations on the resulting puzzle of the widespread existence of sharecropping, examining the role of various deviations from the first best (Quibria and Rashid, 1984; Hayami and Otsuka, 1993). Among those, Laffont and Matoussi (1995) show that a liquidity constraint on tenants increases the prevalence of share contracting relative to fixed rent. Shetty (1988) develops a theoretical model where sharecropping is explained by the risk of ex-post liquidity constraints, as a fixed rent could not be fully paid in bad states of nature. Braverman and Stiglitz (1989) show that, in a general equilibrium model of land allocation, when credit is rationed a capital intensive technological change can induce a long-term increase in concentration of land ownership and in sharecropping arrangements as tenants are capital constrained, reducing productivity. Relaxation of a credit constraint can thus be an effective way of reducing the incidence of second-best efficient share contracts. Theory also explores the role of other deviations from the first best in explaining the superiority of a sharecropping over a fixed rent contract. One is risk, with the tenant more risk-averse than the landlord, and insurance market failure (Stiglitz, 1974). In this case, the landlord can reduce risk for the tenant by increasing his own share of the product and absorbing more risk, but this creates more Marshallian disincentive to effort for the tenant. Hence, there is an optimum share that balances the efficiency loss from risk aversion with the efficiency loss due to disincentive to effort. Another is missing markets for an input such as farm management capacity when it is non-contractible and more efficiently performed by the landlord than by the tenant (Eswaran and Kotwal, 1985). Relaxation of the credit constraint in inducing the choice of a fixed rent over a share rent contract should thus be more intense when risk and non-contractability of missing inputs are less constraining on the tenant. This is what we explore empirically in this paper.

While the theory of sharecropping as a potentially second-best efficient contract when the tenant is liquidity constrained is well established, rigorous empirical evidence on the effect of liquidity constraint on the choice of land contract is limited. Laffont and Matoussi (1995) is the best-known study that investigates this empirically. They estimate a structural model, with liquidity measured as the amount of working capital (available monetary liquidity and rental value of equipment owned). Difficulty with their empirical analysis is with the presumed exogeneity of the tenant's and landlord's working capital in the estimated choice equation. In

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[1] The material in this chapter is from an unpublished work, coauthored with Alain de Janvry and Elisabeth Sadoulet.

[2] There is substantial empirical evidence on the Marshallian inefficiency of sharecropping (Bell, 1977; Shaban, 1987). There are, however, special conditions under which sharecropping can be first-best efficient such as observable and enforceable effort without risk (Cheung, 1969) and cooperation due to kinship ties between landlord and tenant (Sadoulet *et al.,* 1997).

this paper, we estimate the effect of credit on the choice of land contract using a randomized experiment built in a program organized by BRAC in Bangladesh that offered credit to landless workers and smallholder farmers. Further, guided by the theoretical predictions that the prevalence of sharecropping can be explained by tenant's exposure to uninsured risk and deficit in farm management capacity, we investigate whether the effect of credit on the choice of land contract is heterogeneous with respect to these two sources of market failure.

We find that the effect of access to credit on taking fixed rent contracts is positive and large in magnitude for both the number of contracts and the area contracted while the effect on taking share contracts is small and statistically insignificant for both outcomes. This effect is found to be heterogeneous, with larger responses for tenants that are in contexts with less risky weather conditions. We do not find empirical support for the prediction that response would be larger for tenants that are less constrained by farm management capacity.

This paper contributes to the literature on credit markets and sharecropping following the seminal paper of Laffont and Matoussi (1995). We deviate from this and other studies by using data from a randomized experiment of a unique credit program for landless workers and smallholder farmers. As such, we provide the first rigorous empirical evidence of the role of a financial constraint on contract choice, as well as of the mitigating role of risk in responding to a relaxation of the liquidity constraint.

The remainder of this paper proceeds as follows. In section 2.2, we review the theoretical literature explaining the mechanisms through which a liquidity constraint, risk, and management capacity affect the choice of land contract, and use this to formulate four key testable hypotheses. In section 2.3, we discuss land rental contracts in Bangladesh and present the evaluation design for the BRAC credit program. Section 2.4 explains the data used in this study, how variables are measured, and gives descriptive statistics. Section 2.5 specifies the estimated equations and presents the results for the testable hypotheses. Section 2.6 concludes.

2.2 Theory and Testable Hypotheses

The theory of how a liquidity constraint affects the choice of land contract was developed by Laffont and Matoussi (1995). They consider a principal-agent model, where the tenant has limited access to working capital and his effective labor is unobservable to the landlord. The landlord chooses the terms of the contract (a general contract with a share of the product kept by the tenant, a share of the inputs paid by the tenant, and a certain payment made by the tenant to the landlord), under the tenant's incentive and participation constraints. The trade-off is between relaxing the tenant's credit constraint by reducing the fixed payment and his share of input costs, and increasing the disincentive to effort that the corresponding reduction of the product share entails. The model predicts the following: (1) the landlord's utility level rises with the tenant's working capital; (2) conditionally on the level of other inputs, the level of effective labor, and therefore production, are increasing in the tenant's working capital; and (3) conditionally on the level of other inputs, the tenant's output share in the contract is increasing in the tenant's working capital. Hence, if the tenant's working capital is sufficiently high, his/her crop share becomes one and the landlord gets a fixed rent. For lower levels of working capital, the optimal

contract is a sharecropping arrangement. While the solution is derived conditional on the level of other inputs, the authors presume that the result holds unconditionally. The main prediction of the model is thus that the less binding the liquidity constraint is on the tenant, the greater the prevalence of fixed rent over share contracting. In this study, we empirically test this proposition.

The theoretical literature on sharecropping also emphasizes the role of risk in explaining the prevalence of share contracts when insurance markets are failing and when the tenant is not able or willing to absorb all the production risk. Stiglitz (1974) shows that, by defining rent as a share of output, sharecropping allows risk sharing between landlord and tenant as the rent paid varies with the stochastic level of output achieved. Together, the theories of Laffont and Matoussi (1995) and Stiglitz (1974) imply that the effect of credit on fixed relative to share rent contracts is larger for tenants that are less risk-averse or are less exposed to risk.

Eswaran and Kotwal (1985) show that the choice of a share contract can also be explained by the existence of non-contractible inputs such as labor supervision and management capacity which are assumed to be more efficiently provided by the tenant and the landlord, respectively. According to them, if labor supervision is not important or is efficiently provided by the landlord, the best option for him is to manage the farm directly and to hire labor. If management is not important or is efficiently provided by the tenant, then the best contract for the landlord is a fixed rent. If both management and supervision matter, and the tenant is more efficient at supervising while the landlord is more efficient at managing, then sharecropping is the best option. The landlord manages, the tenant supervises, and they share output. Together, the theories of Laffont and Matoussi (1995) and Eswaran and Kotwal (1985) would imply that the effect of credit on fixed relative to share rent contracts is larger for tenants that are able to manage the farm more efficiently.

To summarize, the testable hypotheses for this study are the following three:

(1) Hypothesis 1: Access to credit increases the prevalence of fixed rent relative to share rent contracts.
(2) Hypothesis 2: The effect of credit on the prevalence of fixed relative to share rents is larger for less risk-averse or less risk-exposed tenants.
(3) Hypothesis 3: The effect of access to credit on the prevalence of fixed relative to share rents is larger for tenants who have more baseline experience in managing land.

2.3 The Credit Program and Research Design

In 2010, BRAC, a Bangladeshi NGO providing micro-finance and social programs, started a credit program for landless workers (with some farming experience as tenants) and smallholder farmers. The program is operated in the traditional microfinance framework but provides unique advantages: a low interest rate (the effective interest rate is 20% compared to the traditional microfinance interest rate of about 25%), monthly repayments, and lower levels of installment for the first four months. To be eligible for the credit program, a household must meet the following six criteria: (i) has a national ID card; (ii) age between 18-60 years; (iii) an education level not more than grade 10; (iv) permanent residence in the targeted area for at least three years; (iv) at least three years of farming experience; (v) a total holding size including rented

land if any between 33 and 200 decimals (a decimal is 1/100 of an acre); and (vi) not be a member of any NGO. The targeting criterion related to land holding indicates that the program is intended to serve smallholder farmers including landless tenants, small owner farmers, and mixed tenants (who cultivate both own and rented land).[3] BRAC customized the intervention by providing extension services to borrowers. Extension services include training in modern cropping techniques and livestock husbandry methods; and information regarding product and input markets through VO (village organization)[4] meetings, over the phone, and through individualized field visits. Borrowers are required to save BDT 50 per month. Furthermore, 5% of the total amount is deducted as security savings at the time of loan disbursement.

For the purpose of evaluation, BRAC's introduction of the program in 2012 followed an experimental design over 40 branch offices (each covering a geographical area of about 5-6 km radius from the BRAC local office) from 22 districts.[5] BRAC's Research and Evaluation Division (RED) randomly selected 20 branches for intervention and the remaining 20 branches to serve as controls.[6] The map showing the treatment and control sites can be found in Hossain *et al.* (2014, pp. 18-19).[7] At the onset of the program, RED carried out a village census in six randomly selected villages from each of the treated and control branches, and eligible households were identified based on the program's targeting criteria. The census thus identified 7,563 eligible households in the 40 branch offices. From the list of eligible households, 4,301 households were randomly selected for the household survey, almost equally divided between treated and control areas. After completion of the baseline survey, BRAC offered credit to the eligible households in the treatment branch offices. An eligible household can take credit for the following purposes: (i) general credit for working capital (amounting to BDT 5,000-30,000); (ii) credit to purchase machinery (BDT 30,000-120,000); and (iii) credit for land leasing/mortgaging (BDT 30,000-60,000).[8] In this paper, we estimate the effect on land contracts of all three types of credits as money is fungible.

Because the program offered extension services to participants, the effect of the program on land contract, if any, may also be driven by the extension services. We show that this is secondary as only 7% of program participants in the sample received extension services. For this reason, throughout the paper, we use the term "effect of credit" in referring to the impact of the program.

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[3]In Bangladesh, households owning less than 50 decimals of lands (0.2 ha) are considered to be functionally landless (Scott and Islam, 2008).

[4]Like in much of microfinance lending, this credit program is operated through group formation, known as village organizations (VO). Members are grouped in teams of five members, and four to eight teams consisting of 20 to 40 members form a VO (Hossain *et al.,* 2014). VOs serve as solidarity groups, but do not have joint liability over individual loans.

[5]During 2010-2012 (up to October) the program covered 646,000 households (Hossain *et al.,* 2014).

[6] The evaluation design is also explained in Hossain *et al.* (2014) and Malek *et al.* (2015).

[7]Available at http://www.ruralfinance.org/fileadmin/templates/rflc/documents/BRAC__Credit__Impact__Assessm__for__Tenant__Farmers,__RCT__2009-2012.pdf

[8]In 2014, the exchange rate with the US$ was BDT 77.64.

The cropping pattern in Bangladesh is related to the rainfall cycle. The monsoon season extends from mid-June to mid-October, with the rest of the year a continuous dry period where agriculture is practiced with underground water irrigation. In a year, crops are grown over three seasons: the Aman season generally extends from July-August to November-December and basically depends on monsoon rainfall; the Boro season extends from December-January to April-May and depends on tube well irrigation; and the Aus season bridges the other two between March-April and June-July, also depending on irrigation.[9] Rice is by far the most important crop contributing 61% of total crop value (Ahmed, 2004). Of the three seasons, Boro is the most important for rice, contributing 55% of annual output, while Aman contributes 38%, and Aus 7%. In this study, we focus on the Boro season for land rental activities as it is the most important cropping period and it is the one for which we have the most data (as explained below).

2.4 Data and Descriptive Statistics

A baseline survey was conducted in June-August, 2012, covering 4,301 households (2,155 households from treatment areas and 2,146 from control areas) randomly selected from among eligible households. A follow up survey was administered in June-August, 2014, successfully revisiting 4,141 households (2,072 households from treatment areas and 2,069 from control areas). The overall attrition rate was low (3.72%). Table A2.1 reports the results of an OLS regression of attrition on the treatment indicator and the number of fixed rent and share contracts. Results show that all the estimated coefficients are small and statistically insignificant, indicating that there is no significant difference in the attrition rate between treatment and control areas, and that the baseline outcome variables are not correlated with attrition.

The survey collected information for the last three cropping seasons on the number of plots they cultivated and for each plot, whether owned or rented. For plots reported as rented, information on the types of contracts was recorded. Information on cost and returns including rental rate and duration of land contract was collected for the largest plot from each sample household. Respondent for the survey was the household head.

Twenty percent of the eligible households successfully revisited from treatment areas participated in the credit program offered by BRAC. Forty nine percent of the participant households took one loan from BRAC, 49.6% two loans, and the rest 1.5% three loans. Average size of loans taken by sample participant households was BDT 20,282 (US$ 261.2 at the 2014 exchange rate). Of the participant households, 88% took their first loan in 2013 and 12% in 2014. Among those who took second and third loans, 76% did so in 2014. The timing of the follow-up survey (in June-August) is such that data on Aman season refer to 2013, the first year of the program, while Boro season is 2014. We therefore focus on Boro, when the program has had a bit more time to take-off.

[9]https://www.pecad.fas.usda.gov/cropexplorer/pecad_stories.aspx?regionid=bg&ftype=prodbriefs. These three seasons area also called Kharif 2, Rabi, and Kharif 1, respectively.

In the baseline Boro season, 60% of the rented plots were cultivated under sharecropping arrangements, and the rest were cultivated under seasonal fixed rent (paid in cash and crop), mortgage, and other (gift, using others' land without rent) arrangements.

Throughout the year in Bangladesh, the main sources of risk for rice cultivation are drought, flood, extreme temperatures, and pests (Shelley et al., 2016). During the dry Boro season on which we focus, all rice cultivation is under irrigation, and extreme temperatures are the main risk. Rice grows normally within a temperature range of 20°C to 35°C, and is particularly sensitive to low temperature in its initial stage (March) and high temperature in its final stage (April). To characterize weather at the branch level, we matched each of the 40 branches in the experiment with the closest of 16 weather stations.[10] We verify in Table A2.2 that, in our 2012-2014 panel data for the control villages, yield is indeed negatively affected by temperature dropping below 20°C in March and by temperature exceeding 35°C in April (although when jointly estimated, the high temperature in April dominates). These events are cross-sectionally important. In 2012, 43.8% of the observations had March temperatures below 20°C and 20.4% April temperatures above 35°C. The corresponding figures for 2014 are 77.2% and 79.6%, respectively. The low temperature event reduces yield by 0.57 kg/decimal (23 kg/ha) and the high temperature event by 1.8 kg/decimal (73 kg/ha) (which correspond to 2.8 and 8.8% of the mean yield, respectively). We therefore characterize the risk associated with low and high temperatures by the probability of facing a temperature below 20°C in March and above 35°C in April, respectively, measured by the proportion of years that experienced this temperature in the 10-year period 2003-2012.

Management capacity is measured by the area of land owned and under fixed rent contract at baseline. These are lands where the farmer is the entrepreneur, by contrast to land under share contract where management may be provided by the landlord.

We report in Table 2.1 baseline characteristics for the surveyed households. Asset endowments are very low, with the average amount of land owned equal to 0.25 ha and 3.1 years of education for the household head. The median household in our sample owned 0.16 hectares of land at baseline, indicating that more than half the sample households were defined as functionally landless (see footnote 2). Land rental contracts are quite frequent, with 56% of the households engaged in contracting. Sharecropping is the most prevalent, with 60% of the contracts share rent and 40% fixed rent. 32% of the households have only share contracts and 19% only fixed rent, with 5% engaged in both types of contracts. At the national level, Hossain *et al.* (2014) report that 41% of the land rental contracts are sharecropping. Since the sample for our study represents relatively poorer households, these findings suggest that it is poorer households that are more likely to use share contracts.

Table A2.3 presents the differences in baseline means for the key observable characteristics of the survey households between treatment and control groups. Standard errors of the differences are clustered at the BRAC branch office level (the unit of randomization). None of the differences in baseline means of the outcome variables are statistically significant, and most are small, suggesting that randomization is balanced across observables. One exception however,

[10] Temperature data were collected from Bangladesh Bureau of Statistics (various issues of statistical pocketbook and monthly statistical bulletin).

the difference is quite large for the number of fixed rent contracts (0.498 and 0.587 for the treatment and control group, respectively).

Table 2.2 presents descriptive statistics on the level of rents for fixed rent and share contracts. The amount deposited with the landlord for renting land through the mortgage system was BDT 478,496 per hectare. If we consider the micro-credit interest rate (25% for ASA, one of the large MFIs in Bangladesh) as the market rate of interest, then the landlord earning for the 4 months mortgage was BDT 39,811 (478,496*0.0208*4 i.e., deposit amount *monthly interest rate *4) per hectare. [11] For fixed rent contracts with seasonal cash rent, the amount of rent paid to the landlord was BDT 41,584 per hectare. For share contracts, the landlord's output share was 46.9% and landlord borne 11% of input costs. From the data, the gross value of output per hectare under sharecropping was this year BDT 80,891 while costs were BDT 47,768. Hence, the landlord's return per hectare of land under sharecropping was BDT 32,683 (i.e., 80,891*0.469-47,768*0.11). These statistics suggest that the landlord's gain on average is perhaps higher from fixed rent contracts. The survey did not collect information on the duration of a share contract, but other studies report that share contracts are of short duration, usually one year (Jansen, 1986, as cited by Reiersen, 2004).[12] Information in Table 2.2 also shows that, in a sharecropping contract, landlords contribute a small proportion (11%) of total input costs which include all inputs else than family labor.

2.5 Results and Discussion

2.5.1 Impact of Access to Credit on Contract Choice (Hypothesis 1)

Descriptive statistics showed that the differences in baseline means of the outcome variables between treated and control groups are all statistically insignificant, but the magnitude of the difference is quite large for fixed rent contracts. For this reason, we use a difference-in-differences specification controlling for household fixed effects to estimate the causal effect of the intervention:

$$y_{ijt} = \alpha_i + \delta_t + \beta Tassigned_j * Post_t + \epsilon_{ijt} \quad (2.1)$$

where y_{ijt} is the outcome variable of interest for household *i* in branch office *j* at time period *t*. Time periods refer to 2012 (for baseline) and 2014 (for follow up). $Tassigned_j$ takes the value of 1 if branch office *j* is assigned to treatment and zero for control. $Post_t$ is an indicator variable taking the value of 0 if t = 2012 and 1 if t = 2014. α_i are household fixed effects while δ_t is a time fixed effect. ϵ_{ijt} is an error term, clustered at the BRAC branch office level, the unit of randomization. The parameter β identifies the causal ITT effect of the intervention.

[11] http://www.asa.org.bd/loan-products/

[12]Analysis of land contracts in two consecutive seasons (Aman season followed by Boro season) at baseline shows that among sharecropping contracts in the Aman season, 19% were discontinued and 5% were switched to fixed rent contract in the following season (not shown in the Table). Of the fixed rent contracts (mortgage system with undetermined duration of contract) in the Aman season, 8% of contracts were discontinued in the next season.

We also estimate TOT effects using an instrumental variable approach. The estimating equation is:

$$y_{ijt} = \mu_i + \gamma_t + \varphi Treated_{ij} * Post_t + e_{ijt} \quad (2.2)$$

where $Treated_{ij}$ takes the value of 1 if household i has participated in the program, μ_i are household fixed effects, γ_t is a time fixed effect, and e_{ijt} is an error term clustered at the BRAC branch office level. The parameter φ identifies the causal ToT effect of the intervention. Since not all eligible households from treated areas participated in the program, $Treated_{ij}$ (and hence, $Treated_{ij} * Post_t$) is endogenous. $Treated_{ij} * Post_t$ is instrumented on $Tassigned_j * Post_t$. We estimate the following equation for the first stage:

$$Treated_{ij} * Post_t = v_i + \rho_t + \eta Tassigned_j * Post_t + u_{ijt} \quad (2.3)$$

where, v_i are household fixed effects, ρ_t is a time fixed effect, and u_{ijt} is an error term clustered at the BRAC branch office level.

Table 2.3 reports the estimated effects of the intervention on the number of share and fixed rent contracts. Panel A presents the results of estimating equation (2.1) and panel B equations (2.2) and (2.3). Columns 1, 2, and 3 of Table 2.3 report the effects on share, fixed rent, and total number of land contracts, respectively. Both OLS and IV results show that the effect of the intervention on share contracts is statistically insignificant and very small in magnitude (column 1). The effect of the intervention on fixed rent contracts is positive and statistically significant at the 5% level (column 2). The magnitude of the effect is quite large—the IV point estimate is about 1.69 times the mean of the outcome variable in the control villages at follow-up survey. These results indicate that access to credit increases fixed rent relative to share contract and that this effect is achieved mainly through an increase in fixed rent contracts. Beneficiary households have nearly one additional plot (0.947) in fixed rent. These findings are in line with our testable hypothesis based on the theory developed by Laffont and Matoussi (1995) and are consistent with their empirical results. Regression results presented in column 3 of Table 2.3 show that the effect of the intervention on the total number of contracts is positive and large, although not statistically significant.

First stage results in Panel B show that the estimated coefficient on the interaction term is highly significant (at the 1% level). Point estimate of this coefficient is consistent with the fact that about 20% of those that are assigned to treatment eventually participated in the credit program.

We also analyze in Table 2.4 the change in the total area of land under share and fixed rent contracts due to access to credit, using the same specifications (2.1) to (2.3). Results in column 2 indicate that the program increases the land area under fixed rent contracts, by 29.5%, in line with the increase in the number of plots estimated in Table 2.3. For the land area under share contracts, the effect of credit is very small and statistically insignificant (column 1). Hence,

access to credit increases the total number of plots and area under fixed relative to share rent contracts, which is the key testable hypothesis of this study.

We analyze in Table 2.5 whether the relaxation of the liquidity constraint affects differentially the intensive and extensive margins in fixed rent contracting. The intensive margin is characterized by individuals with baseline fixed-rent contract experience. These individuals had the interest and ability to contract without the credit provided by the program. The extensive margin, by contrast, is characterized by individuals with no baseline fixed-rent contract experience. We find that both margins display a positive increase in fixed rent contracting. The effect for individuals with contracting experience at baseline (intensive margin), is equal to 0.221 additional plots, larger although not significantly so, than the 0.134 plots for the extensive margin.

2.5.2 Heterogeneity of Effects with respect to Risk and Management Capacity

We extend equation (2.1) to estimate heterogeneity of effects with respect to risk and management capacity:

$$y_{ijt} = b_i + \lambda_t + \theta_1 Tassigned_j * Post_t + \theta_2 Tassigned_j * Post_t * x_{ij} + \theta_3 Post_t * x_{ij} + \in_{ijt} \quad (2.4)$$

where x_{ij} is a measure of baseline exposure to risk or management capacity for individual *i* in branch office *j*, b_i are household fixed effects, λ_t is a time fixed effect, and $\in_{ijt}$ is an error term clustered at the BRAC branch office level. The parameters of interest are θ_1 and θ_2. Equation (2.4) is estimated using OLS. Like equation (2.1), equation (2.4) estimates the ITT effect of the intervention. We expect θ_2 to be negative for fixed rent contract and positive for share contract when x_{ij} measures exposure to risk. We expect the opposite for θ_2 when x_{ij} measures baseline management experience as owner-operator or fixed rent tenant.

2.5.2.1 Role of Risk in Contracting (Hypothesis 2)

Table 2.6 shows how exposure to risk affects the impact of access to credit on land contract, with risk measured by exposure to extreme temperatures during the *boro* rice growing season: the proportion of years in 2003-2012 in which the temperature in March was below 20°C, and the proportion of years in which the temperature in April was above 35°C. We find that risk associated with exposure to low temperature has a large negative effect on the impact of credit on fixed rent contracting. Risk associated with high temperatures does not have a significant effect on the role of credit on contracting. When considered together, the low temperature risk is the one that dominates on the role of credit in contacting, with a large and highly significant coefficient. To see the order of magnitude of this effect, consider that the distribution of risk of cold temperature is almost bimodal with probabilities of either 0.2-0.3 or 0.6-0.7, and mean value 0.424. The regression results suggest that in area with a 30% probability of low temperature (low risk) the credit program induces an increase of fixed rent rental by (0.495-0.3*0.696) 0.286 plot, significant at 1%, while there is no effect at all in a high-risk area with a

70% chance of cold weather.

2.5.2.2 Role of Management Capacity in Contracting (Hypothesis 3)

Tenant's management capacity can be measured by the area directly managed under ownership or fixed rent contracts at baseline. Different specifications of management experience with fixed rent contracts included annual fixed rent contracts, maximum seasonal fixed rent area, and Boro fixed rent rentals. Land management experience was classified into four levels: no direct management experience (these would be landless tenants under sharecropping with 37% of the sample), and managed land area (the remaining 63% divided into three terciles: < 60 decimals, 60-122 decimals, and > 122 decimals). We found (results not reported) no statistical difference between these effects across the four groups, suggesting that heterogeneity in management experience is not sufficiently powerful to argue that it affects the response of contracting to access to credit.

Compared to risk, as proposed by Stiglitz (1974), which is a widespread phenomenon originating in many sources but with similar consequences, non-contractible management as a landlord asset, as proposed by Eswaran and Kotwal (1985), takes on many forms that will likely affect contracting differently according to farming demands and circumstances. It is consequently more difficult to capture empirically. The fact that we do not find prior management experience as statistically relevant here does not mean that it has no theoretical relevance, only that we would need a finer data collection strategy to give it empirical support.

2.6 Conclusion

A voluminous literature provides theoretical explanations on the Marshallian puzzle of sharecropping (Hayami and Otsuka, 1993). Because a share contract is inferior to a fixed rent contract in eliciting effort under first-best conditions, sharecropping should not prevail, posing the puzzle of its widespread existence. The literature has explored conditions of market failure under which a share contract may be preferred to a fixed rent contract. In addition to risk with insurance market failure (Stiglitz, 1974) and missing markets for non-contractible inputs not owned by the tenant such as management capacity (Eswaran and Kotwal, 1985), the role of a financial constraint was proposed by Laffont and Matoussi (1995). Rigorous empirical evidence on the latter channel is however still missing. This paper advances our quantitative knowledge on the puzzle of sharecropping by using a randomized experiment of the effect of access to credit on the choice of land contract in the context of Bangladesh where almost half of the land tenure arrangements are share contracts.

We find evidence that the effect of access to credit for potential tenants on the number of, and area under, fixed rent contracts is positive, significant, and large in magnitude. The effect of credit on share contract is very small and statistically insignificant. As predicted by theory, greater access to liquidity thus increases the share of fixed rent relative to share rent contracts in the total number of contracts (Hypothesis 1). The effect was found to be heterogeneous, being

larger for those who have more baseline contractual experience (intensive margin) compared to those without baseline contracts (extensive margin). Further, results indicate that the effect of credit on fixed relative to share rent contracts is larger for those who are less exposed to production risk as measured by the incidence of extreme temperatures, a result consistent with Stiglitz's (1974) theoretical prediction (Hypothesis 2). With the data we have, we do not find statistical evidence that non-contractible inputs such as management experience possessed by the tenant leads to greater preference for fixed rent contracts as the liquidity constraint is being relaxed, as suggested by Eswaran and Kotwal (1985) (Hypothesis 3).

A large empirical literature documents the inefficiency of sharecropping relative to fixed rent contracts (Shaban, 1987; Bell, 1977). The findings of our study suggest that improved access to credit for landless tenants and smallholder farmers can reduce the inefficiency of the land contracting system by increasing fixed rent relative to share rent contracts. Microfinance programs that help relax liquidity constraints on land rental can thus be sources of efficiency gains for the rural poor through the choice of better contracts.

2.7 Tables

Table 2.1: Baseline characteristics of surveyed households

	Mean value
Household head is male (yes=1, no=0)	0.933
Age of household head (years)	44.8
Years of education of household head	3.1
Primary occupation of household head is agriculture (yes=1, no=0)	0.654
Household is food secured (yes=1, no=0)	0.791
House has electricity connection (yes=1, no=0)	0.594
Amount of owned land (ha)	0.25
Land contracts	
Total number of land contracts	1.35
Number of share contracts	0.81
Number of fixed rent contracts	0.54
Household has no land contract	0.44
Household has only sharecropping contracts	0.32
Household has only fixed rent contracts	0.19
Household has both contracts	0.05
Observations	4141

Table 2.2: Rents/deposits for fixed rent and share contracts (baseline, *boro* season)

	(1)	(2)	(3)	(4)
	Fixed rent contract through mortgage*	Fixed rent contract with seasonal rent in cash	Fixed rent contract with seasonal rent in kind**	Share contract***
Number of plots	380	265	76	1479
Money deposited with landlord (BDT per hectare)	478,496			
Seasonal fixed rent in cash (BDT per hectare)		41,584		
Seasonal fixed rent in kind (cash equivalent, BDT per hectare)			31,135	
Crop share to the landlord (%)				46.9%
Landlord share of input costs				11%

*The duration of the contract was not pre-specified, except in 18 of these contracts. **This amount is about 35% of the total values of outputs. ***Landlord's share of input costs is from follow up survey. Baseline survey did not collect detailed information on this. The exchange rate in 2011 was 75 BDT per USD.
Source: Baseline survey

Table 2.3: Effects of credit on the number of land contracts

	(1)	(2)	(3)
	Number of share contracts	Number of fixed rent contracts	Total number of contracts
Panel A: ITT			
Assigned*Post	0.004	0.190**	0.194
	(0.075)	(0.090)	(0.125)
Post	-0.157***	-0.0251	-0.182**
	(0.057)	(0.041)	(0.069)
Individual fixed effects	Yes	Yes	Yes
R-squared	0.011	0.008	0.005
Panel B: TOT (IV regression)			
Treated*Post (instrumented)	0.0204	0.947*	0.968
	(0.366)	(0.501)	(0.665)
Post	-0.157***	-0.0251	-0.182***
	(0.056)	(0.041)	(0.068)
First stage: Treated*Post			
Assigned*Post	0.201***	0.201***	0.201***
	(0.027)	(0.027)	(0.027)
Individual fixed effects	Yes	Yes	Yes
Observations	8282	8282	8282
Mean outcome in control group at follow up	0.662	0.562	1.223

Standard errors in parentheses, clustered at the branch office level
* p<0.10, ** p<0.05, *** p<0.01

Table 2.4: Effects of credit on the land area under share and fixed rent contracts

	(1)	(2)	(3)
	Log of land area under share contracts	Log of land area under fixed rent contracts	Log of total land area under all contracts
Panel A: ITT			
Assigned*Post	-0.0236	0.295**	0.162
	(0.097)	(0.121)	(0.137)
Post	-0.230***	0.0382	-0.188***
	(0.064)	(0.057)	(0.054)
Individual fixed effects	Yes	Yes	Yes
R-squared	0.014	0.015	0.004
Panel B: TOT (IV regression)			
Treated*Post (instrumented)	-0.118	1.471**	0.806
	(0.482)	(0.685)	(0.694)
Post	-0.230***	0.0382	-0.188***
	(0.063)	(0.056)	(0.053)
Individual fixed effects	Yes	Yes	Yes
Observations	8282	8282	8282
Mean outcome in control areas at follow up	1.223	1.08	2.128

Standard errors in parentheses, clustered at the branch office level
* p<0.10, ** p<0.05, *** p<0.01

Table 2.5: Effects of credit on the number of land contracts: Extensive and intensive margins

	(1)	(2)	(3)
	Number of share contracts	Number of fixed rent contracts	Total number of contracts
ITT			
Assigned*Post	0.0155	0.221*	0.237
	(0.131)	(0.123)	(0.171)
Assigned*Post*No baseline fixed-rent contract	-0.0568	-0.0869	-0.144
	(0.145)	(0.122)	(0.168)
Post	-0.490***	-0.202***	-0.692***
	(0.095)	(0.066)	(0.095)
Post*No baseline fixed-rent contract	0.781***	0.415***	1.196***
	(0.111)	(0.075)	(0.100)
Individual fixed effects	Yes	Yes	Yes
Observations	8282	8282	8282
R-squared	0.074	0.028	0.108
Ass. Post + Ass. Post No baseline FR contract	-0.041	0.134*	0.093
	(0.050)	(0.069)	(0.080)

Standard errors in parentheses, clustered at the branch office level
* p<0.10, ** p<0.05, *** p<0.01

Table 2.6: Effects of credit on the number of land contracts: heterogeneity by temperature risk exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Low temperature risk			High temperature risk			Low/High temperature risk		
Number of contracts:	Share	Fixed	Total	Share	Fixed	Total	Share	Fixed	Total
Assigned*Post	0.0441	0.495***	0.539***	0.0714	0.162	0.234	-0.00147	0.550***	0.549**
	(0.130)	(0.150)	(0.184)	(0.0886)	(0.108)	(0.161)	(0.130)	(0.159)	(0.223)
Assigned*Post*Low temp. risk	-0.0951	-0.696**	-0.791*				0.178	-0.811***	-0.633
	(0.274)	(0.306)	(0.412)				(0.265)	(0.283)	(0.422)
Assigned*Post*High temp. risk				-0.0184	0.139	0.120	-0.0047	-0.183	-0.188
				(0.264)	(0.317)	(0.468)	(0.283)	(0.305)	(0.527)
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8282	8282	8282	8282	8282	8282	8282	8282	8282
R-squared	0.011	0.014	0.009	0.015	0.008	0.008	0.015	0.015	0.011

Standard errors in parentheses, clustered at the branch office level**.** * p<0.10, ** p<0.05, *** p<0.01
Low temperature risk is the proportion of years in 2003-2012 in which the lowest temperature in March was below 20 degree Celsius. High temperature risk is the proportion of years in which the highest temperature in April was above 35 degree Celsius
Variables Post, Post*Low temp. risk, and Post*High temp. risk included

2.8 Appendix

Table A2.1: Correlates of attrition, OLS regression results

	Attrition	Attrition
Assigned to treatment	0.00263	0.00235
	(0.00727)	(0.00720)
Number of share contracts at baseline		-0.00343
		(0.00206)
Number of fixed rent contract at baseline		-0.00204
		(0.00228)
Constant	0.0359***	0.0399***
	(0.00524)	(0.00557)
Observations	4301	4301
R-squared	0.000	0.001

Standard errors in parentheses, clustered at the branch office level
* p<0.10, ** p<0.05, *** p<0.01

Table A2.2: Correlation between extreme temperatures and yield

	(1)	(2)	(3)
	Yield (Kg/Decimal)	Yield (Kg/Decimal)	Yield (Kg/Decimal)
Lowest temp. in March < 20 degree Celsius	-0.572**		0.439
	(0.246)		(0.288)
Highest temp. in April > 35 degree Celsius		-1.829***	-2.091***
		(0.270)	(0.319)
Year fixed effects	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes
Mean value of yield	20.88	20.88	20.88
Observations	3164	3164	3164
R-squared	0.124	0.148	0.149

Standard errors in parentheses, clustered at the branch office level
* p<0.10, ** p<0.05, *** p<0.01
Temperature in March is below 20 degree Celsius for 43.8 and 77.2 % of the observations in year 2012 and 2014, respectively.
Temperature in April is above 35 degree Celsius for 20.4 and 79.6 % of the observations

Table A2.3: Balance of covariates

	(1)	(2)	(3)	(4)
	Treatment	Control	Difference	p-value on a test of equality of means
Household head is male	0.9170	0.9483	-0.0313	0.086
	(0.0061)	(0.0049)	(0.0178)	
Age of household head (years)	45.2011	44.4460	0.7551	0.289
	(0.2510)	(0.2609)	(0.7027)	
Years of education of household head	3.2124	3.0184	0.1940	0.547
	(0.0761)	(0.0734)	(0.31901)	
Primary occupation of household head is agriculture	0.6400	0.6689	-0.0290	0.64
	(0.0105)	(0.0103)	(0.0615)	
Household is food secured	0.7799	0.8023	-0.0224	0.729
	(0.0091)	(0.0088)	(0.0643)	
House has electricity connection	0.5869	0.6003	-0.0134	0.838
	(0.0108)	(0.0108)	(0.0654)	
Amount of owned land (ha)	0.241	0.256	-0.014	0.354
	(0.006)	(0.006)	(0.015)	
Land contracts				
Total number of contracts	1.2978	1.4055	-0.1077	0.4910
	(0.0364)	(0.0395)	(0.1550)	
Number of share contracts	0.8002	0.8188	-0.0186	0.903
	(0.0298)	(0.0324)	(0.1506)	
Number of fixed rent contracts	0.4976	0.5868	-0.0892	0.5230
	(0.0258)	(0.0270)	(0.1385)	
Household has no land contract	0.4445	0.4268	0.0177	0.710
	(0.0109)	(0.0109)	(0.0474)	
Household has only Share-cropping contract	0.3296	0.3050	0.0247	0.640
	(0.0103)	(0.0101)	(0.0523)	
Household has only fixed rent contract	0.1810	0.2073	-0.0264	0.590
	(0.0085)	(0.0089)	(0.0485)	
Household has both contracts	0.0449	0.0609	-0.0160	0.105
	(0.0045)	(0.0053)	(0.0097)	
Observations	2072	2069		

* p<0.10, ** p<0.05, *** p<0.01

Note: Standard errors in parentheses. Standard errors of the differences are clustered at the branch office level

Chapter 3

Reducing the Risk of Migration: An Evaluation of BRAC's Safe Migration Program[13]

3.1 Introduction

Economic returns to migration can be very high (Bryan *et al.,* 2014; Halliday, 2006). Migration can also reduce poverty (Beegle *et al.,* 2011; Afsar *et al.*, 2002). But many poor people do not engage in such profitable behavior. In Bangladesh, for instance, only 5 percent of households in *monga* (seasonal food insecurity) prone districts receive domestic remittances against 22 percent among all Bangladeshi households (Bryan *et al.,* 2014). Similarly, the poorest Europeans from the poorest regions were the ones who did not choose to migrate during a period in which 60 million Europeans left for the New World (Hatton and Williamson, 1998). Ardington *et al.* (2009) also provide similar evidence for rural South Africa.

Several studies emphasize the role of risk in explaining why more people do not move despite high returns to migration. Bryan *et al.* (2014) show that the risk of not matching to an employer after migration holds back the poor from migrating. ILO (2013), on the other hand, shows that migrants face the risks of falling victim to human trafficking and forced labor.[14] While these studies focus on risk after departure, a recent study (Das *et al.*, 2014) emphasizes pre-departure risk, and shows that around a third of migration attempts from Bangladesh end in failure with significant financial losses for already poor households. Figure 3.1 (reproduced from Das *et al.*, 2014) reports the distribution of financial losses of failed migrants. The information indicates that failures impose a huge cost on failed migrants, with a median loss of $250, which is about 24% of annual earnings for an average Bangladeshi household. We also analyzed the time it takes for aspiring migrants to eventually succeed, and showed that only 25% of those trying to migrate succeeded in migrating in six months, 50% in a year, and after 36 months 40% have failed to achieve their goal and are still trying. These findings are likely to suggest that risk reduction policy may have substantial benefits through encouraging migration.

In this paper, we investigate whether providing information along with administrative and community support to aspiring migrants throughout the migration process reduces the risk of migration. The risks we focus on include: (1) the risk of failure to depart home country after having spent resources during the process; and (2) the risk of not finding the expected job or salary abroad.

The role of risk in holding back migration and lessening the success of migration has been noted in the literature. Bryan *et al.* (2014) use a randomized controlled trial in Bangladesh to show that a small cash transfer to rural households close to subsistence can induce a large

[13]The material in this chapter is from an unpublished work, coauthored with Alain de Janvry and Elisabeth Sadoulet.
[14]Kleemans (2014) also focuses on risk, but she shows that migration is used as an ex-post risk-coping strategy.

response in seasonal rural-urban migration. They attribute this effect to the risk of not matching to an employer after migration, thus holding back the very poor from migrating. The proposed study examines whether other instruments for risk reduction (information, and administrative and community support) can have an impact on the success of international migration.

Much attention has also been given to the role of social networks in reducing the risk of migration (Massey and España, 1987; Munshi, 2003). Evidence shows that those with family or community networks face lower risks and costs of migration, indicating that network is a powerful source of incentives in attempting to migrate. Results also show that as more people from a community succeed in migrating, community network becomes a more important source of support for migration compared to family networks, spreading the opportunity to migrate across community members (Winters *et al.*, 2001). For black workers after Emancipation in the United States, social networks that formed among workers in large plantations were decisive in the ability to successfully migrate to the North (Chay and Munshi, 2013). Our study assesses whether programs that provide information, and administrative and community support to aspiring migrants are successful in establishing community networks for individuals who do not have the support of a family or community.

Chiquiar and Hanson (2005) look at selectivity in international migration from Mexico to the United States. Economic logic predicts that lower-skilled workers should migrate most. However, this is not what they observe. For men, it is those with intermediate level of skills who migrate most. For women, it is those with the highest level of skills. A possible explanation has to do with the differential role of networks, credit constraints, and ability to take risks in migration across skills, preventing the least skilled men and women from migrating. In this study, we look at how reducing the risk of migration through information, and administrative and community support to aspiring migrants can change the composition of migrants.

Beam *et al.* (2016) show that assisting individuals through a jobs website and passport assistance increases job-search effort and the likelihood of obtaining a job interview but it does not increase actual migration. Our study is closely related to Beam *et al.* (2016) but the program we study in this paper provides administrative as well as community support.

The intervention we study in this paper was implemented in Bangladesh by BRAC, the largest NGO in the world. We randomized the intervention at the union level - the lowest administrative unit in the country. Our results show that the intervention has no statistically significant effect on overall migration success or failure. Similarly, the program has no significant effect on salary received abroad or cost of migration. Further results, however, show that it reduces migration failure for areas where poverty rate is relatively higher (significant at the 10% level). Moreover, the program reduces the costs of migration failure as well as the costs of migration among individuals that attempt to migrate through informal channels. These effects are statistically significant at the 10% level. We also find some weak evidence that due to the intervention, more people from areas where poverty rate is relatively higher attempt to migrate.

Overall, we find little effect of the intervention, but additional results show that the program is more likely to decrease migration failure for areas where the level of education of volunteers

engaged in the program is higher. It is also more likely to increase migration success for areas where the proportion of volunteers that are returning migrants or migrant family members is higher. These results suggest that the Safe Migration Program might be more effective if it engages more educated individuals and/or more returning migrants/migrant family members as volunteers.

Rest of the paper is organized as follows. After the introduction, section 3.2 describes the context and the intervention, while section 3.3 formulates the testable hypotheses. Section 3.4 discusses the evaluation design and data collection, and provides descriptive statistics including balancing test of the randomization. Section 3.5 discusses the results while concluding remarks are made in section 3.6.

3.2 The Context and BRAC's Safe Migration Program

3.2.1 The Context

Remittance is a major source of fund for investment, and plays a vital role in supporting consumption levels and reducing poverty in Bangladesh. It makes a substantial contribution to the economy, amounting to $14.5 billion in 2013 or 10% of GDP, which is about twelve times the level of foreign direct investment and the second largest source of foreign exchange after garment exports (Bangladesh Bank, 2014). Remittances also substantially augment a recipient household's income, consumption and saving. Poverty headcount rates of remittance-receiving households in Bangladesh are 61% lower than of those who do not receive remittances (World Bank, 2012). While 21% of migrant households were moderately poor prior to migration, in the post-migration period this proportion was dramatically reduced to 7% (Afsar *et al.*, 2002), demonstrating the value of remittances for inclusive growth.

The key government agency involved in the labor migration process is the Bureau of Manpower Employment and Training (BMET) under the Ministry of Expatriate Welfare and Overseas Employment (MEWOE). All job seekers need to register in BMET, which provides workers with an emigration clearance (commonly known as SMART card). BMET has, however, limited capacity to provide support to potential migrants. Better regulation of manpower agencies, and information on the costs of migration, overseas job conditions and migrant's rights could help the poor make more informed choices at each step of the migration process.

Generally, social networks and private recruitment agencies are responsible for migrants' job matching (Martin, 2010), and Bangladesh is not an exception to this (Rahman, 2011). Therefore, aspiring migrants who do not have networks with current or returning migrants are likely to rely on recruitment agencies. In Bangladesh, most recruitment agencies are located in two big cities (Dhaka and Chittagong) of the country, and they heavily depend on a group of middlemen or intermediaries (commonly known as *dalals*) to recruit workers from rural areas (Rahman, 2011). However, some middlemen often defraud aspiring migrants. ILO (2014), for example, notes that, "Many migrant workers do not go through their contract papers. Indeed, recruiting agents, sub-agents, and dalals circumvent the obligation of showing the job contract to recruited workers.

And even if the recruited worker tries to go through the contract paper, language barriers and highly technical terms within the paper hinder them in comprehending the terms and conditions". Institutionalized information campaigns and awareness raising initiatives are rarely available in Bangladesh (Siddiqui *et al.,* 2008), and thus, aspiring migrants have to go through layers of other contacts (BRAC University, 2012). Research results also show that fraudulent agent/fake visa is one of the self-reported causes of migration failure (Das *et al.*, 2014).

3.2.2 The Intervention and Target Population

In response to the risks of migration, particularly for already vulnerable households, BRAC started a Safe Migration Program (SMP) in 2006 in 36 sub-districts of Bangladesh. The main objective of this program is to improve safety of the migration process in migrant prone communities by (i) providing better access to accurate and timely information, along with services, for a safe migration, and (ii) strengthening community-based organizations (CBOs) in order to reduce migrants' dependency on middlemen. Based on its field experience with this program, BRAC scaled up the SMP to 80 additional sub-districts in 2014 with financial support from the Japan Social Development Fund (JSDF) through the World Bank. Under the project, BRAC engaged local CBOs and other registered grassroots institutions in each of the 80 sub-districts to implement the program. Key program activities for the CBOs and BRAC include the following:

1. Mass awareness campaign: This informed and mobilized the broader community to support migrant workers and improve knowledge of migration issues. Modalities included community-based activities such as courtyard meetings, interactive popular theater (IPT) and video shows on safe migration.

2. Pre-migration decision information: This was provided to potential/aspiring migrants and their families, which is expected to enable them to assess the social and economic costs, and benefits of migration, and to provide an overview of the migration process in Bangladesh. The pre-decision awareness orientation was announced through the mass awareness campaign and open to all those who are interested. Each group consisted of 25 potential migrants at most. The local CBOs and NGOs conducted the training. The aspirants were informed of the costs that are usually incurred by migrants for obtaining visa, airfare and other documents, and amount of salary that might receive abroad.

3. Language and skills training: Interested parties were linked with skill enhancement training programs at government Technical Training Centers and other private training centers. Basic language training programs were developed through several mechanisms, including village-based courses led by returning migrants.

4. Financial management training: Aspiring migrants were linked to affordable financial instruments to help cover the upfront costs of migration. Training on the costs and benefits of such financial services was also offered. Microfinance and remittance

utilization training modules educated migrants and their families on the use of financial instruments to finance pre-departure costs and manage remittances.

5. Pre-departure orientation: In this orientation, individuals who have already made the decision to migrate and secured placements received practical information about travel to the host country, whom to contact if they encounter problems while abroad, what their rights are in the receiving country, and issues to consider before returning home, such as health, financial, or travel-related information.

6. Life skills training for female migrants: Women migrants received additional training on specific issues women typically face in the process of migration.

The primary beneficiaries of the SMP expansion are the households of poor and low-skilled Bangladeshi workers who seek employment abroad. The project aimed at benefiting 864,000 potential migrants and their families, who received training and orientation programs in 80 upazilas.

The institutional structure that was put into place for the project is as follows:

- There were 10 Safe Migration Facilitation Centers, staffed by CBO facilitators, each responsible for CBO capacity building and supporting service delivery in a two-district area.
- Each sub-district had one CBO with the responsibility of carrying out the program.
- The CBOs were governed by seven members, one of whom was the Migration Associate, who worked full time on CBO activities and received a stipend through the project.
- Six Migration Volunteers per sub-district were trained on Safe Migration Counseling to provide support at the village level in coordination with the CBO Facilitators and Migration Associates working at the district and sub-district levels, respectively.
- In each village a migration forum was formed to assist aspiring migrants through community resource mobilization. Each forum consisted of about 30 members, half of whom were aspiring migrants and the rest half includes influential people from the community. The forum provided support to aspiring migrants through providing information about migration. It also assisted failed migrants to recover the money they have already deposited to middlemen and/or recruiting agencies.
- At the union level, community volunteers were selected. These volunteers were trained so that they can conduct the BCC (Behavior Change Communication) activities at the grass root level, such as courtyard meetings and interpersonal communications. Little incentives were provided to the community volunteers. The volunteers also worked to organize other mass awareness activities.

3.3 Testable Hypotheses

Evidence shows that those with strong community networks face lower risks in migration (Massey and Garcia Espana, 1987; Munshi, 2003). Since the SMP mobilizes community support to establish networks between aspiring migrants and their communities, the program may reduce pre-departure risk, the risk of failure to depart home country. Similarly, the program may reduce the cost of migration failure.

The program activities related to linkage of aspiring migrants with training institutions, basic language training, and improving knowledge of migration related issues may reduce post-departure risks, the risks of not having a job with expected salary in foreign country, through skills development and knowledge enhancement.

The average cost of financing migration by Bangladesh workers stands at BDT 219,394 (USD 3,171 at 2009 exchange rates). But the government has fixed a maximum recruiting charge of BDT 84,000 (USD 1,230) (Martin, 2010).[15] It may be that aspiring migrants are not aware of the actual costs of migration. If so, then the information on migration costs provided by the SMP may apprise them of the costs, and they could eventually incur lower migration cost. We thus expect that the SMP may decrease migration costs.

Evidence shows that those who are good at migrating benefit more from migration (Bryan *et al.*, 2014). But information constraints may not enable a worker to know whether s/he is indeed good or bad at migrating. The SMP may help aspiring migrants to more realistically estimate the costs and benefits of migration as the program provides information on these issues. Hence, the program may discourage some people from migrating. On the other hand, if the program eventually reduces pre-departure risk, it may encourage others to migrate.

In summary, we test the following hypotheses:

1. The SMP may decrease migration failure. The program may also reduce the cost of migration failure.
2. The intervention may increase the salary of migrants.
3. The program may decrease the cost of migration.
4. The intervention may change the composition of the pool of potential migrants by discouraging those who are not good at migrating and encouraging those who are good at migrating.

Main outcome variables of interest for this study are thus migration failure, cost of migration failure, costs of migration, salary received abroad, and the number and composition of the pool of potential migrants.

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[15] Martin (2005) shows that in 1995, Bangladeshi migrants in Kuwait paid the highest recruitment fees and had the lowest monthly earnings, compared to migrants from countries such as Pakistan and Sri Lanka.

3.4 Evaluation Design and Data Collection

3.4.1 Evaluation Design

As already mentioned, BRAC scaled up the intervention to 80 sub-districts in 2014.[16] For the purpose of evaluation of the program using the cohort of 2014, we randomized the intervention at the union level. [17] The study population was purposely restricted to 50 sub-districts that have at least 10 unions. These 50 sub-districts contain a total of 605 unions. Among the 605 unions, 302 were randomly assigned to treatment, stratified at the sub-district level. In each sub-district, 5, 6, or 7 unions were treated, with this number randomly chosen, while the remaining unions served as control. Afterwards, we listed all the bazars located in each union, and one of the three largest bazars was randomly chosen. The nearest village from this bazar was selected as the study site. The reason for choosing a village located near a bazar is that IPT shows of the program was arranged in a public place like a large bazar.

We conducted a village census to determine the pool of potential/aspiring migrants. We define potential migrant as the one who has started the process of arranging migration, such as starting communication with recruitment agency/middlemen or relatives to procure work visa and/or spending some resources. The census covered up to 150 households depending on the village size. Then, a baseline survey was conducted on the potential migrants identified in the village census. From the list of households that had at least one potential migrant, we randomly selected 10 households. In villages that have fewer than 10 families with potential migrants, we surveyed all households. These potential migrants were followed up through phone calls to collect information on whether they have eventually succeeded. We discuss below in detail about the phone survey. These data are used to test the first and second hypotheses of this study.

As discussed below in detail, after completion of the intervention, we conducted a second round of census in the same villages covered by the first round of census. The second round of census was followed by a second round of household survey. We use these data to test the fourth hypothesis.

3.4.2 Data Collection

3.4.2.1 Baseline Census and Survey

The first round of census and baseline survey took place in late August to early December, 2014. The census identified the number of potential, current and past migrants in the sample villages. In the household survey, we collected information on the channels through which the potential migrants were trying to migrate. It also collected information on the status of getting various documents required to migrate, knowledge of migration, current occupation and salaries, and a reverse digit and personality test of the potential migrants. At the household level, the survey collected information on asset holdings and food security. The census covered 89,266

[16] There are 64 districts in Bangladesh.
[17] Union is the lowest administrative unit of Bangladesh government.

households while the baseline survey covered 3,051 households and 3,106 potential migrants (Table 3.1).

Table 3.2 reports results from a balancing test of the randomization using the full sample of baseline data. The table presents the results of regression of the dependent variable listed in column 1 on an indicator variable for treatment and sub-district fixed effects. Panel A reports results for village level characteristics and panel B for individual (i.e., aspiring migrant) and household level characteristics. Results show that the difference between the treatment and control samples is statistically insignificant for all the variables reported except for two variables (age and indicator variable for past migration). The differences for these two variables are statistically significant at the 10% level. It may be that these minor differences are due to chance. Statistics also show that an average potential migrant in the sample has about 7 years of education, indicating that**,** in Bangladesh, international out migration is mainly practiced by unskilled workers. Around 25% of the sample aspiring migrants reported that they failed at least once in their migration attempts in the past. This is consistent with the finding from existing study on failed migration (Das *et al.*, 2014).

Table A3.1 presents information on the channels via which migration is attempted by the aspiring migrants covered in the baseline survey. We divide the channels into four categories: (1) personal network, (2) private recruiting agency, (3) middleman/intermediary, and (4) others that include government lottery system and self-arrangement. Information shows that personal network is the predominant channel for migration attempts; 58% of the aspiring migrants were trying to migrate through this channel (Table A3.1). This is similar to the statistics reported by Rahman (2011) who shows that 62% of total migration from Bangladesh is arranged through personal networks. As mentioned earlier, recruiting agencies in Bangladesh often engage middlemen to recruit workers from rural areas as they do not have direct contacts with rural workers. We find that 32.6% of the aspiring migrants were trying to migrate through middlemen. Recently, the Bangladesh government has started to recruit workers through a lottery system to deploy them in Malaysia.[18] We find that 2.3% of the aspiring migrants were trying to migrate through the government's lottery system and self-arrangements.

After completion of the baseline survey, BRAC started the intervention, which continued until December, 2016.[19] Program's MIS data show that in each union, the duration of volunteer activities was about 42 person months. In each union, 12 IPT and 2 video shows were arranged. However, dropout rate among volunteers was very high, though BRAC recruited new volunteers against the dropouts. For instance, among volunteers that joined in January-February, 2015, 35% dropped out by December, 2015 and 38% by November, 2016. The remaining 27% continued until the end of the intervention. Information shows that 2% of the selected volunteers were returning migrants and 28% migrant family members. Level of education the volunteers was 10.7 years on average. Thirty seven percent of the volunteers were females.

[18] http://www.state.gov/j/tip/rls/tiprpt/countries/2014/226677.htm. Accessed on 17 May, 2015.
[19] In few unions, program activities were continued until March, 2017.

3.4.2.2 Follow up Phone Call Survey

To record information on the progress in migrant attempts of the potential migrants covered in the baseline survey, we started a phone call survey in February, 2014, about two months after completion of the baseline survey. The phone survey was repeated in every 5-6 months, and continued until December, 2016. Among the 3,106 potential migrants covered by the baseline survey, 2,851 were successfully interviewed in the phone survey at least once. The telephone survey collected information on: (i) whether the potential migrants have stopped the process of migration, or changed the medium used in attempting to migrate, (ii) updated information on the cost of migration attempts, and (iii) the status of advancement in obtaining various documents required to migrate.

Attrition rate in the phone survey was 8.21% (8.30% for treatment and 8.12% for control group). In Table A3.2 (appendix), we test whether attrition rates are different between the treatment and control groups. We also test whether baseline characteristics are correlated with attrition. Results show that there is no statistically significant difference in attrition between the treatment and control groups (column 1). However, migration experience is correlated with attrition (column 2). But the results reported in column 3 shows that the estimated coefficients on all interaction variables except for one are statistically insignificant. These results are likely to suggest that the attrition occurred randomly.

Information shows that out of the 2,851 individuals interviewed in the telephone survey, 864 migrated as of December, 2016, 656 stopped trying to migrate, and the rest were still trying to migrate. Among those who stopped trying to migrate, 122 individuals incurred financial losses. As mentioned earlier, migration failure is one of the outcome variables for this study. We use two different definitions of migration failure: (i) broader definition; and (ii) strict definition. Under the broader definition, a migration attempt is considered a failed attempt if the aspiring migrant stopped trying to migrate. Under the strict definition, by contrast, a migration attempt is considered a failed attempt if the aspiring migrant stopped trying to migrate and incurred financial losses.

We attempted to collect information on salary in the destination country from the migrants (864 migrants) identified by the phone call survey. However, it was not possible to reach many migrants because contract number in the destination country was not available. Specifically, the phone survey collected information on salary in destination countries from 287 migrants. Attrition rate in this survey was 67%. These data were collected during January, 2015 to June, 2016. In Table A3.3, we show whether attrition rates in this survey are different between the treatment and control groups. Further, we test whether baseline characteristics are correlated with attrition. Results show that there is no statistically significant difference between treatment and control groups (column 1 of Table A3.3). However, some baseline characteristics are correlated with attrition (column 2 of Table A3.3). But the results reported in column 3 show that the estimated coefficient on each of the interaction variables except for one is statistically insignificant. These findings are likely to suggest that the attrition was random.

3.4.2.3 Second Round Census and Household Survey

In May-June, 2017, we conducted a second round of census in the same villages covered by the first round of census. Like the first round, the second round of census identified the number of potential, current and past migrants in the sample villages. It identified a total of 2,552 potential migrants (Table 3.3).

We also conducted a second round of household survey in May-June, 2017. From the list of households that had at least one potential migrant, we randomly selected 10 households. In villages that have fewer than 10 families with potential migrants, we surveyed all households. In this survey, 1,925 potential migrants were interviewed. The survey collected almost the same information contained in the first round of household survey. Information collected by the second round of census and household survey is used to examine whether the program affects the composition of the pool of potential migrants.

3.4.2.4 Remittance Utilization Survey

As already mentioned, the telephone survey identified that out of 2,851 potential migrants covered, 864 migrated as of December, 2016. We conducted a remittance utilization survey on a randomly selected sample of these migrants. Specifically, we selected one migrant from each village (based on availability). The respondent of this survey was the spouse or parent of the migrant. The survey covered a total of 398 migrant households. The survey collected information on the amount of money remitted in the last year and utilization of the remittance. This survey also collected information on the total cost of migration. We use the information on migration costs to test the 3rd hypothesis of this study.

3.5 Results and Discussion

3.5.1 Impact on Pre-selected Potential Migrants

In this section, we present the impacts of the intervention on the outcomes for potential migrants identified by the first round of census. Specifically, we assess whether the program affects migration success and failure, costs of migration, and salary received abroad if migrated of this group of individuals. It needs to be mentioned here that an increase (a decrease) in migration success among these individuals does not necessarily mean a decrease (an increase) in failure among them since some of the sample individuals were still trying to migrate. Hence, we estimate treatment effects on both migration success and failure.

3.5.1.1 Impact on Migration Success and Failure

We estimate the effect on migration success and failure using the following equation:

$$Y_{ius} = \alpha_1 + \alpha_2 Treatment_{us} + \tau_s + \mu_{ius} \quad (3.1)$$

Where Y_{ius} is the outcome variable (indicator variable for migration success or failure) for individual *i* from union *u* in sub-district *s*; $Treatment_{us}$ is an indicator variable taking the value of 1 if union *u* is assigned to treatment and 0 if control; τ_s are sub-district fixed effects; and μ_{ius} is an error term. Standard errors are clustered at the union level since the intervention was randomized at this level. We estimate equation (3.1) using OLS.[20]

Table 3.4 presents the regression results of equation (3.1). Results show that migration success is 0.7 percentage points higher in the treatment areas compared to the controls but this effect is not statistically significant (column 1). Results presented in column 2 of Table 3.4, on the other hand, show that migration failure rate using the broader definition is higher in the treatment areas but the effect is statistically insignificant. However, using the strict definition, we find that the program decreases migration failure by 0.4 percentage points but again this effect is not statistically significant (column 3 of Table 3.4).

We also examine whether the effects on migration failure and success are heterogeneous with respect to poverty across the sample districts. Information on poverty was obtained from World Bank poverty mapping.[21] Poverty was reported at the district level. The rate of poverty varies between 4-53% across our sample districts. We expect that the program may have larger effects on migration success for areas where the rate of poverty is higher because the poor may have less access to accurate information and community networks compared to the non-poor. Similarly, we expect that the program may be more likely to decrease migration failure for high poverty areas. For estimating heterogeneity of the effects with respect to poverty, we extend equation (3.1) as follows:

$$Y_{iusd} = \delta_1 + \delta_2 Treatment_{usd} + \delta_3 Treatment_{usd} * Poverty_d + \gamma_s + \omega_{iusd} \quad (3.2)$$

where $Poverty_d$ is the rate of poverty in district *d*; γ_s are sub-district fixed effects; and ω_{iusd} is an error term, clustered at the union level.

We also estimate heterogeneity of the effects with respect to channel of migration attempts. We extend equation (3.1) as follows:

$$Y_{ius} = \theta_1 + \theta_2 Treatment_{us} + \theta_3 Middleman_{ius} + \theta_4 Treatment_{us} * Middleman_{ius} + \pi_s + \zeta_{ius} \quad (3.3)$$

[20] Logit model produces almost similar results.
[21] http://www.worldbank.org/en/data/interactive/2016/11/10/bangladesh-poverty-maps

where $Middleman_{ius}$ is an indicator variable taking the value of 1 if individual i was trying to migrate through middleman at baseline, and 0 if otherwise; π_s are sub-district fixed effects; and ζ_{ius} is an error term, clustered at the union level.

Regression results of equation (3.2) are presented in columns 1 though 3 of Table 3.5. Columns 4 through 6 of Table 3.5, on the other hand, report the regression results of equation (3.3). Results reported in columns 1 and 2 show that the estimated coefficient on *Treatment*Poverty* is positive for migration success and negative for migration failure (using broader definition) but these effects are statistically insignificant. For migration failure using the strict definition, the estimated coefficient on the interaction variable (*Treatment*Poverty*) is negative and statistically significant at the 10% level (column 3 of Table 3.5). This result suggests that the program is more likely to decrease migration failure for areas where poverty rate is relatively higher. Results in columns 4-6 show that treatment effects are not heterogeneous with respect to channel of migration attempt.

In Table 3.6, we present the effects of the intervention on the costs of migration failure. This analysis uses the full sample of individuals covered in the phone survey. For those that did not fail, the cost of failure is assumed to be zero. Column 1 of Table 3.6 reports the estimated effect on this outcome using specification (3.1). Columns 2 and 3 present heterogeneity of the effect with respect to poverty rate and channel of migration attempt (using specifications 3.2 and 3.3), respectively. Findings show that the costs of migration failure are lower among individuals from the treatment areas compared to the controls but the effect is not statistically significant (column 1). Results presented in column 2 show that the estimated coefficient on *Treatment*Poverty* is negative but it is statistically insignificant. It is, however, found that the program is likely to decrease the cost of migration failure among those that attempt to migrate through a middleman, and this result is statistically significant at the 10% level (column 3).

Results show modest effects of the intervention on the pre-selected potential migrants. As mentioned earlier, one of the possible channels for these effects is through raising aspiring migrants' knowledge of migration related issues. To understand whether the program affects the knowledge of migration, we provide some descriptive statistics using the data collected by the second round of household survey. In this survey, respondents (potential migrants) were asked to report how important the following documents are for migration: passport, visa, work permit report, medical test report and BMET clearance for migration. Answer to this question was pre-specified: must need; may need, and do not need/do not know. Figure 3.2 graphs the percentage of responses reporting "must need". As can be seen from Figure 3.2, for most of these documents, aspiring migrants from the treatment areas are slightly more likely to report "must need". It needs to be mentioned here that the composition of the pool of aspiring migrants covered in the second round of household survey may be affected by the intervention. Hence, this difference may not be attributable to the effect of the intervention. Nonetheless, these statistics are likely to provide some suggestive evidence that the program has little effect on aspiring migrants' knowledge of migration.

As mentioned in section 3.2.2, the program activities were implemented through, among others, community volunteers. Two percent of the selected volunteers were returning migrants

and 28% migrants family members. The level of education of the volunteers was 10.7 years on average. Thirty seven percent of the volunteers were females. It may be that the program effect is sensitive to gender and education of volunteers. The effect can be also different for areas where more volunteers were selected from returning migrants and migrant family members. We examine heterogeneity of the effects with respect to these issues. For this purpose, we generate the following three variables at the district level: proportion of volunteers that are returning migrants or migrant family members; proportion of male volunteers; and average years of education of volunteers. Next, we estimate the following equation:

$$Y_{iusd} = \beta_1 + \beta_2 Treatment_{usd} + \beta_4 Treatment_{usd} * Edu_d + \beta_4 Treatment_{usd} * Gender_d + \beta_4 Treatment_{usd} * Migrationexp_d + \lambda_s + e_{iusd} \quad (3.4)$$

Where Edu_d is the average education level of volunteers in district d; $Gender_d$ is the proportion of male volunteers in district d; and $Migrationexp_d$ is the proportion of volunteers that are returning migrants or migrant family members in district d; λ_s are sub-district fixed effects; and e_{iusd} is an error term, clustered at the union level.

Regression results of equation (3.4) are presented in Table 3.7. It is found that the program is more likely to increase migration success for areas (i.e., districts) where the proportion of volunteers that are returning migrants or migrant family members is higher (column 1). The effect on migration success is not, however, heterogeneous with respect to gender or education (results are statistically insignificant). Results reported in column (2) reveal that the effect on migration failure is heterogeneous with respect to education of volunteers. Specifically, the program is more likely to decrease migration failure for areas where the level of education of volunteers is higher. These results suggest that the Safe Migration Program might be more effective if it engages more educated individuals and/or more returnee migrants/migrant family members as volunteers.

3.5.1.2 Impact on Salary

As mentioned earlier, the telephone survey collected information on salary in destination countries from 287 migrants. We use this sample of migrants to analyze the effect of the intervention on salary received abroad.[22] The effect is estimated using specification (3.1). Since the program has no effect on overall migration success (if any, the magnitude of the effect is small), and the attrition rate in the phone survey for collecting the salary information is evidently random (see appendix Table A3.3), it is likely that the error term in equation (3.1) for this outcome (salary received abroad) using selected sample is uncorrelated with the treatment indicator. Therefore, the coefficient on the treatment indicator in equation (3.1) is likely to be an unbiased estimate of the effect of the intervention on salary received abroad. Table 3.8 reports the estimated effect on salary. Results show that the intervention increases the salary of migrants, but this effect is not statistically significant, perhaps because of low statistical power.

[22] Salary information is for the last month of the survey. It was collected in destination country's currency. It was converted to USD using 2016 exchange rate.

3.5.1.3 Effect on the Costs of Migration

In this section, we examine whether the program reduces the costs of migration. We use the cost information collected by the remittance utilization survey. We estimate treatment effect on this outcome using specification (3.1). As already shown, the program has no effect on overall migration success (if any, the magnitude of the effect is small). It is thus likely that the error term in equation (3.1) for this outcome (costs of migration) using selected sample is uncorrelated with the treatment indicator. Therefore, the coefficient on the treatment indicator in equation (3.1) is likely to be an unbiased estimate for the effect of the intervention on the costs of migration.

Table 3.9 presents the estimated effect on the cost of migration. Findings show that migrants from treatment areas are likely to incur lower costs of migration compared to those from control areas (column 1). This effect is, however, not statistically significant. In column 2 of Table 3.9 we report heterogeneity of the effect with respect to channel of migration attempt using specification (3.3). Results show that the program reduces the costs of migration for individuals who attempt to migrate through middleman, an informal channel of migration, and this effect is statistically significant at the 10% level. This finding is expected because, as mentioned earlier, migrants from Bangladesh face high costs of migration, and a substantial amount of these costs goes to the middlemen.[23] The channel for the effect on the costs of migration may be through raising aspiring migrants' awareness level of the true cost to migrate (i.e., BDT 84,000 fixed by Bangladesh), which is lower than the cost migrants typically incur. Using data from second round of household survey, we provide some descriptive evidence on whether the program affects aspiring migrants' knowledge of the costs of migration. The survey asked aspiring migrants to report the costs of procuring a work visa. Figure 3.3 graphs the distribution of the responses. As can be seen from Figure 3.3, aspiring migrants from the treatment areas are likely to report lower costs of procuring a work visa. It needs to be mentioned here that the difference between the treatment and control groups may not necessarily be attributable to program effect because the program might have affected the composition of the pool of potential migrants covered in the second round of household survey. Hence, this result provides some suggestive evidence rather than causal effect that the intervention raises aspiring migrants' knowledge of the costs of migration.

3.5.2 Effect on the Composition of the Pool of Potential Migrants

In this section, we test the fourth hypothesis that the program may change the composition of the pool of potential migrants. To test this hypothesis, at first we use the phone call information on the initial cohort of aspiring migrants to estimate the following two models of migration success:

$$Y = f\left(X\beta^{0}\right) \quad \text{for aspiring migrants in the control group} \ldots\ldots \quad (3.5)$$

$$Y = f\left(X\beta^{P}\right) \quad \text{for aspiring migrants in the treatment group} \ldots.. \quad (3.6)$$

[23] IOM (2010) shows that about 59.5% of the costs goes to middlemen.

where *Y* is a success indicator (successfully migrating) and *X* is a vector of predictors of success indicator.

Using the estimates from equation (3.5), we then compare the distributions of predicted success $f\left(X_T\hat{\beta}^0\right)$ and $f\left(X_C\hat{\beta}^0\right)$ for aspiring migrants in the second round of census in the treatment and control villages, respectively. Here, X_T and X_C are the vectors of characteristics of aspiring migrants identified by the second round of census in treatment and control villages, respectively. The difference between these distributions is solely due to the difference in characteristics between the two pools of aspiring migrants. The discouragement effect would be that there is less density of aspiring migrants who are predicted to be less successful.

Column 1 of Appendix Table A3.4 reports the estimated coefficients of a regression of migration success of the initial pool of aspiring migrants from the control areas on a set of baseline characteristics while column 2 reports that of the treatment areas. Figure 3.4 graphs the predicted successes for the potential migrants identified by the second round of census. They are estimated using the point estimates reported in column 1 of Table A3.4 and the characteristics of the aspiring migrants identified by the second round of census. The distribution of the predicted successes graphed in Figure 3.4 is slightly different for treatment and control groups; the density of the control group seems to be higher at the lower level of predicted success. This provides some weak evidence that the program has affected the composition of the pool of potential migrants.

We also examine whether the intervention encourages more people to attempt to migrate. For this purpose, we use the information collected by the second round of census. Analysis is conducted at the union/village level. Specifically, we estimate the following equation:

$$Y_{us} = \sigma_1 + \sigma_2 Treatment_{us} + \eta_s + \vartheta_{us} \qquad (3.7)$$

Where Y_{us} is the total number of potential migrants in union u from sub-district s; $Treatment_{us}$ is an indicator variable taking the value of 1 if union u is treated and 0 if control; η_s are sub-district fixed effects; and ϑ_{us} is an error term.

Regression results of equation (3.7) are presented in column 1 of Table 3.10. Results show that the number of potential migrants is higher in the treatment areas but the effect is not statistically significant (column 1). Point estimate, however, indicates that the program increases the number of potential migrants by 9 percent relative to control group mean. In column 2 of Table 3.10, we report heterogeneity of the effect with respect to poverty rate in the sample districts. Results show that the estimated coefficient on the interaction variable (Treatment*Poverty Rate) is positive but it is statistically insignificant (column 2). Column 3 of Table 3.10 presents similar results as those of column 2, but these effects are estimated using an indicator variable for poverty (it takes the value of 1 if the rate of poverty is 30 or more and 0 if otherwise) instead of poverty rate.[24] We find that the intervention increases the number of potential migrants for areas where the rate of poverty is 30 or higher, and this result is

[24] According to BBS (2012), poverty rate in Bangladesh is 31%. Hence, we use 30 as the cutoff.

statistically significant at the 10% level. Taken together, results presented in Table 3.10 show that there is some weak evidence that more people from the treatment areas attempt to migrate compared to the controls.

3.6 Conclusion

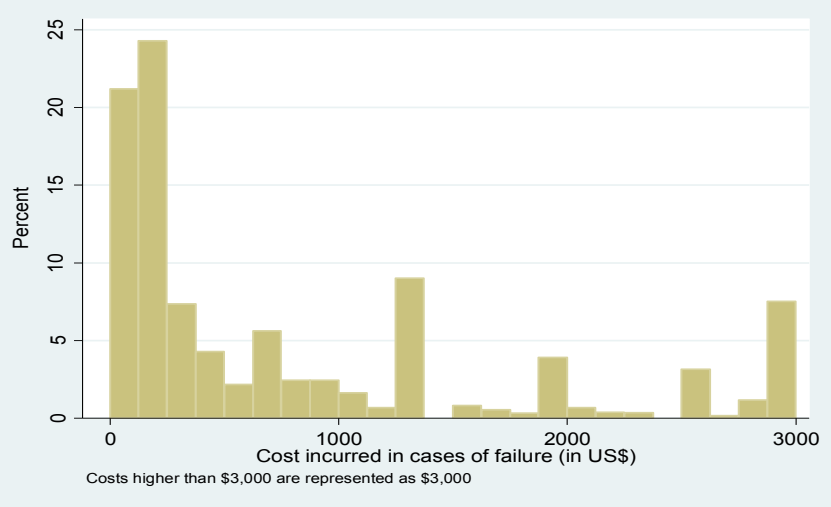
Economic returns to migration can be very high but many poor people do not engage in such profitable behavior. Studies emphasize the role of risk in explaining why more people do not move despite high returns to migration. In this paper, we investigate whether providing information along with administrative and community support to aspiring migrants throughout the migration process reduces the risk of migration. The risks we focus on include: (1) the risk of failure to depart home country after having spent resources during the process; and (2) the risk of not finding the expected job or salary abroad. To answer this question, we evaluate BRAC's Safe Migration Program (SMP) that assists aspiring migrants by (i) providing better access to accurate and timely information and services for safe migration and (ii) strengthening community-based organizations (CBOs) in order to reduce migrants' dependency on middlemen. The program is implemented in Bangladesh. We randomized the intervention at the union level, the lowest administrative unit of the country.

Our results show that the intervention has no statistically significant effect on overall migration success or failure. Similarly, the program has no statistically significant effect on the salary received abroad by migrants. Further results, however, show that it reduces migration failure for areas where poverty rate is relatively higher. Results also show that the program reduces the costs of migration failure among individuals that attempt to migrate through middlemen/intermediary, an informal channel of migration. Similarly, the program reduces the costs of migration among those that attempt to migrate through this channel. We find that due to the intervention more people attempt to migrate from areas where the rate of poverty is higher.

Overall, we find little effect of the intervention, but additional results show that the program is more likely to decrease migration failure for areas where the level of education of the volunteers engaged in the program is higher. The intervention is also more likely to increase migration success for areas where proportion of volunteers that are returning migrants or migrants' family members is higher. These results suggest that the Safe Migration Program might be more effective if it engages more educated individuals and/or more returnee migrants/migrant family members as volunteers.

3.7 Figures

Figure 3.1. Cost of migration failures



Source: Das et al. (2014), Figure 1, pp 20

Figure 3.2: Knowledge of necessity of various documents for migration

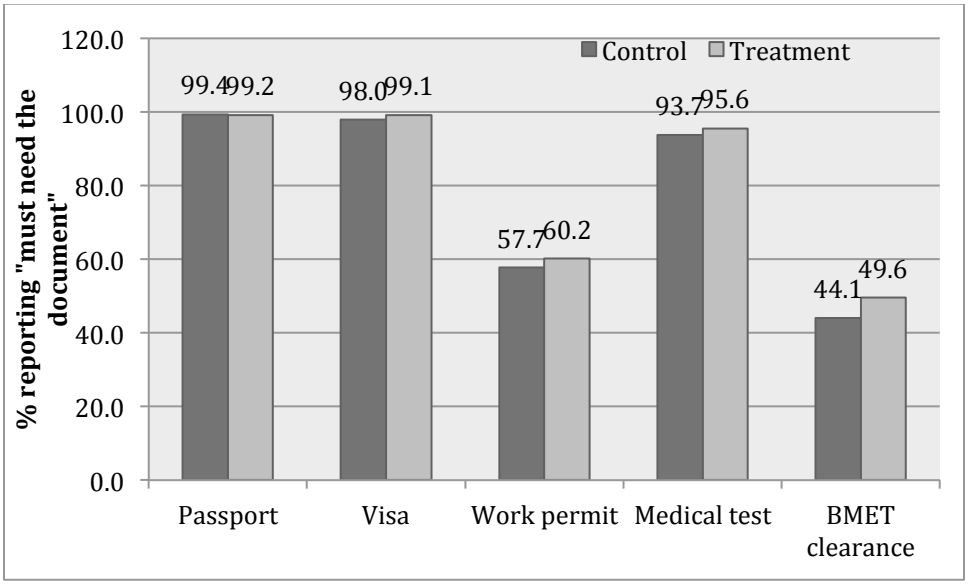


Figure 3.3: Distribution of the costs of procuring work visa (perceived costs reported by aspiring migrants)

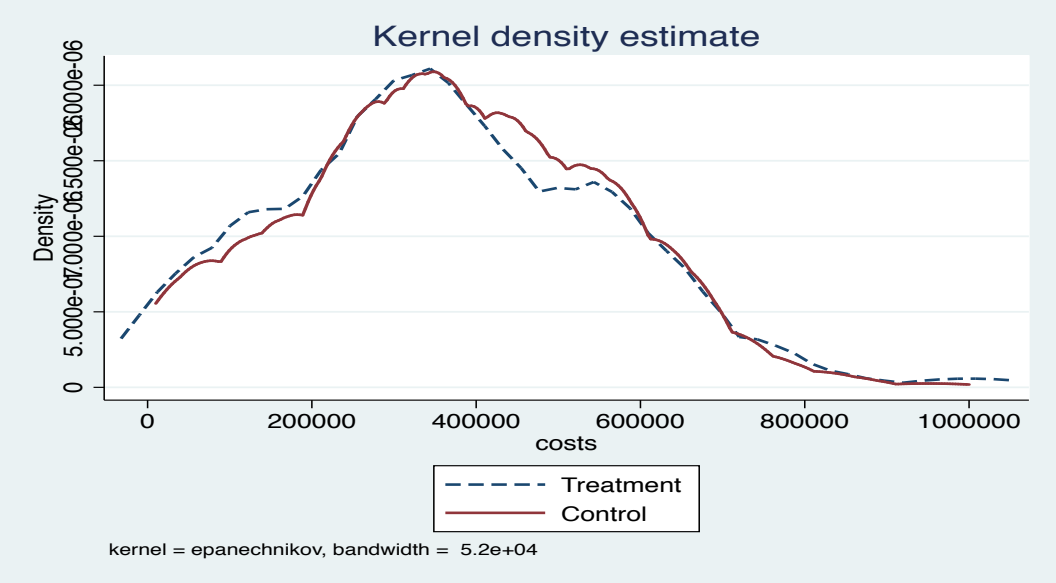
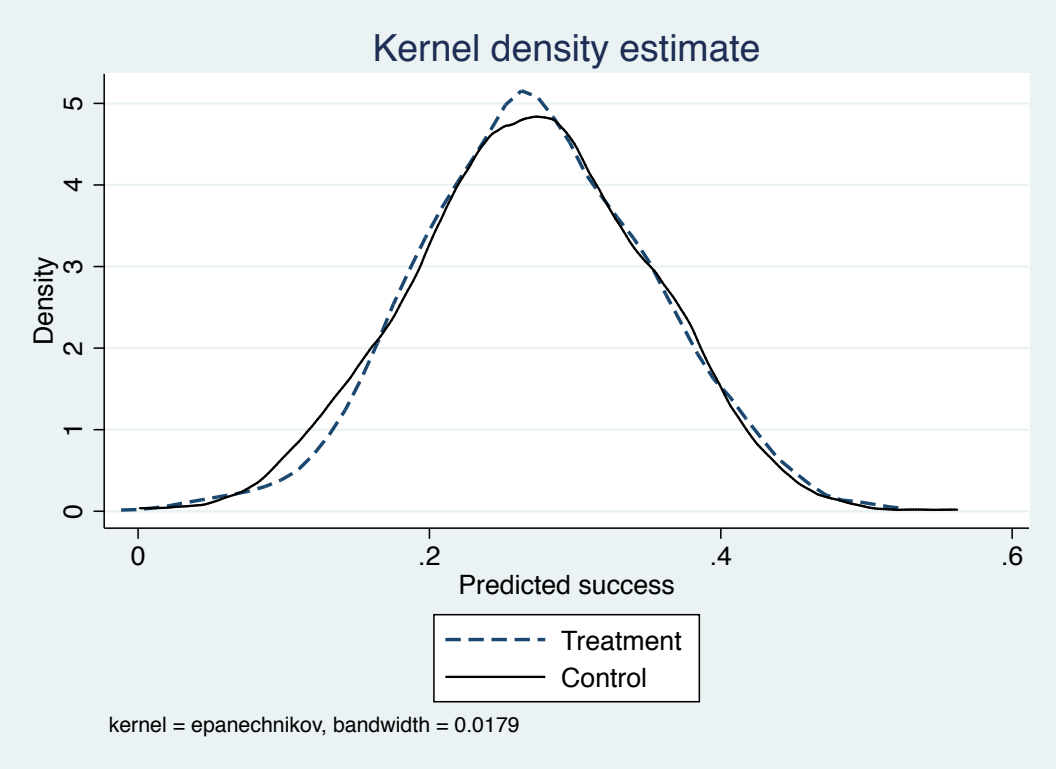


Figure 3.4: Predicted successes of the potential migrants identified by second round of census



3.8 Tables

Table 3.1: No. of households and potential migrants covered in the first round of census and survey

	Number
Census	
No. of households	89,266
No. of households with at one potential migrant	4,474
No. of potential migrants	4,585
Household survey	
No. of households with at one potential migrant	3,051
No. of potential migrants	3,106

Table 3.2: Balancing test of randomization

(1)	(2)	(3)	(4)	(5)
	Treatment	Constant	Observations	R-squared
Panel A: Village level characteristics				
No. of potential migrants	0.230	4.376***	605	0.382
	(0.350)	(1.427)		
No. of current migrants	0.282	17.56***	605	0.640
	(1.320)	(5.375)		
Panel B: Individual (potential migrant) and household level characteristics				
Age (years)	0.483*	28.39***	3106	0.033
	(0.250)	(2.436)		
Sex (male=1, female=0)	-0.00182	1.000***	3106	0.057
	(0.00529)	(0.000903)		
Past migrant (yes=1, no=0)	0.0317*	0.0971*	3106	0.077
	(0.0168)	(0.0565)		
Years of education	-0.0585	7.411***	3106	0.047
	(0.137)	(0.907)		
Ever failed in attempting to migrate (yes=1, no=0)	0.00539	0.255***	3106	0.053
	(0.0186)	(0.0694)		
Land amount	-1.027	132.1***	3106	0.045
	(11.70)	(39.13)		

Standard errors in parentheses. Standard errors reported in panel B are clustered at the union level.
*p<0.10, **p<0.05, ***p<0.01

Table 3.3: No. of households and potential migrants covered in the second round of census and survey

	Number
Census	
No. of households covered	89,784
No. of HHs with at least one potential migrant	2,511
No. of potential migrants	2,552
Household survey	
No. of households with at least one potential migrant	1,925
No. of potential migrants	1,925
No. of villages/unions covered	605

Table 3.4: Effects on migration success and failure

	(1)	(2)	(3)
	Migrated (yes=1, no=0)	Failed (yes=1, no=0) (broader definition)	Failed (yes=1, no=0) (strict definition)
Treatment	0.00657	0.0106	-0.00434
	(0.0192)	(0.0162)	(0.00831)
Sub-district fixed effects	Yes	Yes	Yes
Observations	2851	2851	2851
R-squared	0.050	0.047	0.037
Control group mean	0.29	0.22	0.047

Standard errors in parentheses, clustered at the union level.
*p<0.10, **p<0.05, ***p<0.01

Table 3.5: Heterogeneity of the effects on migration success and failure with respect to poverty and migration channel

	(1)	(2)	(3)	(4)	(5)	(6)
	Migrated (yes=1, no=0)	Failed (broader definition) (yes=1, no=0)	Failed (strict definition) (yes=1, no=0)	Migrated (yes=1, no=0)	Failed (broader definition) (yes=1, no=0)	Failed (strict definition) (yes=1, no=0)
Treatment	-0.0491	0.0334	0.0413	0.0102	0.00119	-0.000615
	(0.0631)	(0.0508)	(0.0278)	(0.0234)	(0.0205)	(0.0100)
Treatment*Poverty	0.00160	-0.000655	-0.00131*			
	(0.0017)	(0.00142)	(0.00078)			
Treatment*middleman				-0.00671	0.0281	-0.0107
				(0.0389)	(0.0332)	(0.0171)
Middleman				0.091***	-0.0331	0.0217
				(0.0289)	(0.0238)	(0.0133)
Sub-district fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2851	2851	2851	2851	2851	2851
R-squared	0.051	0.047	0.038	0.056	0.047	0.038
Control group mean	0.29	0.22	0.047	0.29	0.22	0.047

Standard errors in parentheses, clustered at the union level. *p<0.10, **p<0.05, ***p<0.01

Table 3.6: Effects on the costs of migration failure

	(1)	(2)	(3)
	Costs of failure (BDT)	Costs of failure (BDT)	Costs of failure (BDT)
Treatment	-801.5	2785.1	576.7
	(1174.8)	(3815.8)	(1408.9)
Treatment*Poverty		-103.0	
		(110.0)	
Treatment*Middleman			-4183.5*
			(2395.7)
Middleman			4018.5*
			(2354.4)
Sub-district fixed effects	Yes	Yes	Yes
Observations	2851	2851	2851
R-squared	0.034	0.034	0.036
Control group mean	4346	4346	4346
Treatment+Treatment*Middleman			-3606.8*
			(1989.0)

Standard errors in parentheses, clustered at the union level.
*p<0.10, **p<0.05, ***p<0.01

Table 3.7: Heterogeneity of the effects on migration success and failure with respect to volunteers' education, gender and migration experience

	(1)	(2)	(3)
	Departed (yes=1, no=0)	Failed (broader definition) (yes=1, no=0)	Costs of migration failure (BDT)
Treatment	-0.0254	0.859**	26960.9
	(0.466)	(0.358)	(38550.5)
Treatment*proportion of volunteers that are returning migrants or migrants' family members	0.206*	0.0276	318.8
	(0.123)	(0.107)	(14332.0)
Treatment*proportion of male volunteers	0.147	0.0210	-5278.4
	(0.164)	(0.130)	(12265.7)
Treatment*average education level of volunteers	-0.0119	-0.0811**	-2203.6
	(0.0488)	(0.0370)	(3937.7)
Sub-district fixed effects	Yes	Yes	Yes
Observations#	2777	2777	2777
R-squared	0.051	0.051	0.030
Control group mean	0.29	0.22	4346

Standard errors in parentheses, clustered at the union level. *p<0.10, **p<0.05, ***p<0.01
#Information on volunteers was not available for one district. This district has been dropped from analysis.

Table 3.8: Effect on the salary of migrants (USD/month)

	Salary (USD)
Treatment	10.04
	(20.78)
Sub-district fixed effects	Yes
Observations	287
R-squared	0.096
Control group mean	365

Standard errors in parentheses, clustered at the union level. *p<0.10, **p<0.05, ***p<0.01

Table 3.9: Effects on the costs of migration

	(1)	(2)
	Log of costs	Log of costs
Treatment	-0.0553	0.0100
	(0.0549)	(0.0708)
Treatment*Middleman		-0.160
		(0.114)
Middleman		0.0941
		(0.0793)
Sub-districts fixed effects	Yes	Yes
Observations	398	398
R-squared	0.386	0.390
Control mean	12.84	12.84
Treatment+Treatment*Middleman		-0.150*
		(0.086)

Standard errors in parentheses, clustered at the union level. *p<0.10, **p<0.05, ***p<0.01.

Table 3.10: Effect on the number of potential migrants

	(1)	(2)	(2)
	No. of potential Migrants	No. of potential Migrants	No. of potential Migrants
Treatment	0.378	0.114	0.0153
	(0.254)	(0.717)	(0.455)
Treatment*Poverty Rate		0.0077	
		(0.0198)	
Treatment*High Poverty (yes=1, no=0)			0.527
			(0.548)
Sub-district fixed effects	Yes	Yes	Yes
Observations	605	605	605
R-squared	0.340	0.33	0.341
Control group mean	4.06	4.06	4.06
Treatment+Treatment*High Poverty (yes=1, no=0)			0.542*
			(0.306)

*p<0.10, **p<0.05, ***p<0.01

3.9 Appendix Tables

Table A3.1: Channels of migration attempts

Channel	%
Middlemen	32.6
Personal networks	58.4
Recruitment agency	6.7
Others (lottery or self-initiatives)	2.3

Table A3.2: Correlates of attrition in the phone survey on aspiring migrants

	(1)	(2)	(3)
	Attrition	Attrition	Attrition
Treatment	0.00174	0.00141	-0.185
	(0.0119)	(0.0119)	(0.120)
Age of potential migrant (years)		0.000618	0.00119
		(0.000779)	(0.00118)
Sex of potential migrant (male=1, female=0)		-0.0951	-0.217**
		(0.0591)	(0.102)
Whether potential migrant is returning migrant (yes=1, no=0)		-0.0240**	-0.0344**
		(0.0112)	(0.0149)
Years of education of potential migrant		-0.000320	0.0000535
		(0.00158)	(0.00244)
Amount (decimal) of land owned by potential migrant's household		-0.00000505	-0.0000250**
		(0.0000109)	(0.0000110)
Number of cows owned by potential migrant's household		0.00317	0.00938
		(0.00322)	(0.00628)
Age*Treatment			-0.00122
			(0.00155)
Sex (male=1, female=0) *Treatment			0.229**
			(0.113)
Returning migrant (yes=1, no=0) *Treatment			0.0246
			(0.0224)
Years of education*Treatment			-0.00101
			(0.00316)
Owned land (decimal) *Treatment			0.0000299
			(0.0000198)
Number of cows owned*Treatment			-0.00919
			(0.00710)
Observations	3106	3106	3106
R-squared	0.000	0.004	0.007

Standard errors in parentheses, clustered at the union level.
*p<0.10, **p<0.05, ***p<0.01

Table A3.3: Correlates of attrition in the phone call survey conducted on migrants for collecting salary information

	(1)	(2)	(3)
	Attrition	Attrition	Attrition
Treatment	-0.0471	-0.0469	-0.131
	(0.0470)	(0.0462)	(0.320)
Age of potential migrant (years)		0.00265	0.00857*
		(0.00349)	(0.00465)
Sex of potential migrant (male=1, female=0)		0.118	-0.184
		(0.137)	(0.163)
Whether potential migrant is a returning migrant (yes=1, no=0)		-0.0173	-0.0119
		(0.0405)	(0.0610)
Years of education of potential migrant		0.00867	0.0148*
		(0.00553)	(0.00760)
Amount (decimal) of land owned by potential migrant's household		0.0000334*	-0.000231
		(0.0000188)	(0.000263)
Number of cow owned by potential migrant's household		-0.0222*	-0.00299
		(0.0128)	(0.0205)
Age*Treatment			-0.0104
			(0.00675)
Sex (male=1, female=0) *Treatment			0.441*
			(0.240)
Returning migrant (yes=1, no=0) *Treatment			-0.00121
			(0.0812)
Years of education*Treatment			-0.00961
			(0.0107)
Owned land (decimal) *Treatment			0.000276
			(0.000264)
Number of cows owned*Treatment			-0.0289
			(0.0266)
Observations	864	864	864
R-squared	0.003	0.013	0.023

Standard errors in parentheses, clustered at the union level. *p<0.10, **p<0.05, ***p<0.01

Table A3.4: Correlates of migration success
Dependent variable: indicator variable for migration success (1 if migrated, 0 if otherwise)

	(1)	(2)
Baseline characteristics of aspiring migrants	Control	Treatment
Years of education	-0.000197	-0.00181
	(0.00355)	(0.00435)
Sex (male=1, female=0)	-0.0777	-0.188*
	(0.120)	(0.113)
Age (years)	-0.00650***	0.000713
	(0.00199)	(0.00221)
Channel of migration (middleman=1, otherwise=0)	0.0952***	0.105***
	(0.0293)	(0.0291)
Amount of land (decimal)	-0.000064**	0.0000353
	(0.0000324)	(0.0000305)
No. of cows	0.00324	-0.00850*
	(0.00984)	(0.00509)
No. of migrant relatives	0.00282	0.00756*
	(0.00384)	(0.00438)
Know that one can migrate through govt. (yes=1, no=0)	-0.0240	0.0237
	(0.0292)	(0.0271)
Know that work visa needs to get verified (yes=1, no=0)	0.0216	0.0311
	(0.0626)	(0.0586)
Employed (yes=1, no=0)	-0.0507	-0.104***
	(0.0382)	(0.0330)
Reversed digit test score	-0.0197	-0.0176
	(0.0167)	(0.0140)
No. of times visited Dhaka	0.0199	0.00334
	(0.0125)	(0.0102)
Constant	0.486***	0.474***
	(0.159)	(0.144)
Observations	1404	1433
R-squared	0.026	0.026

Standard errors in parentheses, clustered at the union level. *p<0.10, **p<0.05, ***p<0.01
Ownership of assets (cow and land) is at the household level.

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