The London School of Economics and Political Science

Social protection and human capital accumulation in developing countries

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A thesis submitted to the Department of International Development of the London School of Economics for the degree of Doctor of Philosophy, London, March 2012

Declaration

I certify that the thesis I have presented for examination for the MPhil/PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

I warrant that this authorization does not, to the best of my belief, infringe the rights of any third party.

I declare that my thesis consists of 33,401 words.

Acknowledgements

I would like to thank Oriana Bandiera for her constant guidance in finishing this work. I have also benefitted from regular supervisions and discussions with Robin Burgess, Imran Rasul, and Diana Weinhold. Special thanks to Shahe Emran, Selim Gulesci, Fahad Khalil, Imran Matin, and Samer Al-Samarrai.

Part of this research benefited from funding by the World Bank, and the UK Department for International Development (DFID). DFID funding was part of the iiG, a research program to study how to improve institutions for pro-poor growth in Africa and South-Asia. The views expressed are not necessarily those of DFID. All remaining errors are mine.

Abstract

My thesis comprises of three stand-alone papers, which are connected by the theme of social protection and human capital accumulation. In the first paper, using experimental data from South Sudan, I focus on evaluating the effects of food transfer on household labour supply decisions and crowding-out of informal private transfers. I do not observe significant impact on either of these two domains, except reduction in child labour. This effect corresponds with increased school enrolment of children. I find that positive income shocks from short-term food transfers induced the households to invest in durable goods, and child 'non-work' is a luxury good for the ultra-poor. The second paper evaluates the effects of a policy related to exam standard on labour market performance of secondary school graduates in Bangladesh. Using a natural experiment, the paper shows that lowering standard reduced labour market returns for the graduates. General equilibrium effects of increased supply of graduates and lower human capital accumulation due to lower standard have been identified as possible mechanisms underlying this labour market effect. In my third paper, I evaluate the effects of an asset transfer programme for the ultra-poor in Bangladesh on children's enrolment. I find that despite exceptionally large positive impact on household income, asset transfer did not increase enrolment rates. Moreover, there was increased demand for child labour in these households. The evidence suggests that asset transfer may not be sufficient to increase school enrolment among households in extreme poverty and may have unintended effects on child labor.

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Incentive and crowding out effects of food assistance: Evidence from randomized evaluation of a food-for-training project in South Sudan

Abstract

Food assistance is one of the most common forms of safety net programs in developing countries. Though there are strong humanitarian arguments for such programs, they are often criticized on the grounds of their possible influence on creating disincentives to work and on crowding out private transfers. According to nutrition-based efficiency wage argument, on the other hand, food assistance can increase ability to work and labor supply of the poor. Based on a randomized evaluation, this paper estimates the effects of a food-for-training program, which comprised of food transfer, training and access to credit services, on labor supply and informal transfers. We do not observe any significant effect of the program on the hours of work or the type of economic activities of the adult members. However, there was a significant negative impact (about 20-25%) on per capita household income. This decline in income mostly came about through significant reduction in child labor. We also find that short-term food transfer assisted the households to invest in durable assets, mostly in housing, which is a means for the poor to spread gains from a short-term transfer program over their life-cycle. Results do not show private transfers being crowded out for the participants although there was a positive effect on transfers given out by them. Further evidence suggests that these effects are primarily of food transfer component rather than training or credit component of the program.

Keywords: food assistance, incentive, crowding-out, South Sudan
JEL Classification: J22, O12, Q18.

1 Introduction

Food assistance is one of the most common forms of safety net programs for the poor in developing countries. Besides the humanitarian objective, it is often believed that such assistance may work as a livelihood promotional mechanism by providing the poor with an opportunity to save and build a stronger asset base for maintaining their livelihoods. Food assistances can also enable them to fulfill their nutritional requirement, and thereby work harder. On the other hand, there are widespread skepticisms regarding food assistances since such transfers can create disincentives to work among the participants, generate dependency and crowd out private transfers.

There is a large literature in the field of social protection discussing different aspects of food assistance. This includes (but certainly not limited to) incentive effects of such transfers on labor supply (Abdulai et al, 2005), changes in local production through price effects (Tadesse and Shively, 2009), crowding out of informal assistances (Dercon and Krishnan, 2003), effects on productivity through improved nutritional status, effects on asset accumulation to break poverty traps (Gilligan and Hoddinott, 2007), appropriate forms of transfers (cash vs. in kind) (Basu, 1996) or efficacy of conditionality. Most of these empirical studies, especially those based on micro data, are limited to relatively stable contexts and the evidences are often ambiguous. For example, in her careful review of 25 years of literature, Lentz (2003) did not find any incidence of a clear evidence of (dis)incentive effects of food assistance. Moreover, despite the lack of rigorous evidence, food transfers are often assumed to have positive effects on peace building by materializing developmental effects in post-conflict settings (Bailey and Harragin, 2009). Barrett and Maxwell (2005) describe these two roles of food assistance as “emergency” and “developmental”.

This paper investigates two key aspects of food transfers, influence on incentive to work and effects on crowding out informal private transfers, using randomized evaluation of a program in South Sudan. The program evaluated in this paper is called Food for Training and Income Generation (FFTIG), and was implemented by BRAC[1] for the ‘ultra-poor’ living in Juba. Along with food transfers, this program provided training on income generating activities and offered credit services. We used over-selection of potential beneficiaries to construct a randomized control group. Following a baseline survey in 2008, randomly selected households were provided with a monthly food package for 9 months. One adult woman from each beneficiary household also participated in training on an income generating activity for 5-20 days and 1-3 hours per day, depending on the type of training. They were also offered credit services although only 6% of the treatment households took credit from BRAC.

Using a panel data collected with one-year interval, our results show that the program did not make any substantial change either in the amount of time spent on earning activities by the participant households or in the pattern of their economic activities. We also do not observe any significant impact on the structure of economic activities of other adult household members. The participants did not start the activities that they received training on, which indicates that the training component of program did not have any significant influence.

The program had reduced the extent of child labor in the beneficiary households by about 85 hours a year, which is about 60% of average hours of total child labor per household in the baseline. We also find about 20-25% reduction in the per capita income of the participant households, and about 10

[1] BRAC is an NGO originating in Bangladesh and currently implementing various development initiatives (such as microfinance, primary healthcare, agriculture extension, youth development etc.) in several Asian and African countries.

percentage points increase in school enrolment of girls. There is also no evidence of crowding out of informal private transfers, which is probably due to very limited informal transfers to begin with. Interestingly, we find that program participation increased the likelihood (and amount of) transfers given out.

In terms of effects on welfare, food assistance helped the participants to improve their housing conditions, e.g. homestead ownership increased by about 8 percentage points, and use of better construction materials increased by 9 percentage points. These changes in housing are consistent with positive effects on annual non-food expenditures of the participants. No major impact is observed on accumulation of other physical assets, except marginal increases in ownership of shed for livestock, electric fans and bed nets. Since many of the households were recent returnees from camps and many were living in make-shift houses, this priority of investment in housing is understandable. This also shows that the households spread the gain from a 'short-term' food transfer over a life-time by investing in housing, instead of reducing current labor supply, as the permanent income hypothesis predicts.

Findings from this study indicate that food transfers do not necessarily lead to reduced labor supply by participants, and households may smooth their consumption from this gain by investing in durable goods. The results cannot be generalized to all forms of food transfers, but to transfers that are for a specified period. Although the training and credit components of the program seem to have been ineffective, reflected through very low uptake of the activities they received training on and of credit, these effects could not be disentangled. As far as improving livelihood strategy and productivity of the poor is concerned, food-for-training programs are not sufficient to bring any substantial change. Other forms of transfers, e.g. productive assets, could be tested as possible means of bringing sustainable changes in livelihoods of the poor.

The next Section gives a brief description of the context, the program and the data. The impact on the supply of labor and informal transfers is discussed in Section 3. The impact on various other welfare indicators including income, expenditure, savings, enrolment rates and assets are presented in Section 4. A few checks for robustness of the main results with alternative specifications and sampling restrictions are conducted in Section 5. Section 6 concludes the paper.

2 Program description and the experiment design

BRAC-South Sudan (BRAC-SS) initiated a pilot program called Food-for-training and income generation (FFTIG) in Juba in collaboration with the World Food Program (WFP) and Consultative Group to Assist the Poor (CGAP). The objective of the program was to combine the "protection" and "promotion" aspects of safety nets (Matin and Hulme, 2003); food transfer is the protection component, and training in income generating activities and access to credit being the promotional aspects. It was expected by the program that combing food transfers with skill development and financial services will enable the households to move into a regular source of income and build an asset base to cope with minor shocks.

2.1 The context

It is important to have a good description of the context because of the uniqueness of South Sudan, and hence the results may not be generalizable for most food transfer programs. South Sudan went through five decades of civil conflict that took a serious toll on the lives and livelihoods of the people. During the

conflict, over two million people died of famine and fights since 1983 (USAID, 2004), over four million were displaced within Sudan and over half a million people took refuge in neighboring countries (UNDP, 2004). These figures are enormous in relation to its population of 8 million. Though things started to improve with the peace agreement of 2005, the conditions in South Sudan remains one of the harshest in the world.

The first ever official statistical record in fifty years in South Sudan shows only a glimpse of this harshness (SSCCSE, 2009). One in every 10 children in Central Equatoria[2] does not survive the first year of their lives (Table 1). Average life expectancy at birth is 42 years, which is the lowest in the world. South Sudan also has the highest maternal mortality. Although poverty in South Sudan cannot easily be measured in economic terms, UNDP (2004) estimates show that over 90% of its population lives on less than a dollar a day. According to more recent statistics, more than half of the population is living below the poverty line (SSCCSE, 2010). Livelihoods in rural areas consist almost entirely of agriculture and livestock. Even in Juba, the capital and a county of Central Equatoria, livestock rearing is the major economic activity and is the most commonly used indicator of wealth (WFP, 1999; SSCCSE, 2010).

Given the circumstances in South Sudan, food aid has been one of the key factors of development and famine control in this region for almost half a century (Gelsdorf et al, 2007). Food has been a key facet of politics of Sudan as well. Keen (2008) argues that famines in Sudan have sometimes been generated by deliberate obstruction of aid efforts and plunders. Following the peace agreement in 2005, Sudan has become one of the top three countries for WFP interventions. In 2008, Sudan alone received more than 11% of the total food aid delivered by WFP. However, food aid in South Sudan is often criticized for failing to embed any 'exit strategy' (Pantuliano, 2007). More recently, WFP's food assistance in South Sudan is being promoted as a means to reintegration by enhancing "the ability of returnees to secure the political, economic and social conditions to maintain their life, livelihood and dignity" (Bailey and Harragin, 2009).

2.2 The intervention

Each participant household received food for a period of 9 months starting from March 30, 2008. The amount of food transferred followed the WFP guidelines for food rations for training programs. The ration includes 450 grams of cereals, 50 grams of pulses, 30 grams of vegetable oil, 10 grams of iodized salt, 30 grams of sugar and 50 grams of corn soy blend for one person per day. The participants received their monthly allocation of food based on the initial size of their households. To minimize the cost of collection, the transfers took place at BRAC branch offices, which are mostly within walking distance from the participants' houses.

From mid-April 2008, adult women from each participant household (either the head or spouse of head) were mobilized into small groups. In group meetings, alternative training opportunities were described and each participant chose one income generating activity (IGA) to receive training on. Though five different IGA trainings were provided as choices, almost 80% of the participants opted for vegetable cultivation. The other trainings included setting up nursery, tailoring, petty trade and cattle rearing. One agriculture sector specialist conducted the training sessions. The trainings included both class-room discussion in the branch offices and working in the fields. A typical training period lasted for 2 hours per day for 5 days. Although food aid was designed to allow the households to become engaged in new

[2] Juba is located in Central Equatoria, which is one of the 10 states of South Sudan.

earning activities, the actual transfers took place irrespective of whether or not they took up the new activity.

Among the three components, food transfer was the major part of the program in terms of costs. Over 90% of the total cost of the program was associated with the food transfers.[3] The trainings were for too short a duration and food transfers were not conditional on uptake of the activity. We also find that the trainings were very ineffective (discussed in Section 3.1) and unlikely to have influenced the main results. The credit component was available for all the households in those communities. However, the uptake of loans was very low, only 2% in the baseline for both the treatment and control groups, which increased to 6% during the follow-up period for the treatment group. It remained the same for the control group. Such low uptake of credit is also very unlikely to drive the main results of this evaluation.

2.3 Participant Selection

The program provided support to 500 households situated in the vicinity of 6 branch offices in Juba. We over-selected the number of potential participants to construct a control group. Selection of potential beneficiaries started in December 2007 and followed three steps. In each branch office, a list of very poor households was prepared by consulting the microfinance group members[4], village elders and local chairpersons. Describing the characteristics of 'very poor' in a post-conflict setting such as Juba was quite challenging. BRAC staff, therefore, often started by asking the community to identify the poorest households in the village. They stopped collecting names when around 10 households had been identified in a village. In subsequent meetings with other people of the same village, the staff used the prior list of names as a reference group and asked for any other households whom they consider to be poorer or equally poor.

Once the household list of a village was prepared, BRAC staff visited each household to collect information on a few basic characteristics including female headship, whether they own/rent the house, main material of the wall of their house, number of dependents and number of regular earners. A regular earner was defined as someone who had been involved in any earning activity for at least 24 days in the past month. This information was used to assign a poverty score to each household. The score was the sum of 4 variables (female headship=2, house wall made of stick/hay=1, living in other's house as charity=1, at least 3 dependents per earner=1). All the households with a score of at least 3 were primarily selected by staff at the branch office. The program manager from the country office visited each of the primarily selected households to verify their status and made the final selection. The households already participating in microfinance were excluded in the final selection. Out of 1,250 primarily selected households, 1,049 households were finally selected and were eligible to participate in the program.

Almost all the households in our sample are female headed. About 80% did not own any cattle in the baseline. While SSCCSE (2010) defined the national poverty line at per capita annual income of SDG 874 (380 dollars), average per capita income of the sample households in 2008 was SDG 595 (259 dollars).

[3] Total amount of transfer in the program was 198 MT of cereal and 56 MT of other food items. According to WFP costing reports in May 2008, the costs of procurement and transportation of these foods to Juba was USD 192,000 with per household cost of USD 384. Including the cost of distributed food, transportation, storage, training and BRAC staff expenses, per beneficiary cost is over 425 dollars.

[4] BRAC-SS have been operating microfinance programs in these study villages for almost 2 years when this study was initiated.

According to a multi-dimensional poverty measure, a baseline report by BRAC-SS found that the households in our study were quite worse-off than the general population of Juba, showing effective targeting by the program (BRAC-SS, 2008). About 44% of these households had no other earner in the household except the female head. In the baseline, only 4% of them were engaged in cultivation, either as primary or secondary activity. Their primary activity was self-employment in the non-farm sector. These mostly included activities such as collecting wild food or fruits, charcoal making, collection and sale of firewood, home based brewery and food processing (baking breads). Wage employment opportunities were highly scarce, and only 12% of the female heads were involved in any work for wage in the baseline.

2.4 Evaluation strategy and data

A randomized evaluation of this program was designed. Once the eligible households were finally selected by BRAC-SS, 500 households were selected randomly for the intervention and the other 549 households were assigned to control group. Randomization was done at household level. A baseline survey was conducted in March 2008, just before the intervention started. Out of the 1049 households, 994 households could be interviewed in the baseline. The follow-up survey took place a year later and 943 households were interviewed. This gives an attrition rate of 5%, which is reasonable given the fact that many of the households were recent returnees from IDP camps. Comparison of the attrited households with the observations in the panel does not reveal major differences in their baseline characteristics (Annex 1). Moreover, treatment status did not have any statistically significant effect on the likelihood of being interviewed in the follow-up survey though attrited households were 7 percentage points less likely to be in the treatment group compared to the households in the panel. Therefore, sample attrition is very unlikely to have a large influence on the results.

The actual intervention did not fully comply with the random assignment to treatment and control group. Among the households in the panel, 14% of the control group wrongly received intervention and 12% of the treatment group did not receive the food transfers (Table 2). Confusion with the names of the respondents has been identified as one of the reasons for this non-compliance. Family names are not commonly used in Juba and a few first names are highly prevalent. For example, in the list of 1058 households, first name of the household heads was Mary in 72 cases. Such misidentification accounts for 20% of the erroneous inclusion/exclusion. There is also considerable variation across the branches in the level of (non)compliance. Jabel Kujur Branch had the lowest compliance (and highest contamination as well) followed by Munuki. It is interesting to note that both these branches among the 6 are located the furthest away from the central office, where the program manager was based. In the 4 other branches, non-compliance was very low. Because of the high level of non-compliance/contamination, the observations from Jebel Kujur branch have been excluded from the analysis resulting with a panel of 814 observations (391 treatment households and 423 control households).

3 Impact on labor supply and informal transfers

The fact that there are control households who received food assistance poses a significant challenge in the impact evaluation. Because of this contamination and non-compliance, we cannot estimate the average treatment effect on the treated (ATT). Contamination is likely to be a more serious problem if the contaminated observations are characteristically different from the rest of the cases. However, there is no apparent selection bias in contamination in terms of their observable characteristics (Annex 2). Among the control households, those who received intervention are not very different from the rest.

In our base analysis, we have used households' initial treatment-control status in random assignment to estimate the impact of program participation.

Table 3 presents a balance check in the baseline of this restricted sample, where column 2 gives the mean values for control group and column 1 shows the tests of the differences of treatment group from the control means. Comparison across a range of baseline characteristics reflects the random assignment of treatment and control groups as they are, on average, not significantly different from each other. A test of the joint significance of all these 18 baseline covariates gives a Wald chi-squared statistic of 15.56 (dof 18), which is not significant (p>0.62).[5] In this sample, 10.6% of the control households and 91.3% of the treatment households participated in the program (the last row). Though this does not appear to be a very high rate of non-compliance, the estimates based on initial random assignment are likely to be downward biased (in absolute terms). Our base estimation uses double-difference to measure standard intention-to-treat (ITT) effects.

$$y_{it} = \beta_0 + \beta_1 treat_i + \beta_2 followup_t + \beta_3 treat_i * followup_t + \delta_k X_i + u_{it} \quad (1)$$

where y_{it} is outcome of household *i* in time *t*, $treat_i$ is the dummy equal to 1 for household who were assigned to treatment in the randomized experiment and 0 for the control group, $followup_t$ is dummy equal to 1 for follow-up survey period and 0 for baseline, X_i is a vector of household controls from the baseline and u_{it} is the error term. While the baseline characteristics are uncorrelated with treatment and unlikely to affect the coefficients, inclusion of these controls can reduce standard errors of the estimates[6]. The coefficient of the interaction between treatment and follow-up interaction (β_3) identifies the impact of the program under two key assumptions of a) common trend between treatment and control groups, and b) no spillover effects of the intervention to the control group. Subsequently, estimates from alternative specifications and sample restriction are also presented to check robustness of the base results.

3.1 Economic activities of household members

Table 4 reports estimates of the impacts of the program on hours spent on economic activities and on income from these activities by the respondent and other adult members of their households. The respondents are the women who were selected from each household to participate in the program. As it has been noted, these women are in most cases the main earner in their households. Both hours of work and income were collected for the year preceding the survey. Between the baseline and the follow-up survey, either the income or hours of work done by the respondents have not changed significantly.[7] More importantly, we do not observe any significant effect of program participation on the amount of work and on income from the work for respondents. Similarly, we do not find any

[5] Balancing check for the full sample (i.e. not dropping observations from Jabel Kujur branch) finds the same level of similarity between the treatment and control groups.

[6] Baseline characteristics included as controls are household demography (number of members in different age groups, sex of household head, number of members with disability), respondent characteristics (age, religion and tribe), variables on migration and experience of crisis event, and branch fixed effects.

[7] There were, however, declining trends in hours worked and income by other adult members of both the treatment and control groups. This general trend reflects the return to Juba of large numbers of previously displaced people, which put excessive pressure on the limited wage employment opportunities, especially for men (Bailey and Harragin, 2009).

evidence of food transfers affecting labor supply of the other adult male or female members of the households.

Attenuation bias, i.e. biasing estimates towards zero effect, due to measurement errors could account for this insignificance of effects. The impact coefficients are not consistently negative casting doubt over possibility of such a bias. Another way to reflect on the precision of the point estimates is to look at the confidence intervals. We can reject the possibility of a reduction of more than 175 hours of work (15% of the baseline mean) or an increase of 324 hours of work (27% of the baseline mean) by the respondents. In other words, the data rejects any disincentive effect of more than 15% of baseline labor supply by the respondents. Standard errors of the impact estimates for the other adult members are also of similar magnitudes. This suggests that even though we find no significant effect on the labor supply and income by the respondents or by other adult members, the point estimates are not very precisely estimated. Moreover, this result of no disincentive effect may not be generalized to all forms of food transfers or in different contexts.

A large part of the empirical research on incentive effects of transfer programs has utilized experiments with welfare reforms in the US. The evidence in general suggests that participation in welfare programs reduce labor supply; and welfare reforms reduce participation in such programs and increase labor supply (Moffitt, 2002). The evidence of such disincentive effects in developing countries is often based on anecdotes (Lentz, 2003). Sahn and Alderman (1996) find that food subsidy in rural Sri Lanka reduced work effort and income. Abdulai et al (2005) argue that most of those evidences suffer from their failure to take endogeneity in program placement and participation into account. Using cross-sectional data from Ethiopia, they find no evidence of disincentive effects after controlling for the household and local level factors of participation. They also rely on endogenous program participation to identify impact estimates. In a randomized evaluation of cash and in-kind (food) transfer in Mexico, Skoufias et al (2008) find no effect of either form of transfers on labor market participation. Therefore, this paper contributes to this empirical literature with a robust assessment of the effect of a short-term food transfer program on labor supply of the poor in a particular context, and the main findings are consistent with the other empirical literature in developing countries.

The findings of this particular evaluation are also relevant to three different strands of theories related to social safety net programs – disincentive effects of transfers, nutritional efficiency wage argument and permanent income hypothesis. These theories make different predictions of the effects of a transfer program.

Disincentive effects in labor economics more commonly predicted based on the labor-leisure framework of labor supply decision (Moffitt, 1992 and 2003; Blundell and MaCurdy, 2000). In the simplest static labor supply model, an increase in non-labor income influences labor supply decisions by moving the budget line away from origin. When aid supplements individuals' earnings, they would become wealthier and consume more of both leisure and other commodities. Therefore, food assistance would create a disincentive to work.[8] However, the welfare policies generally discussed in this literature are very different from the usual food-for-training programs. Eligibility to those welfare policies, as well as the size of transfers, is often linked with actual amount of work/income of the individuals. The particular policy intervention (food-for-training) evaluated in this paper is not conditional on amount of work. The participants, once selected, received their monthly food aid for pre-declared 9 months period.

[8] The predictions from models of disincentive effect of transfers often depend on the underlying model, preference map of the individuals, size of transfer and the transfer structure.

There are various versions of nutritional efficiency wage argument or theories on nutritional poverty trap (Dasgupta, 1997; Dalgaard and Strulik, 2011). Links between nutritional status, capacity to work and productivity are the key to this argument. Although there are concerns with the empirical approaches of investigating this nutrition-productivity nexus (Dasgupta, 1997), Thomas et al (2006) have shown that better health can increase labor supply and productivity. Therefore, it can be argued that short-term food transfer can improve the productivity and work efforts of the beneficiaries.

The transfer program evaluated in this paper makes a short-term increase in non-labor income (like winning a lottery or a similar windfall gain). Based on life-cycle theory, such a one-off transfer may reduce labor supply (and income) but the effect will be distributed over the life-span of the individual. Therefore, the effect on the particular year may not be high enough to be detected. Unlike the labor-leisure models, life-cycle theory predicts increase in consumption (especially of durables) and savings (Imbens et al, 2001). The income effect can also lead to reduced child labor in those households (Basu and Van, 1998).

Although the primary objective of this paper is not to tease out these three sets of predictions, the findings allow reflecting on these theories. While the lack of effect on labor supply matches the prediction of permanent income hypothesis, this could be explained equally well by the nutritional and disincentive effects counteracting to one another. However, subsequent findings presented in this paper relate more closely to permanent income hypothesis.

Table 5 reports the impact estimates on labor supply of children (6-14 year old) and their income. Significant effects are observed both on total hours worked by children and on income earned from these activities. Program participation reduced total amount of work done by the children by 85 hours, which is about 60% of 142 hours spent by children on economic activities per household in the baseline. Similar reduction in income from child labor is also observed. These are quite substantial effects and in line with the "luxury axiom" of child labor (Basu and Van, 1998), where children's non-work is a luxury in the household's consumption basket. Therefore, this cannot be interpreted as disincentive to work.

These effects, estimated separately for male and female children, are also presented in Table 5. Point estimates reveal reduced labor supply by both male and female children though the effects are not statistically significant for male children. During the baseline, female children had higher extent of engagement in earning activities than the male children (85 hours vs. 69 hours), which could be a reason for the different point estimates although the differences are not significantly different.[9] Results on school enrolment presented later in this paper show significant effects on enrolment for girls but not for the boys, which correspond to this gender difference in effects on child labor. These impacts on child labor and enrolment are also in line with the predictions of permanent income hypothesis.

The coefficient of program effect on income earned by the respondent is negative although statistically insignificant at conventional levels (Table 4). The opposite sign of effect on their income, compared to their total hours worked, indicates the possibility of a structural change in their economic activities. Since most of the participants received training on agriculture, a shift to farm self-employment can reduce income over a short period. We do not observe any significant shift in their economic activities (Table 6). As noted earlier, non-farm self-employment (petty trading of different sorts) was the major activity of the respondents and their involvement in cultivation remained minimal. Proportions of

[9] Chow test was conducted to check significance of the coefficients of the two models.

respondents engaged in farming during the baseline period were 3.6% and 4.3% for the treatment and control groups respectively, which did not change significantly during the follow-up period (4.9% and 5.2%). This lack of change in farming activities shows that the trainings were ineffective, and the results are unlikely to be affected by the training component of FFTIG program. We do not observe any heterogeneity in the effects on labor supply or on per capita income between households with/without engagement in farming in the baseline (results not reported). This lack of heterogeneity could also support lack of influence by training but the sample size for households engaged in farming is too small for adequate power.[10]

Another channel through which the disincentive effect may take place is change in work intensity. The hours of work may not fully capture the incentive effect as it does not measure the intensity of work. Since self-employment is by far the major activity, it is plausible that the negative effect on income is driven by reduction in work intensity. We do not observe any significant impact on their productivity (measured as earning from per hour worked) in non-farm self-employment activities (last column in Table 6).

3.2 Informal private transfers

Informal transfers play critical roles in risk-sharing strategies of poor households in developing countries, especially until formal safety net programs are developed. Public transfers are highly likely to create crowding out effects if private transfers are motivated by altruism. However, the theoretical predictions are ambiguous when private transfers are considered to have risk-sharing components embedded in these transactions (Cox et al, 1998). In post-war and fragile settings, such informal insurance can be almost non-existent as conflict and displacement usually reduce informal risk-sharing mechanisms of consumption smoothing (Maria and Andres, 2010). In such situations, formal transfers can play a complementary role to informal transfers and facilitate risk-sharing by allowing the poor households to invest in rebuilding these networks.

As expected, the extent of private transfers (either received or given out) was very low among our sample households in the baseline. Only 9% households reported receiving any transfer/gift and 3% households reported making such transfers during the year preceding the baseline survey. Table 7 reports impact estimates on these transfers. We do not find any impact of program participation on the likelihood of receiving transfers or the value of transfers received. This absence of any crowding-out effect could be due to private transfers being very small to begin with. We observe a significant positive trend in transfer receipts by both treatment and control households. For both treatment and control groups, the likelihoods of receiving transfers increased by about 4% percentage points between the baseline and the follow-up. The value of transfers received also increased by 14 SDG per household, which is about 58% of baseline level of per household transfer receipts. These changes probably reflect that these households started developing risk-sharing as things were settling down.

Unlike zero effects on transfers received, there is a positive impact on both likelihood and value of transfer *given out* by the treatment households. Program participation increased probability of transfers given out by about 6 percentage points, which is quite substantial given that only 3% households reported making such transfers during the baseline period. The impact on the value of transfers given

[10] A qualitative assessment of a similar food transfer program by BRAC, called Income Generation for Vulnerable Group Development (IGVGD), showed that beneficiary households participated primarily to receive food transfers and they were not interested in the training or credit components (Webb et al, 2002).

out in one year is about 15 SDG (6.5 dollars), which is three times the size of annual transfers made by these households in the baseline. The types of transfers indicate that increased transfers came from a greater extent from transfers in-kind rather than cash transfers. This corroborates that the results are an effect of participation in the food transfer program.

These results of informal transfers contrast with most of the empirical literature on this issue. Existing evidence in general demonstrates significant crowding out effects though they differ regarding the magnitudes of these effects. One dramatic example is presented in Cox and Jimenez (1995). According to their estimates in the urban Philippines, a transfer of 100 pesos from public program can reduce informal support by 92 pesos, resulting in only 8 pesos of net gain from 100 pesos of support. Jensen (2003) finds that public transfer for the elderly people in South Africa was counterbalanced by a 20-40% decline in private transfer. Albarran and Attanasio (2005) present the strongest evidence of public safety nets undermining informal support system in Mexico.

Despites these effects on the amount of transfers, an ethnographic study by Heemskerk et al (2004) shows important qualitative change in informal safety net mechanisms. Crowding out can be considered as an acceptable cost for public transfers since reduced private transfer is not a pure wastage (Morduch, 1999). Hoff and Sen (2005) even argue that crowding out informal transfers can be desirable if it helps to break poverty traps arising from ethnic or social class solidarity (Hoff and Sen, 2005). Our results indicate that the households start rebuilding their risk-sharing networks relatively quickly in post-conflict situations. The transfers observed in our study could also be pure altruism though it is very hard to imagine it was the case here for such poor households. Moreover, there is a strong positive correlation between likelihoods of receiving and giving out transfers by households (results not shown), which suggests reciprocity.

4 Impact on welfare

Given the main results of this paper, the effects of this program on household welfare are discussed in this section. We estimate the impact of program participation on income and expenditure, savings, school enrolment, housing and other asset accumulation.

4.1 Per capita household income

Table 8 presents the regression results of equation 1, where the dependent variables are per capita annual income (nominal) and log of per capita income. There is indication of a general decline in per capita income of the households over the one year period though the coefficients for follow-up are not consistently significant. According to monthly CPI of Juba, price declined marginally after September, 2008 (six months after initiation of food transfers). Furthermore, there had been a surge of returnees from IDP camps during this period. According to Bailey and Harragin (2009), only 13% of the returnees come through the official reintegration process and the rest are spontaneous returnees. Most of these returnees were flocking into the urban areas including Juba. There have also been reports of violence between the returnees and settled inhabitants as the unskilled returnees were intensifying pressure on the already saturated labor market. These situations explain the general reduction in income.

We observe a higher reduction in the per capita income for the treatment group (Column 1 in Table 8). Participation in the program appears to have reduced per capita annual income by SDG 120 (52 dollars), which is about 20% of baseline average. Taking the log of per capita income shows a higher effect size of

about 26%. In order to look into the heterogeneity of impact between households with and without child labor in the baseline, Column 2 and 4 show the estimates of the following regression

$$y_{it} = \beta_0 + \beta_1 treat_i + \beta_2 followup_t + \beta_3 treat_i * followup_t + \beta_4 childlab_b + \beta_5 childlab_b * treat_i + \beta_6 childlab_b * followup_t + \beta_7 childlab_b * treat_i * followup_t + \delta_k X_i + u_{it} \quad (2)$$

where y_{it} is per capita income (either in nominal value or in logarithm), and $treat_i$ and $followup_t$ are defined as the same way as in equation 1. $childlab_b$ is a dummy equal to 1 if the household had any child (6-14 year old) engaged in earning activity during the baseline. In this specification, β_3 estimates the impact of the program on households without any child labor in the baseline and β_7 is the difference in impact for households with child labor. Regression of both nominal and log of per capita annual income yield negative values for β_3, indicating that program participation reduced income of the households without child labor in the baseline. However, none of the two coefficients are significant at conventional levels. The coefficients of the triple interaction term are also negative but not significant. The negative point estimates indicate that the possible effects on income are stronger if the household had child labor. Estimated differential impact on log of income is over 4 times higher for households with child labor (β_7= -0.58) than the effect on households without any child labor (β_3= -0.14) although statistically insignificant. In brief, we observe significant negative impact on per capita income and this effect is likely to be driven (at least partially) by reduction in income from child labor.

Barrett (2002) argues that much of the observed negative impact of food transfers on labor supply and household income is primarily due to targeting errors. According to this argument, negative impact on income should be higher for higher income households. Though the study sample consists of extremely poor households, quantile regression shows impact estimates on per capita income (log) at different deciles (Figure 1). Though the coefficients have relatively large confidence intervals, it does not reveal any wealth effect. This particular finding is also relevant to the nutritional poverty trap. If this were the predominant force in determining the average impact of the program, we should have observed positive impacts on the poorer quintiles, which is not the case here.

4.2 Consumption expenditure and savings

Table 9 reports impact estimates on out-of-pocket expenses on food and non-food items along with savings and credit. Food consumption data were collected by 3-day recall method. Since the food transfers ended about 3 months before the follow-up survey, any impact on food consumption is likely to reflect indirect effect of food assistance through changed income and/or taste instead of its direct effect. Any impact on food consumption and reallocation of expenses across different food items during the food assistance period cannot be observed from this data. No impact on current per capita food consumption is observed.[11]

The non-food consumption items are divided into monthly and annual expenditure. Monthly items include transportation, fuel, and toiletries; and annual items are education, clothing, durables, dowry or other ceremonial expenses. Impact on the monthly items would also reflect changes in income or taste since the reference period does not overlap with the period of food transfer. There is no impact on per

[11] Among the different food items (not reported), a decline is observed in consumption of pulses. Wheat and pulse constituted the major part of the food transferred and pulse is not a common item of Sudanese diet. A small decline in alcohol consumption is also observed though the amount spent on this item was very small on average.

capita expenditure incurred for monthly non-food items (second column of Table 9). In terms of specific items (results not reported), a small positive impact is observed in expenditure on toiletries and effects on all the other monthly items are insignificant.

We observe significant effect on annual non-food expenses. The recall period for these non-food expenses was the year between baseline and follow-up and overlaps with the food transfer period. Estimated effect on total expenses on annual non-food items was SDG 117 (51 dollars), which is 47% of total expenses on these items during baseline period. It appears that households have reallocated their income to more non-food items, especially on durables. The positive effect on education expenses is quite large (SDG 122 or 34% of baseline mean) but this effect was imprecisely estimated and hence statistically insignificant (estimates not reported).

The likelihood of a household's borrowing increased because of participation in the program as the households were eligible to borrow from BRAC at the end of training. However, very few of the households (6%) actually availed the credit services from BRAC. The participants increased their borrowing from other sources as well, and were 7 percentage points more likely to have outstanding loans in the follow-up. There was a negative impact of 14 percentage points on savings but it is worth noting that intervention households were 10 percentage points more likely to have any savings in the baseline. Therefore, this decline should not be fully attributed to the program.

4.3 School enrolment of children

Since we observe reduction in child labor and indication of increase in education expenses, it is possible to have an increase in enrolment rates. In fact, we find significant increase in enrolment rates of girls but not for boys. Estimated impact on enrolment rates of girls is about 10 percentage points (Table 10). This impact on enrolment rate is consistent with the findings that the reduction in child labor was higher for girls than boys.

4.4 Housing and other assets

One of the arguments for food transfer is that it helps to prohibit distress sale of assets and thereby protect households from getting trapped into poverty. There is some evidence of this protective role of food transfers in Africa (Gilligan and Hoddinott, 2007). The promotional argument for food transfer is that it can help households to build an asset base.

Since many of these respondents are returnees from different camps and housing quality was one of the key indicators used by the program to identify their potential participants, their housing condition was quite poor. Almost all the households in our sample lived in a single-room makeshift hut with an earthen floor and walls made of rudimentary materials. About 60% of the households owned homestead land. We did not collect data on house improvement expenses. However, there is general improvement on different housing indicators (Table 11). This is expected given the poor state of housing in the baseline. There are some positive effects of program participation on housing quality. However, the participants were more likely to have acquired homestead land and to have replaced their mud-pole walls with unburned bricks. Significant impact is observed on housing space (number of rooms) as well. Only 5% households had electricity connection in the baseline and there is a positive effect (3 percentage points) of program participation.

It seems that the households improved their housing by reallocating their income from food expenses to investment in housing. The size of the food aid was substantially large (USD 384) to yield these improvements in housing. It is also possible that the households have sold (part of) the food aid to invest in housing. If they sold the food, it would have been captured in their income and we should have observed a positive impact on the income of the respondent women. We do not observe such an impact although it is also possible that the participants strategically under-reported such sales. They may have also taken credit from BRAC for their housing improvement. However, we do not observe positive correlations between borrowing from BRAC in the follow-up and different indicators of housing status (results not reported). This indicates that the savings made from reduced food expenditure, which was supported by food transfers, was the major source of investment in housing.

There is no major impact observed on different types of other household assets (Table 12). Program participation did not necessarily lead to any significant accumulation of physical assets. Significant positive effects, albeit relatively small, are observed on the probability of ownership of a shed for livestock (3 percentage points), electric fans (3 percentage points) and insecticide treated nets (8 percentage points). The impact on ownership of a shed for livestock is in line with the homestead/housing improvement. Similarly, the impact on owning electric fans is aligned with higher access to electricity.

5 Robustness checks

In this section, we discuss several concerns, which could cast doubt on the results. Robustness of the main findings has been verified with alternative specification and sample restriction. Table 13 compares the ITT effects discussed so far (Column 1) with three alternative specifications. In column 2, actual recipients of food assistance instead of their treatment-control assignment in RCT have been used in double difference estimate of equation 1. Column 3 has the same specification as in column 1, but all the observations from Munuki branch, which had the highest contamination after Jabel Kujur, have been excluded. These alternatives do not change the major conclusions from our base estimates. We find similar results in all three estimates - decline in income, no effect on hours worked by the respondent and other adult member, decline in child labor, no effect on per capita food and monthly non-food expenditure, positive effect on annual non-food expenditure, no crowding out of receipts of private transfers and increase in transfers given out.

In Column 4, results from instrumental variable (IV) regression are presented. In this estimate, the dependent variables in the second stage are the changes in the variables from baseline to follow-up.

$$y_{it} - y_{it-1} = \beta_0 + \beta_1 \widehat{part_i} + \delta_k X_i + u_{it} \qquad (3)$$

In the first stage, actual participation is regressed on RCT assignment and household characteristics from the baseline. Naturally, the instrument is found to be a very good predictor of participation (F-statistics is 122) in the following first stage regression.

$$part_i = \alpha_0 + \alpha_1 treat_i + \gamma_k X_i + e_{it} \qquad (4)$$

Since the initial assignment was exogenous, it meets the exclusion restriction. However, impact estimates of this IV regression have slightly different interpretation from the base ITT effects. The

estimates show the effects of the program on those participants for whom the initial random assignment increased the chances of participation, the so called local area treatment effect (LATE). The direction and magnitudes of these impact estimates are almost identical to our base results. Similar to our ITT estimates, we find that there was no effect on the labor supply of adult members, extent of child labor and per capita income declined, non-food expenditures increased, informal transfer received was unaffected, and the extent of transfers given out increased. These comparisons build confidence on the results and indicate that non-compliance (or contamination) is not seriously biasing our results.

A second issue, as in most other targeted social programs, is the possibility of spillover effect. Our impact results also indicate possibility of spillover through at least two channels. The program may have a discouragement effect on the control households. For example, the control households may decide to work less hard with the expectations of participating in this program in future. If there is indeed any discouragement effect, this could lead to flawed conclusion of no disincentive effect. Similarly, if part of the increase in transfers given out by the participants is made to the control households, estimated impact on consumption/ expenditure is likely to be biased downwards.

In order to get a sense of the extent of spillover effects, heterogeneity in changes for the control households have been estimated by treatment density. We use GPS data to measure density of treatment households from each of the control households.

$$y_{it} = \beta_0 + \beta_1 followup_t + \beta_2 density_i + \beta_3 density * followup + \delta_k X_i + u_{it} \qquad (5)$$

In equation 4, *density*$_i$ is defined as the number of treatment households living within specific distance from each control household. Only control cases are included in this regression and β_3 might be able to capture if there are any strong spillover effects. In Table 14, column 1-3, different distance cut-offs (25, 50 and 100 meters) have been used to test spillover effects on income of control households. Treatment density does not show any significant association with the change in income of the control households.

Similarly, column 4-6 in Table 14 test whether among the control households, those who lived closer to treated households were differentially more likely to receive transfers. There is no evidence in favor of spillover in terms of probability of transfers received by the control households. Moreover, the size of transfer is very low relative to their per capita income. On average, the treatment households gave out SDG 13 in the follow-up survey, which is 3% of their average annual per capita income. Lack of any association between treatment density and change in transfers received by the control households also indicate that these transfers may not have been given out (or exchanged) with neighbors.

These alternative specifications and robustness checks build further confidence in the main results. One of the remaining limitations of this evaluation is not assessing the impacts, due to lack of data, on nutritional consumption and health status of the participants, which are the primary outcomes of food transfer programs. Another major limitation is that the effects of the three components of the FFTIG program (food transfer, access to credit and training) could not be separated out.

6 Conclusion

Food assistance programs as direct food distribution, food-for-training, food-for-work, school feeding or in many other forms are widespread in every country of the world. There is very little rigorous evidence on the impacts of these food transfer programs, especially in developing countries. On the other hand,

skills training programs are abundant and there could potential complementarities between the two. This paper evaluated the impact of a food-for-training program in South Sudan (which included training and access to credit in addition to food transfers) on labor supply, informal transfers and a few welfare indicators.

We do not find any evidence of disincentive or crowing out effects in this study. However, we find a significant negative impact (20-25%) on per capita income. The major driver of this effect on per capita income is reduction in child labor in the beneficiary households. There is also a positive effect on school enrolment of girls indicating that non-work and schooling is a luxury for these households. There is also a small but significant impact of transfers given out by the participants. This indicates the possibility of short-term targeted aid re-creating risk sharing networks of the poor households.

In terms of effects on welfare, food assistance helped the participants to improve their housing conditions, e.g. homestead ownership increased by about 8 percentage points, and use of better construction materials for houses increased by 9 percentage points. These changes in housing are consistent with positive effects on annual non-food expenditures of the participants. However, no impact is observed on accumulation of other major physical assets. Since many of the households were recent returnees from camps and many of them had been living in make-shift houses, this priority of investment in housing is understandable. This also show that the households spread the gain from a 'short-term' food transfer over a life-time by investing in housing, instead of reducing current labor supply, as the permanent income hypothesis predicts.

These findings support food transfer programs as a short-term solution by making it clear to the participants about the duration of supports. This could allow the poor households to invest in durables, such as housing, that are necessary conditions for sustainable livelihood. However, such programs may not be successful in affecting productivity and/or income of the participants. Given that food transfer programs are quite costly, alternative transfer programs (such as transfer of productive assets) should be piloted.

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Table 1. Selected social indicators for South Sudan

Social indicators	Central Equatoria in South Sudan (2006)[a]	Sub-Saharan Africa (2006)[b]
Infant mortality (per 1000)	107	89
Life expectancy at birth	42	51
Maternal mortality (per 100,000 live births)	1867	900
Contraceptive prevalence (%)	7.5	22.8
Net primary enrollment	43	72
Primary completion rate	1.6	60
Access to improved water sources (%)	36	58

[a] Sudan Household Survey-2006; Juba is located in Central Equatoria region. [b] World Bank online Database

Table 2. Compliance with RCT design

		Branch					
	Full sample	Atlabara	Munuki	Hai-gabat	Jabel Kujur	Buluk	Katun
Treatment	0.733	0.820	0.683	0.810	0.271	0.829	0.918
(1=yes, 0=No)	(0.022)***	(0.046)***	(0.056)***	(0.043)***	(0.085)***	(0.047)***	(0.031)***
Control mean	0.141	0.071	0.187	0.086	0.358	0.108	0.068
	(0.016)***	(0.028)**	(0.040)***	(0.031)***	(0.059)***	(0.036)***	(0.027)**
Observations	943	158	173	187	129	138	158
R-squared	0.54	0.68	0.46	0.65	0.07	0.68	0.83

Robust standard error in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

Table 3. Treatment-control balance (excluding Jabel Kujur branch)

Baseline characteristics	Treatment (1=yes, 0=No)	Control mean	n
	(1)	(2)	
Household size	0.066 (0.169)	5.377 (0.123)***	813
Number of children (<15 years old)	0.006 (0.118)	1.855 (0.084)***	813
Number of working aged (15-65 years) male	-0.044 (0.097)	1.509 (0.070)***	813
Number of working aged (15-65 years) female	0.106 (0.080)	1.955 (0.055)***	813
Number of old (>65 years) members	-0.006 (0.017)	0.059 (0.012)***	813
Number of household members with disability	-0.033 (0.031)	0.171 (0.022)***	813
Maximum years of schooling in the household	-0.148 (0.118)	2.807 (0.085)***	784
Male headed households (1=Yes, 0=No)	-0.005 (0.013)	0.036 (0.009)***	813
Respondents can read and write (1=Yes, 0=No)	-0.025 (0.029)	0.227 (0.020)***	813
Respondent is married (1=Yes, 0=No)	0.046 (0.029)	0.197 (0.019)***	813
Age of the respondent (in years)	0.693 (0.821)	45.03 (0.572)***	812
Respondents' religion (1=Catholic, 0=other)	-0.018 (0.035)	0.581 (0.024)***	813
Respondents' tribe (1=Bari, 0=Other)	0.010 (0.033)	0.315 (0.023)***	813
Respondent was born in the same district where currently living (1=Yes, 0=No)	-0.036 (0.031)	0.279 (0.022)***	814
Number of relatives (households) in the village	-0.996 (0.607)	6.950 (0.454)***	814
Owns homestead land (1=Yes, 0=No)	-0.033 (0.033)	0.693 (0.022)***	814
Owns house (1=Yes, 0=No)	0.000 (0.035)	0.443 (0.024)***	808
Own cattle (1=yes, 0-No)	-0.035 (0.028)	0.222 (0.020)***	814
Any member was seriously ill last year (1=Yes, 0=No)	-0.027 (0.035)	0.600 (0.024)***	812
Received food transfers (1=yes)	0.807 (0.021)***	0.106 (0.015)***	814

Excluding observations from Jabel Kujur branch

Robust standard error in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

Table 4. Engagement in earning activities by adult household members

	Respondent		Other male adult		Other female adult	
	Hour	Income	Hour	Income	Hour	Income
Treatment	-33.87	160.42	-129.53	-221.65	8.26	-37.64
(1=Yes, 0=Control)	(85.81)	(110.35)	(83.26)	(122.98)*	(64.29)	(86.33)
Follow-up	77.09	-50.85	-202.30	-330.64	-119.67	-100.81
(1=2009, 0=2008)	(88.43)	(105.47)	(76.47)***	(116.61)***	(59.70)**	(94.46)
	75.03	**-175.38**	**21.10**	**190.09**	**-1.86**	**-29.45**
Treatment X follow-up	**(127.22)**	**(151.71)**	**(106.94)**	**(170.67)**	**(87.84)**	**(133.14)**
Constant	1,235.74	608.90	63.81	-118.25	-358.50	-459.48
	(246.14)***	(257.05)**	(181.61)	(293.33)	(175.76)**	(237.82)*
Observations	1,618	1,618	1,618	1,618	1,618	1,618
R-squared	0.11	0.06	0.08	0.10	0.07	0.07

Note: Includes baseline characteristics and branch dummies.
Robust standard error in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

Table 5. Engagement in earning activities by children

	All children		Male children		Female children	
	Hour	Income	Hour	Income	Hour	Income
Treatment	6.52	54.86	-4.97	20.60	11.49	34.26
(1=Yes, 0=Control)	(38.68)	(35.64)	(26.13)	(20.66)	(27.61)	(27.71)
Follow-up	-25.70	-24.89	-19.93	-5.84	-5.76	-19.05
(1=2009, 0=2008)	(35.06)	(27.20)	(25.02)	(18.40)	(23.17)	(18.95)
Treatment X	**-85.28**	**-105.69**	**-25.62**	**-43.50**	**-59.66**	**-62.19**
follow-up	**(47.42)***	**(42.20)****	**(32.87)**	**(28.27)**	**(33.35)***	**(29.82)****
Constant	53.76	23.95	21.13	-2.98	32.64	26.92
	(92.40)	(98.84)	(65.89)	(82.83)	(62.19)	(53.05)
Observations	1618	1618	1618	1618	1618	1618
R-squared	0.03	0.04	0.02	0.02	0.03	0.03

Note: Includes baseline characteristics and branch dummies.
Robust standard error in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6. Business activities of the respondent

	Hours worked				Non-farm productivity
	Wage employment	Farm self-employment	Non-farm Self-employment	Other employment	
Treatment	4.00	-14.47	-15.74	-7.66	0.18
(1=Yes, 0=Control)	(41.38)	(14.78)	(83.37)	(26.38)	(0.15)
Follow-up	-87.16	14.65	108.23	41.37	-0.44
(1=2009, 0=2008)	(38.65)**	(17.54)	(85.78)	(30.49)	(0.12)***
Treatment X	**2.56**	**33.92**	**47.79**	**-9.24**	**-0.19**
follow-up	**(53.03)**	**(28.34)**	**(123.71)**	**(40.70)**	**(0.19)**
Constant	190.09	44.76	830.90	170.00	1.39
	(109.95)*	(38.23)	(236.35)***	(89.69)*	(0.37)***
Observations	1618	1618	1618	1618	949
R-squared	0.03	0.03	0.11	0.03	0.11

Note: Includes baseline characteristics and branch dummies.
Robust standard error in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%
Non-farm productivity has been measured as per hour income from such activities

Table 7. Impact on informal private transfers

	Transfers received (1=yes)	Amount of transfer received (in SDG)	Transfers given out (1=Yes)	Amount of transfer given out (in SDG)	Transfer made in cash (1=yes)	Transfer made in kind (1=yes)
Treatment (1=Yes, 0=Control)	0.01	10.00	-0.01	-9.74	-0.01	0.00
	(0.02)	(8.04)	(0.01)	(4.51)**	(0.01)	(0.00)
Follow-up (1=2009, 0=2008)	0.04	14.40	0.02	-6.05	0.00	0.02
	(0.02)**	(6.96)**	(0.01)	(5.02)	(0.01)	(0.01)***
Treatment X follow-up	**0.04**	**9.07**	**0.06**	**14.87**	**0.02**	**0.04**
	(0.03)	**(12.18)**	**(0.02)****	**(6.22)****	**(0.02)**	**(0.01)*****
Constant	0.09	16.47	0.01	5.12	-0.00	0.01
	(0.06)	(21.86)	(0.04)	(7.66)	(0.03)	(0.02)
Observations	1,603	1,618	1,613	1,618	1,618	1,618
R-squared	0.05	0.04	0.05	0.02	0.03	0.07

Note: Includes baseline characteristics and branch dummies.
Robust standard error in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

Table 8. Impact on household income

	Per capita income		Ln(per capita income)	
	(1)	(2)	(3)	(4)
Treatment	-18.58	-32.09	0.04	-0.02
(1=Yes, 0=Control)	(47.26)	(54.76)	(0.09)	(0.10)
Follow-up	-53.02	-72.44	-0.17	-0.21
(1=2009, 0=2008)	(49.84)	(57.03)	(0.09)*	(0.10)**
Treatment X follow-up	**-121.95**	**-80.63**	**-0.26**	**-0.14**
	(64.20)*	**(73.99)**	**(0.13)***	**(0.15)**
Child Labor (in 2008)	-	-44.98	-	0.03
(1=Yes, 0=No)		(84.39)		(0.20)
Child Labor X treat	-	113.87	-	0.35
		(111.25)		(0.25)
Child Labor X follow-up	-	-11.58	-	0.02
		(115.19)		(0.30)
Child Labor X treat X Follow-up	**-**	**-41.34**	**-**	**-0.58**
		(156.82)		**(0.39)**
Constant	608.75	369.38	5.75	5.45
	(116.89)***	(117.35)***	(0.24)***	(0.23)***
Observations	1,618	1,618	1,618	1,618
R-squared	0.12	0.12	0.09	0.10

Note: Includes baseline characteristics and branch dummies as controls.
Robust standard error in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

Table 9. Impact on expenditure, savings and borrowing

	Per capita 3-day food	Per capita monthly non-food	Per capita annual non-food	Have outstanding loan	Have savings at home
Treatment	3.33	11.62	18.04	-0.02	0.10
(1=Yes, 0=Control)	(0.93)***	(7.05)*	(29.19)	(0.02)	(0.03)***
Follow-up	-0.91	14.34	-46.29	-0.03	-0.05
(1=2009, 0=2008)	(0.78)	(5.37)***	(29.19)	(0.01)**	(0.03)
Treatment X follow-up	**-0.82**	**7.59**	**116.83**	**0.07**	**-0.14**
	(1.67)	**(11.56)**	**(61.53)***	**(0.02)*****	**(0.05)*****
Constant	11.92	17.96	227.83	-0.01	0.46
	(2.67)***	(23.07)	(103.98)**	(0.04)	(0.08)***
Observations	1613	1618	1613	1618	1618
R-squared	0.13	0.08	0.07	0.04	0.19

Note: Includes baseline characteristics and branch dummies.
Robust standard error in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

Table 10. Enrolment (6-14 years old)

	Enrolment	
	Boys	Girls
Treatment	0.01	-0.03
(1=Yes, 0=Control)	(0.04)	(0.04)
Follow-up	-0.18	-0.29
(1=2009, 0=2008)	(0.04)***	(0.04)***
Treatment X follow-up	**-0.02**	**0.10**
	(0.06)	**(0.06)***
Constant	-0.25	-0.24
	(0.27)	(0.26)
Observations	990	1,016
R-squared	0.15	0.20

Note: Includes household level baseline characteristics and branch dummies. Additional controls in this regression are children's individual characteristics (i.e. age, disability and marital status). Robust standard error in parentheses;
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 11. Impact on housing

	Own homestead	Wall made of mud-pole	Number of rooms	Access to safe water	Electricity connection
Treatment	-0.02	0.02	-0.12	-0.02	-0.02
(1=Yes, 0=Control)	(0.03)	(0.03)	(0.08)	(0.02)	(0.01)
Follow-up	0.07	-0.10	0.54	0.09	-0.01
(1=2009, 0=2008)	(0.03)**	(0.03)***	(0.08)***	(0.02)***	(0.01)
Treatment X follow-up	**0.08**	**-0.09**	**0.18**	**0.04**	**0.03**
	(0.04)**	**(0.05)***	**(0.10)***	**(0.03)**	**(0.02)***
Constant	0.59	0.55	1.03	0.06	0.05
	(0.08)***	(0.09)***	(0.19)***	(0.06)	(0.04)
Observations	1,618	1,601	1,520	1,604	1,586
R-squared	0.13	0.10	0.22	0.10	0.02

Note: Includes baseline characteristics and branch dummies.
Robust standard error in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

Table 12. Impact on asset ownership [a]

	Cultivable land	Livestock	Livestock shed	Shop premises	Electric fan	Bed	Bed net (ITN)
Treatment	-0.00	-0.03	-0.02	-0.01	-0.02	0.02	-0.04
(1=Yes, 0=Control)	(0.03)	(0.03)	(0.01)*	(0.02)	(0.01)	(0.03)	(0.03)
Follow-up	-0.06	0.01	-0.01	-0.05	-0.02	0.11	-0.06
(1=2009, 0=2008)	(0.03)**	(0.03)	(0.01)	(0.01)***	(0.01)	(0.03)***	(0.03)**
Treatment X follow-up	**0.01**	**-0.02**	**0.03**	**0.01**	**0.03**	**-0.04**	**0.08**
	(0.04)	**(0.04)**	**(0.02)***	**(0.02)**	**(0.02)***	**(0.04)**	**(0.04)***
Constant	0.16	-0.04	0.02	0.06	0.06	0.63	0.08
	(0.07)**	(0.07)	(0.03)	(0.03)**	(0.03)*	(0.07)***	(0.07)
Observations	1,618	1,618	1,618	1,618	1,618	1,618	1,618
R-squared	0.05	0.05	0.02	0.05	0.02	0.08	0.04

Note: Includes baseline characteristics and branch dummies.
Robust standard error in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%
[a] dependent variables are whether owns the asset [1] or not [0];

Table 13. Impact estimates with alternative identification

Dependent variable	Treatment in RCT (1)	Actual treatment (2)	Excluding Munuki branch (3)	IV regression (4)
Hours worked by respondent	75.03 (127.22)	0.60 (127.22)	30.94 (139.90)	74.74 (136.84)
Hours worked by other adult male members	21.10 (106.94)	46.24 (106.88)	-4.91 (116.90)	20.91 (121.78)
Hours worked by other adult female members	-1.86 (87.84)	32.76 (87.75)	0 .12 (90.77)	7.86 (101.82)
Hours worked by children	-85.28 (47.42)*	-88.70 (47.52)*	-55.48 (53.44)	-89.61 (45.83)**
Log (per capita income)	-0.26 (0.13)*	-0.28 (0.23)	-0.30 (0.15)*	-0.37 (0.17)**
Per capita 3-day food expenditure	-0.82 (1.67)	0.32 (1.64)	-0.20 (2.06)	-0.69 2.07
Per capita non-food expenditure (last month)	7.59 (11.56)	-5.35 (11.36)	-1.59 (12.72)	11.29 14.16
Per capita non-food expenditure (last year)	116.83 (61.53)*	144.97 (60.76)**	179.56 (75.49)**	146.01 (73.19)**
Transfers received (1=yes)	0.04 (0.03)	0.05 (0.03)	0.06 (0.04)	0.05 (0.04)
Amount of transfers received (in SDG)	9.07 (12.18)	8.33 (12.07)	11.77 (14.75)	6.83 (14.83)
Transfers given out (1=yes)	0.06 (0.02)**	0.04 (0.02)**	0.06 (0.02)***	0.07 (0.03)***
Amount of transfers given out (in SDG)	14.87 (6.22)**	7.84 (6.29)	15.35 (6.98)**	18.53 (7.41)***

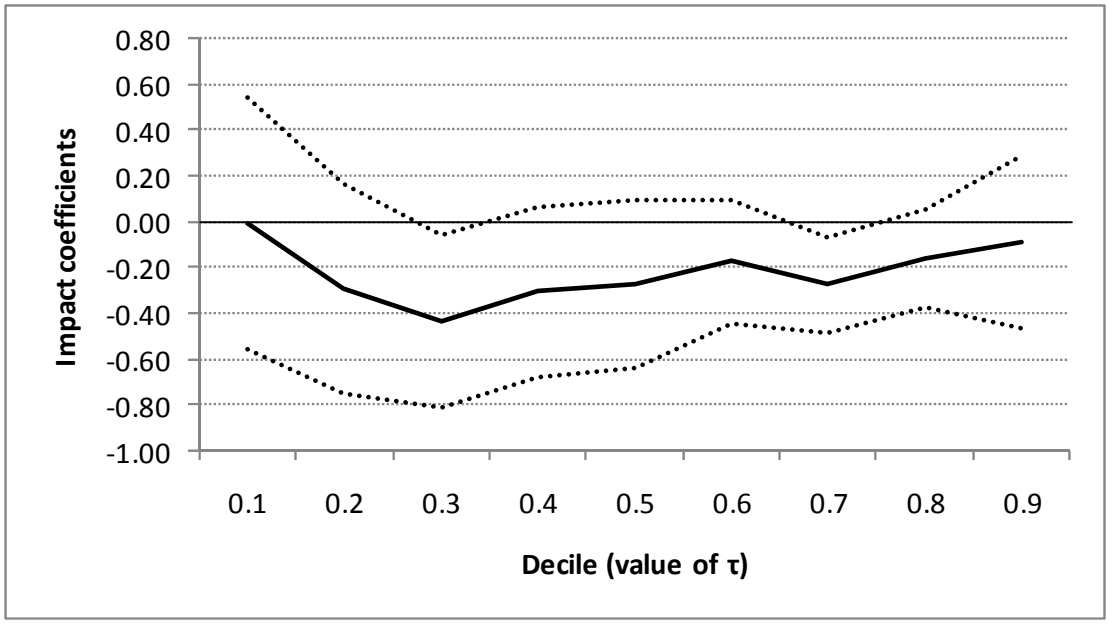
Column 1: Base results;
Column 2: Using actual treatment instead of RCT assignment in double-difference estimates;
Column 3: Dropping observations from Munuki branch and using the same specification as in the base estimate.
Column 4: Using RCT assignment as instrument for actual treatment with full sample (i.e. only excluding Jabel Khujur banch). F-statistics of first stage is 122.33
Robust standard error in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

Table 14. Spillover effect for control households on transfers received

	Log of per capita income			Received transfers		
	25 meter	50 meter	100 meter	25 meter	50 meter	100 meter
Follow-up (1=2009, 0=2008)	-0.132 (0.126)	-0.203 (0.134)	-0.176 (0.145)	0.069 (0.028)**	0.065 (0.030)**	0.041 (0.033)
Treatment density	-0.065 (0.060)	-0.034 (0.036)	-0.004 (0.018)	0.005 (0.015)	-0.000 (0.008)	-0.003 (0.004)
Follow-up X Treatment density	0.103 (0.112)	0.075 (0.047)	0.023 (0.021)	-0.007 (0.020)	-0.001 (0.010)	0.004 (0.005)
Constant	6.325 (0.394)***	6.342 (0.400)***	6.298 (0.411)***	-0.053 (0.081)	-0.049 (0.083)	-0.035 (0.085)
Observations	558	558	558	649	649	649
R-squared	0.104	0.107	0.105	0.09	0.09	0.09

Note: Includes baseline characteristics and branch dummies.
Robust standard error in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%
[a] treatment density is defined as number of treatment HHs within 'x' (25, 50 or 100) meter radius of each control hosuehold.

Figure 1. Quantile regression of log of per capita income



Annex 1. Sample attrition

Baseline characteristics	Attrited	Mean for panel	n
Treatment status in RCT (1=Treated, 0=Control)	-0.07 (0.07)	0.48 (0.02)***	994
Household size	-0.43 (0.31)	5.31 (0.08)***	990
Number of children (<15 years old)	-0.09 (0.27)	1.87 (0.06)***	990
Number of working aged (15-65 years) male	-0.17 (0.18)	1.44 (0.04)***	990
Number of working aged (15-65 years) female	-0.19 (0.15)	1.95 (0.04)***	990
Number of old (>65 years) members	0.02 (0.05)	0.06 (0.01)***	990
Number of household members with disability	0.00 (0.05)	0.14 (0.01)***	990
Maximum years of schooling in the household	0.20 (0.24)	2.64 (0.06)***	939
Male headed households (1=Yes, 0=No)	0.01 (0.03)	0.03 (0.01)***	989
Respondents can read and write (1=Yes, 0=No)	0.09 (0.07)	0.20 (0.01)***	989
Respondent is married (1=Yes, 0=No)	-0.05 (0.06)	0.24 (0.01)***	989
Age of the respondent (in years)	-0.45 (2.00)	45.12 (0.39)***	988
Respondents' religion (1=Catholic, 0=other)	0.02 (0.07)	0.60 (0.02)***	989
Respondents' tribe (1=Bari, 0=Other)	-0.11 (0.06)*	0.34 (0.02)***	989
Respondent was born in the same district (1=Yes, 0=No)	0.05 (0.07)	0.24 (0.01)***	994
Number of relatives (households) in the village	-1.29 (0.97)	6.17 (0.28)***	994
Owns homestead land (1=Yes, 0=No)	0.12 (0.06)**	0.69 (0.02)***	994
Owns house (1=Yes, 0=No)	-0.03 (0.07)	0.45 (0.02)***	980
Own cattle (1=yes, 0-No)	-0.07 (0.05)	0.19 (0.01)***	994
Any member was seriously ill last year (1=Yes, 0=No)	0.01 (0.07)	0.62 (0.02)***	988
Hours worked by the respondent in the last year	-107 (183.7)	1,184 (43.34)***	994
Hours worked by other adult members in the last year	36.22 (238.0)	1,001 (55.02)***	994
Hours worked by children in the last year	-85.83 (74.48)	166.6 (21.67)***	994
Log(annual per capita income)	-0.18 (0.14)	5.77 (0.04)***	875
Whether received any transfer last year (1=Yes, 0=No)	-0.02 (0.04)	0.08 (0.01)***	974
Whether given out any transfer last year (1=Yes, 0=No)	-0.01 (0.02)	0.03 (0.01)***	986

Note: observations include all the cases in the baseline; Attrited [1] if could not be followed-up, else [0];
Robust standard error in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

Annex 2. Selection bias due to contamination

Baseline characteristics	Actual Treatment	Control mean	n
Household size	-0.20 (0.28)	5.29 (0.12)***	488
Number of children (<15 years old)	0.01 (0.22)	1.86 (0.09)***	488
Number of working aged (15-65 years) male	-0.00 (0.17)	1.46 (0.07)***	488
Number of working aged (15-65 years) female	-0.17 (0.13)	1.91 (0.05)***	488
Number of old (>65 years) members	-0.04 (0.02)	0.07 (0.01)***	488
Number of household members with disability	-0.01 (0.06)	0.15 (0.02)***	488
Maximum years of schooling in the household	-0.16 (0.23)	2.70 (0.09)***	462
Male headed households (1=Yes, 0=No)	-0.02 (0.02)	0.04 (0.01)***	488
Respondents can read and write (1=Yes, 0=No)	0.01 (0.05)	0.21 (0.02)***	488
Respondent is married (1=Yes, 0=No)	0.08 (0.06)	0.21 (0.02)***	488
Age of the respondent (in years)	-2.76 (1.44)*	45.11 (0.59)***	487
Respondents' religion (1=Catholic, 0=other)	-0.06 (0.06)	0.61 (0.02)***	488
Respondents' tribe (1=Bari, 0=Other)	0.02 (0.06)	0.34 (0.02)***	488
Respondent was born in the same district where currently living (1=Yes, 0=No)	-0.04 (0.05)	0.26 (0.02)***	490
Number of relatives (households) in the village	-2.34 (0.77)***	6.80 (0.46)***	490
Owns homestead land (1=Yes, 0=No)	0.13 (0.05)**	0.68 (0.02)***	490
Owns house (1=Yes, 0=No)	-0.09 (0.06)	0.47 (0.02)***	483
Own cattle (1=yes, 0-No)	-0.03 (0.05)	0.21 (0.02)***	490
Any member was seriously ill last year (1=Yes, 0=No)	0.03 (0.06)	0.63 (0.02)***	488
Hours worked by the respondent in the last year	154.0 (197.54)	1,217 (64.24)***	490
Hours worked by other adult members in the last year	-140 (183.6)	1,053 (86.87)***	490
Hours worked by children in the last year	11.95 (91.90)	177.35 (35.12)***	490
Log(annual per capita income)	-0.05 (0.18)	5.76 (0.07)***	419
Whether received any transfer last year (1=Yes, 0=No)	-0.05 (0.03)**	0.08 (0.01)***	482
Whether given out any transfer last year (1=Yes, 0=No)	0.01 (0.03)	0.03 (0.01)***	485

Note: Sample includes households assigned as control in RCT.

Robust standard error in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

Quantity vs. quality of education: Exam difficulty and labor market performance in Bangladesh

Abstract

Substantial increases in access to education in the last few decades have been one of the major achievements in human development for many developing countries. With this success in increasing the quantity of education, its quality has emerged as a major challenge for most of these countries. Despite its intuitive appeal, improving standards in curriculum-based external exit examinations has received relatively little attention as a possible means of improving the quality of education, especially in developing countries. Using a "natural experiment" in the secondary school exit exam in Bangladesh, this paper evaluates the labor market effects of lowering exam difficulty. When exam standards are high, passing the exit exam increases the likelihood of attaining formal employment by 12-13 percentage points (compared to those who completed similar number of years of education but did not pass the exam) for the male sample, and by about 7 percentage points for female. When exam standards are low, these labor market gains disappear. Further evidence shows that this decline in labor market gains is driven by lower human capital accumulation during the experimental period and/or by the general equilibrium effect of greater supply of secondary school graduates. The results indicate the need for greater emphasis on exam standard to improve labor market performance of the graduates.

Keywords: Exam standard, education, labor market.
JEL classification: I23; J24.

1. Introduction

Education is considered as one of the mainstays of development, both for its intrinsic value as well as the instrumental role it plays in expanding other opportunities. Celebrating 20 years of human development reports, UNDP (2010) denotes that progress in education has been more extensive than progresses in health and income.[1] It is also commonly noted that years of schooling does not necessarily measure learning or cognitive abilities: the quality of that education also counts. There are claims that the quality, rather than the quantity, of education plays a more important role in determining income of an individual and economic growth of a country (Hanushek and Woessmann, 2007).[2] One of the potentially effective policy mechanisms for improving the quality of education in developing countries is the standardized exit exam. The level of difficulty set in these exams can have a strong influence both on the learning of students and on their success in the labor market.

Despite its potential importance in influencing the quality of education, the standards of curriculum-based external exit examinations (CBEEE) have received relatively little attention in policy discussion compared to increasing educational inputs. A review piece by Glewwe and Kremer (2006) takes stock of the evaluations of policy interventions for improving education quality and the academic performance of students in developing countries. The policies discussed in this review are primarily concerned with inputs and their delivery mechanisms, such as more teachers, textbooks, other educational materials, remedial education, computer-aided instructions, or radio instructions, amongst others. In essence, these policies only consider the educational production function. Becker and Rosen (1992), Costrell (1994) and Betts (1998) have used incentives of learning to model the influence of standardized exams and grading standards on competency, graduation, school drop-out and performance in labor market. Assuming that passing a standardized exam signals the ability of graduates to employers, these models have shown that higher standards in exit exams can induce students to exert greater efforts to learn. Using a natural experiment from Bangladesh, this paper estimates the effect of lowering the difficulty in secondary school exit exams on the labor market performance of graduates.

During 1992 to 1995, the introduction of a new assessment system led to a reform in the structure of secondary school exit exam in Bangladesh. Previously, examinees were assessed on their descriptive answers to exam questions. In 1992, multiple choice questions (MCQ) were introduced in a new assessment system. As part of this reform, the students also received 'question banks', a set of 500 MCQ for each subject. While these question banks were meant to be used as guides in exam preparation, all of the actual MCQ questions in the exam were drawn from this set of questions. This meant that many students could easily pass the exam by studying only the MCQs in the question banks. After realizing that the introduction of question banks had led to a drastic fall in the standard of the exam, the exam structure was again changed in 1996. This paper compares the cohort exposed to this "experiment" with

[1] Average years of schooling of adults (aged 15 or older) in developing countries have increased from a mere 2.55 years in 1960 to 7.09 years in 2010 (Barro and Lee, 2010).

[2] Hanushek and Zhang (2009) indicate that typical estimates of returns to education are about 30% lower than quality adjusted return to schooling.

both their older and younger cohorts in order to assess the impact of the lowering of exam standards on their labor market performance.

The results reveal that easier exams reduced the probability of getting formal employment and wage rates for the secondary school graduates. During the periods of the 'conventional standard' – before and after the use of question banks – the students who passed the exit exam were 12 to 13 percentage points more likely to get salaried employment and earned about 10% higher wage in comparison with students who completed similar number of years of schooling but either did not take the exam or failed in the exam. However, we do not observe any statistically significant labor market gain for those students who passed the same exam during the question bank period. The effect of a lower exam standard is more evident for male students than female, which is most likely due to the limited labor market participation of female graduates. However, the estimates are consistent across different specifications for the male sample, and are causal effects of the exam policy.

Two mechanisms might underlay the results. Firstly, lower exam standard may have reduced the incentives to exert efforts to learn, which lead to a lower level of human capital acquisition by the graduates. Secondly, greater number of students passing during the experimental period may have reduced the likelihood of acquiring formal employment. Despite a lack of evidence on the relative importance of human capital and general equilibrium effects, the results call for greater attention to the standards of exit exams as a possible means of improving the quality of education and its economic return in developing countries.

The paper is organized as follows: Section 2 gives a brief review of relevant literature on the relationship between exam standards and labor market performance. Section 3 gives an overview of Bangladesh's education sector, along with a description of the structure of secondary schooling and the natural experiment. Section 4 describes the data and specifications used. Main results are presented in Section 5. Section 6 examines the possible mechanisms underlying the effects of the exam experiment. Additional robustness checks have been done in Section 7, and Section 8 concludes the paper.

2. Literature review

The standard of exit exams has received relatively little attention compared to educational inputs in the literature on improving the quality of education in developing countries. However, a number of theoretical arguments are often put forward in favor of both introducing standardized exams and setting a high standard in these exams in order to improve the quality of and the return to education. According to commonly cited arguments, these policies a) signal the accomplishment of students to potential employers, thus inducing them to compete and exert more efforts towards learning (Becker and Rosen, 1992), b) can increase efforts and inputs from teachers, school management and parents (Bishop, 1997), c) reduce peer denigration of more studious students and their tolerance of disruptive behavior (Bishop et al 2000); and d) reduce the marginal cost of higher standards for schools under

pooling[3] (Costrel, 1994). Though the importance of education in these above-mentioned models is derived from the role it plays in signalling the ability of graduates' to potential employers, higher standard in education can also increase workers' productivity through human capital accumulation.[4]

The main concern, however, against setting higher exam standards is primarily distributional. While higher standards can increase the efforts of more able students, less able students may cease to exert any effort at all. Based on this reasoning, Crostell (1994) argues that an egalitarian policy maker may choose a lower exam standard compared to a standard that maximizes students' future income. Betts (1998) makes a counter-argument showing that an egalitarian planner may set higher standards than an income-maximizing planner *if* the employers cannot fully distinguish the productivity of an individual worker. He makes the case that with an increase in grading standards, the pool of less-able workers starts including some of the higher ability workers who would have otherwise graduated. Eventually, firms identify an increase in the average productivity of less-able workers and make an upward adjustment in their wage, which benefit the least-able workers. Andrade and Castro (2008) present a different case for possible counter-productive outcomes of higher exam standards. Their argument relies on the assumption that exams do not evaluate non-cognitive skills, which along with cognitive skills, are factors in determining labor productivity. By raising standards, tests may influence students to emphasize more on the skills assessed in exams and give less attention to their non-cognitive skills. This can, in turn, reduce their average productivity when they enter the labor market. Despite these possible concerns, exam standards in theoretical literature are expected to improve quality of education.

Empirical studies on this subject are almost entirely based on data from developed countries. Bishop et al (2000) compare students from New York with other states in the United States (US), to assess whether the state-wide requirement of CBEEE affects students' learning aptitudes, dropout and performance in the labor market. They find the 8th graders from New York perform significantly better than the rest in mathematics tests, but observe no difference in school dropout rates. A similar cross-state comparison in Canada also shows significant association between provincial CBEEEs and student performance in aptitude tests (Bishop, 1997). However, both these papers suffer from possible omitted variable bias since the states and/or provinces adopting these policies are likely to differ in many other important aspects.

A cross-country study by Woessmann (2003) uses a large dataset covering 39 OECD and middle-income countries. Controlling for a large set of variables, he finds a significant association between central

[3] In a decentralized system where each school sets their own standards, raising standard can be costly for schools as more students will decide not to meet it since they cannot reap the full payoff because of pooling in the labor market.

[4] Following the seminal works of Spence (1973) and Stiglitz (1975), there has been a long literature debating human capital and sorting hypotheses of returns to education. From human capital perspective, the most careful and influential approaches on estimating return to education used effects of birth date on schooling (Angrist and Krueger, 1991), schooling differences of identical twins (Ashenfelter and Krueger, 1994) and policies affecting access to education (Dufflo, 2001). Riley (1979), Lang and Kropp (1986), Bedard (2001), among others, try to disentangle the two effects. However, the difficulty in empirically distinguishing between these two competing roles of education arises due to their almost identical predictions, and Weiss (1995) explains the reasons for doubting existing evidence on this debate.

examinations and students' performance in mathematics tests, but a relatively weak correlation with science tests. Statistical significance of these results depends on the unit of clustering. Constructing a measure of teacher-level grading standard from a panel dataset in Florida, Figlio and Lucas (2004) find that higher grading standards adopted by teachers is associated with greater learning of their students in elementary schools. A similar approach for 12^{th} graders in the US shows a strong influence of grading standards on test performance, but not on graduation (Betts and Grogger, 2003).

While the evidence points towards a favorable effect of exam/grading standards on students' incentives to acquire academic competency, there are a few studies that evaluate the labor market effects of exam standards. In this literature, it is hypothesized that better education will lead to better job opportunities for graduates by increasing their productivity (*learning*) and/or by transmitting information about their unobservable abilities (*signalling*). Bishop and Mane (2005) find that both minimum competency exams and higher course requirements for graduation in some of the states in the US are positively associated with total earnings as well as the wage rates of graduates. More importantly, these associations are stronger for medium-term labor market outcomes (eight years after graduation) than short-term outcomes (within 21 months of graduation). Though their study controls for a few socio-economic backgrounds of the students, any independent state effect could bias their estimates. Contrary to these results, Betts and Greggor (2003) do not find any association between grading standards and graduates' earning. Dee and Jacob (2006) look at the heterogeneity in the effects of exit-exams and do not find any clear evidence of higher standards exacerbating inequality in the US. While dropout rates for black students increase with the introduction of CBEEE, graduates from the black community and Hispanic females exposed to the graduation requirement perform better in the labor market.

To the best of my knowledge, there has yet not been any study that looks into the labor market effects of exam or grading standards in developing countries. This is not surprising given that increasing access to education has been the priority for the policymakers in low-income countries during the past few decades. Consequently, schooling attainment and the actual competency of students have received relatively little attention. A comparison of the quality of education shows that children in developing countries perform 20% standard deviation lower than students of the same grades in developed countries (Glewee and Kremer, 2006). This is equivalent to almost three grade difference. This study does not include least developed countries, where the difference in quality is likely to be even more pronounced. The ratio of 15 to 19 year old students who are functionally illiterate ranges between 43% and 82% in developing countries, which is in sharp contrast with the developed world (Hanushek and Woessmann, 2007). These findings of increasing cross-country inequalities clearly demonstrate the need for identifying alternative means (in addition to educational inputs) of improving the quality of education in developing countries. Setting a higher standard in CBEEE has the theoretical and intuitive appeal of being such a possible means.

3. Description of education in Bangladesh and the 'natural experiment'

3.1 Educational status in Bangladesh

Education has been one of the priority sectors in Bangladesh for the last two decades. Investment in the education sector consistently accounts for about 15% of annual government expenditure (Al-Samarrai, 2007) and this ratio was 14% in the budget for 2010-11. Through this investment, Bangladesh has achieved significant progress in increasing access to education. Net primary and secondary enrolment rates in 1990 were 56% and 28% respectively (Sen, 2005), which increased to 88% and 42% by 2009 (UNDP, 2010). In addition, adult literacy rates have increased from 34% to 55% during this period. There have also been remarkable progresses in reducing gender disparity in access to education. However, the country is still lagging behind in improving the quality of its education. Both primary and secondary education are marked by high student and teacher absenteeism, low educational attainment, frequent grade repetition and low competency achievements.

Using a specialized instrument of assessing basic competencies, Ahmed et al (2003) find that more than one-third of the students in Bangladesh remained non-literate or semi-literate after completing five years of formal primary schooling. A similar assessment of literacy and numeracy skills revealed that only 29% of the children aged between 11 and 12 years had the basic competencies associated with that level of education (Chowdhury et al, 2003). The scenario is not very different at secondary level (6^{th} to 10^{th} grade). In a subject-wise test of competency, only 30%, 27%, 16%, and 43% of 10^{th} grade students were able to correctly answer half of the questions in Bangla, English, Mathematics and Everyday Science respectively. The proportions of secondary students failing to correctly answer at least one-third of the questions of the competency tests in Bangla, English and mathematics were 36%, 40% and 53% respectively.

Estimates of financial returns to schooling for Bangladeshi school graduates are consistent with the gloomy scenario of the quality of education and students' competencies. Using the mincerian approach, Asadullah (2006) finds an annual 7% labor market return to each additional year of schooling for wage employed people. This is quite low in comparison with the experiences of most of the developing countries, where the rate of return is around 10% on average (Hanushek and Woessmann, 2007). Considering the non-linearity in returns, the estimated annual returns to education in Bangladesh are found to be 4.1%, 4.0% and 12.8% for primary, secondary and higher education respectively. This is consistent with the low cognitive achievements by students at primary and secondary levels. This could also be because the additional years of education at primary and secondary level are a weak signal of the ability of students. A different set of estimates by Shafiq (2007) finds somewhat higher rates of return at each level of education for full-time male workers engaged in wage or salaried employment.

Overall, it is acknowledged by the policy makers in Bangladesh that the quality of education needs to be considerably improved. A number of reforms have also been suggested in the National Education Commission of 2003, which are at various stages of implementation.

3.2 The Secondary School Certificate (SSC) Examination[5]

The schooling system in Bangladesh comprises of five years of primary schooling (Class 1 – 5) followed by three years of junior secondary school (Class 6 – 8), two years of secondary school (Class 9 and 10), two years of higher secondary school, and between three and four years of university education. The first curriculum-based external exit examination takes place after 10 years of schooling. Six autonomous education boards are responsible for conducting this nationwide exam. Schools are assigned to a particular board based on administrative divisions. After passing the examination, students receive an official certificate called the Secondary School Certificate (SSC). Below SSC level, there is no official credential issued by any external authority. Students can obtain certificates of 7th or 8th grade completion from the schools they attended.

To be able to sit for the SSC exam, examinees have to register with the education board at least two years prior to the year of examination (i.e. at the beginning of 9th grade). Once registered, a student can sit for the examination up to three times. After this, unsuccessful candidates can re-register five years after their last registration. After completion of the SSC exam, graduates must get admission to a different institute (colleges offering higher secondary education) in order to continue studying for the higher secondary certificate (HSC). This exam is also administered by the same education boards.

In the years relevant to the SSC – grades 9 and 10 – students study 10 courses, seven of which are predetermined and 3 of which are chosen from different sets of courses. In order to be awarded an SSC, a student must sit for exams on all the 10 courses and achieve at least 33% of the full marks in each course. Those who score at least 750 out of the maximum score of 1,000 in the 10 courses combined, earn a 'distinction' (popularly called 'star marks'). The other grades are 'first division' (score 600-749), 'second division' (score 450-599) and 'third division' (330-449).

3.3 The natural experiment: Multiple Choice Question (MCQ) with a Question Bank

Until 1992, students were evaluated on the basis of their descriptive answers to SSC exam scripts. There were concerns regarding the comparability of the evaluation of narrative answers by individual examiners. Nearly one million students taking the exam each year made it even more complicated to ensure fair assessments. To reduce examiner bias and to make the grading system more objective, Multiple Choice Questions (MCQ) method was introduced in 1992. In this new system, the evaluation weights were equally distributed between MCQ and descriptive answers for all the courses (except for mathematics, which followed the earlier structure). The structure of the descriptive answers remained the same, but this section was given half the weight within the overall evaluation. Pass marks in the new exam structure followed the previous cut-offs, but students were required to obtain at least 33% of total marks in MCQ and narrative answers combined for each course.

[5] There have been a few more recent changes in education structure. The following description, however, is valid for the period between 1985 and 1999, which is relevant to the time-span for this paper. The major changes since then have been the introduction of letter-marking grades in 2000 and an additional exit exam at junior secondary (after 8th grade) from 2010.

To help students prepare for the newly-structured MCQ exam as well as to guide teachers in preparing their students, a 'question bank' of 500 MCQs for each course was published by the education board. All six education boards used the same 'question banks'. Though the question banks were originally meant to provide only a guideline, examiners utilized the same 'question banks' to prepare exam papers: these were identical even in the ordering of the four answer choices to each question. Therefore, a student could pass the examination by studying the 500 questions for the two years before the SSC exam. This reduced the incentive for students to study the components of cognitive learning in order to pass their exams. This provided a scope for many students to pass the exam and obtain SSC certificates without achieving any additional cognitive abilities. Unsurprisingly, this caused the rate of passing the SSC examination to increase remarkably (Figure 1). The numbers in this graph have been compiled from officially published exam results for all the examinees in each exam year. The question bank period in the graph is marked by both a very high average pass rate and a surge in the number of examinees. The mean pass rate during the four years of question banks was 67 percent, which was much higher compared to mean pass rates of 49 percent during the four years prior to and following the question bank period. The pass rate in 1991, the year before the start of question bank, was also as high as the average pass rate of the next four years. Because of the changed exam structure, examinees of 1991 were not allowed to take the exam in the following year and required re-registration. There is unofficial information that examiners were guided to 'go easy' in grading the exam papers so that 'unlucky students' did not have to lose two additional years before taking the exam again. A second possible explanation of the high pass rate in 1991 is that the examinees studied harder to avoid wasting additional years.

Another important change took place during this experimental period, which is of particular relevance for interpreting the results of this paper. The switch to objective grading came with a change in anonymity of the examinees to the examiners. Prior to the 1992 exam year, the answer scripts contained the names of the examinees and the schools they attended. Since 1992, this has been made anonymous to reduce possible bias in grading or corruption by the examiners. The implications of this change will be discussed in Section 6.

As yet, there has not been any study that objectively verifies the effects of this weak exam standard on the actual cognitive abilities of graduates. Although no official statistics is available on this issue, a large share of the students was reported to have passed the SSC exam during that period relying on the MCQ part of the exam despite doing badly on the narrative sections. Realizing the effects of question banks on the quality of secondary education, the education boards declared several changes in 1994 that were to be made effective from 1996. The first was the abolishment of question banks for exam preparation. In addition, students were required to obtain at least 33% in both MCQ *and* narrative answers to pass the SSC. These changes led to a dramatic fall in the SSC pass rate (Figure 1). The number of students appearing in the examination also returned to the original trend after a sharp decline in 1996.

These changes in the examination structure provides a "natural experiment", where a good share of the students who acquired the certificate during 1992-1995 would have otherwise failed to do so or would

have had to study harder to gain the required competencies necessary to pass the exam. This paper exploits this variation to assess the labor market effects of an easier exam.

4. Data and methodology

Two different datasets have been used in this paper. In both datasets, the age of the individuals has been used as the exogenous determinant of their exposure to the 'question bank' scheme. The primary set of data for this paper comes from the national Household Income and Expenditure Survey of 2005 (HIES-2005) conducted by the Bangladesh Bureau of Statistics. This data has been used to assess the effects of the experiment on participation in salaried employment. This dataset contains a very small sample to evaluate any effect on wage rates. Employee records of a large non-governmental organization in Bangladesh have been used to assess the effects on salary and salary progression, which complements the findings from HIES-2005.

There are three major challenges in using age as the exogenous determinant of exposure to the experimental exam structure. Since there is no official record of age or birth registration in Bangladesh[6], the surveys collect age of household members by recalling their year of birth using event calendars.[7] This often creates measurement errors and, in fact, we observe a tendency of reporting rounded figures for age (i.e. 25 years, 30 years, 35 years, and so on) in the data. Secondly, according to the education system of Bangladesh, an individual should get enrolled in grade 1 at the age of 6 and sit for SSC at the age of 16. However, a number of surveys have shown that a large proportion of the students get enrolled for the first time at the age of 7, and sit for their SSC examination between the ages of 16 and 18 (Nath et al, 2008). Thirdly, the students who were 16 in 1991 or 1992 could also decide to sit for SSC exam during the 'question bank' period to take advantage of the less difficult exams by re-registering.

However, after learning that the grading system was going to change again, those students who were 16 in 1996 could not decide to sit for the examination in question bank period since they have to register at least two years ahead of the exam. Therefore, people aged 26 to 31 in 2005 can be considered to have been exposed to the question bank (Figure 2). Individuals aged 30 and 31 in 2005 have also been included in the exposed cohort since a) many students take the examination at the age of 17 and 18 and b) they could decide to delay sitting for the examination by two years.

There is an additional challenge in using the second dataset of employee records, which is about a graduate's 'real' versus 'certificate' age. Since age limits are often used by the government and private sector in Bangladesh as one of their recruitment criteria, there is a widespread tendency of under-reporting age. Employers use the date of birth recorded in each individual's registration with the education board, which takes place in the beginning of grade 9. Students, therefore, quite often record a date of birth that makes them six months to two years younger in the records. Sometimes, they also do it simply due to report a convenient date of birth. For example, according to the employee records, over

[6] Birth registration has been introduced in Bangladesh in early 2000, and this is yet to be made universal in the country. Moreover, national identity card has been introduced in 2008.

[7] Event calendars use major political and natural events, for example, independence, election year, or major flood.

6% of their 65,000 employees were born on the 1st January of their year of birth. This phenomenon of distinguishing between 'certificate' and 'real' date of birth is commonplace in Bangladesh, and introduces additional errors in the exposure measure in employer records since the record contains 'certificate' age. According to a national survey of adolescents and youth in Bangladesh, the majority (72%) of students who completed at least SSC, did it at the 'certificate age' of 15-17.[8] The exposure measure in the employee record, therefore, takes account of this by reducing the age for exposure by one year.

Analysis of HIES-2005 data includes the individuals who were aged between 23 and 37 at the time of the survey and had grade 9 or SSC as their highest educational achievement. Among them, the individuals aged between 26 and 31 years have been defined as the exposed cohort, and individuals aged 32 to 37 years and 23 to 25 years constitute the comparison group defined as 'older cohort' and 'younger cohort' respectively (Figure 1). Individuals younger than 23 (in 2005) have not been included as many of them were still studying. Nath et al (2008) have found that a number of SSC graduates continue their education for several years even though they keep failing the Higher Secondary Examinations (HSC). The younger cohort in HIES data, therefore, comprises of 3-years age bracket instead of 6 years. However, the analysis of employer records makes the cohorts by 6-years age brackets since the data is relatively more recent (collected in June, 2008).

In most parts of the analysis in this paper, the sample is restricted to individuals who have completed grade 9 or SSC. This is to make a neater assessment of the question bank experiment. Students who have either completed HSC and above or did not study as far as 9th grade are unlikely to be directly affected by the experiment regarding their performance in labor market. Using this sample, the basic reduced form specification is essentially a difference-in-difference estimate.

$$y_i = \alpha + \beta_1 SSC_i + \beta_2 SSC_i * EXPOSED_i + \beta_3 EXPOSED_i + \gamma_k X_{ki} + u_i \qquad (1)$$

where y_i denotes participation in salaried work, equal to 1 if observation *i* is involved in salaried employment and 0 otherwise. Salaried employment in Bangladesh is important both for its financial return and for the social prestige associated with such jobs. Limitation of salaried employment as the main outcome variable and results of alternative dependent variables will be discussed in the findings section. SSC_i is a dummy for whether individual *i* has passed SSC, which takes the value of 1 if the person has completed SSC (but not HSC) and 0 if completed 9th grade. Therefore, β_1 measures the return to SSC over grade 9 graduates for the older and younger cohorts. $EXPOSED_i$ is a dummy variable, equal to 1 if an individual is between 26 and 31 years old as of 2005, and 0 otherwise. X_i is the set of their individual characteristics and regional dummies. The key parameter of interest is β_2, which should be zero if passing SSC examination during the question bank has no effect on labor market performance. $\beta_1+\beta_2$ is the returns to holding an SSC certificate (in terms of the likelihood of having salaried employment) for the exposed cohort.

[8] See RED (2006) for details of the dataset

Given that the exposure by age is expected to contain measurement errors, this could lead to attenuation bias and erroneously give an estimate of the parameter closer to zero. Despite this problem, using age to estimate exposure has an important advantage over the actual year for sitting for the SSC exam. Students might decide in which year to take the exam once they are in grade 9, which could introduce selection bias in our model (e.g. relatively weak students deciding to take the advantage of question bank). However, age should remain exogenous to the exposure even though some of the students did not actually take the exam during this period. Using age allows controlling for omitted ability since there is no reason to believe that these underlying abilities are associated with age.

Table 1 shows descriptive statistics of the two datasets, both of which include only those who have completed grade 9 or SSC. HIES-2005 data (in Panel A) shows that the rate of passing SSC examination is higher for the exposed cohort than the other two cohorts for both male and female students. This gives some confidence in the exposure measure although the difference between older and exposed cohorts is marginal for male students (57.4% vs. 60.1%). Labor force participation of males is around 90%, which differs greatly from female (only 10%). For males, engagement in salaried employment is lower (28.6%) for the exposed cohort in comparison with both the older (35.1%) and younger (31.3%) cohorts.

From the employee records data in Panel B, about 80% of the employees are male. Average job tenure of the employees is obviously correlated with their age. The proportion of employees with an SSC certificate is higher for the exposed cohort compared to the older cohort, which is expected given the differential pass rate between the two groups. However, there is no difference between exposed and younger cohorts in this ratio, which indicate that the organization is recruiting relatively greater proportion of SSC graduates from younger cohort. More relevant comparison is the difference between the average salary of 9^{th} and SSC graduates across the different cohorts. While the average salaries of SSC graduates are higher than those of 9^{th} graders for both the older and younger cohorts, there is no such difference for exposed cohort.

5. Main results

Since information about the amount of earnings is available for only a small number of observations in the national data, participation in salaried employment has been used as the key outcome indicator. It has already been noted that salaried employment is the most desired and rewarding form of employment in Bangladesh. Osmani et al (2003) found that an individual in Bangladesh can achieve the highest amount of economic returns by making an occupational shift to non-farm salaried employment from other forms of employment. Sen et al (2007) illustrate similar scenario where they find that an occupational shift to the non-farm sector is one of the major drivers of economic mobility for the poor. They also reveal that this shift is more likely to happen through salaried employment after achieving higher education. HIES-2005 data also shows a strong correlation between salaried employment and education. The extent of salaried employment increases from 9% for those who have completed grade 9

to 61% for those with a master's degree.[9] An anthropological work on the youth who completed between 8 and 12 years of education reveals an extremely high degree of social status associated with salaried employment, and finds that many youth are willing to remain unemployed for several years trying to get a salaried job before taking any non-salaried work (Ahsan, 2008).

Using engagement in salaried employment as the dependent variable, regression results of equation 1 for male and female samples are presented in Tables 2 and 3 respectively. Completion of SSC is positively associated with the probability of being employed in a salaried job for both males and females. This shows a significant return to the SSC (in terms of higher probability of salaried employment), which could be either because of greater human accumulation by SSC graduates over 9th graders or signalling of their higher abilities. It is important to note here that the estimated returns (12 percentage points for male and 7 percentage points for female[10]) are the returns to completion of SSC over 9th graders. These are not estimates of the return to 10 years of schooling. Although these appear to be very high estimates of the return to SSC certificate over 9th graders, it is not surprising given that there is no official credential below SSC and it is very easy to produce a fake certificate of completing grade 8 from many schools.

The coefficients for *SSC*EXPOSED*, which is the key parameter of interest for this paper, are negative and significant at less than 10 percent level. This reveals that the returns to SSC certificate, in terms of accessing salaried employment, are significantly lower for those who were exposed to the question bank method of examination. Point estimate of the effect is higher for the male sample (11 percentage points) than the female sample (5 percentage points) although this difference is not statistically significant.[11] Among the exposed cohort, the SSC graduates are not doing any better than those who completed 9th grade ($\beta_1+\beta_2$ is not statistically different from zero). These estimates are consistent across different specifications of linear probability models and logit model.[12]

Table 4 makes the same comparisons relative to the older and younger cohorts separately to investigate whether the average effects are particularly driven by the difference of the exposed cohort from either of the two non-exposed cohorts. We find that point estimates of the effects relative to the older cohort are significant for both male (13 percentage points) and female (7 percentage points) graduates. Estimated effects relative to the younger cohort are also negative (7 percentage points for male and 4

[9]For the full sample, the extents of salaried employment for different education level are 9% (with 9th grade), 18% (SSC), 27% (HSC), 46% (graduates) and 61% (masters).

[10] The difference of these point estimates is not significant (p-value 0.145).

[11] Difference of the coefficients of the two regressions was tested using Chow test.

[12] Some of the control variables in the third and sixth regression (Rural/urban location, marital status, and administration division dummies) are not 'ideal' control variables since these variables themselves are likely to be affected by the key regressor of interest, i.e. completion of SSC. For example, present location could be influenced by completion of SSC if they migrate to urban areas seeking better employment or educational opportunities. Ideally, we would require information on these variables at the time of taking the SSC exam, which may have determined their probability of passing the exam as well as securing salaried employment. However, this information is not available in this cross-sectional data. Therefore, these variables have been excluded in subsequent analysis.

percentage points for female sample) although not statistically significant. The SSC graduates of the exposed cohort appear to be performing worse than both the older and younger cohorts. The returns for the younger cohort are expected to rise, as 2005 is too early to find their real likelihood of salaried employment. As noted earlier, such SSC graduates try out different alternatives (including higher secondary study before dropping out) before settling into any employment. An important finding for this paper is the consistency in zero payoffs, in terms of the likelihood of having formal employment, to SSC certificate for the exposed cohort as shown by $\beta_1+\beta_2$ being not different from zero in all four regressions.

6. Mechanisms underlying the effect on labor market performance

In terms of linking the effect of question bank policy on labor market performance, there are four possible explanations. Two of these mechanisms, lower human capital accumulation through reduced incentive to learning and noisy signal of abilities, have already been discussed in Section 2. A third possible explanation arises from the general equilibrium effects of increased supply of SSC graduates, which could reduce the probability of getting formal employment of the exposed graduates when they entered labor market for the first time.[13] In fact, as Figure 1 demonstrates, there were increases not only in pass rates but also in the number of examinees during the experimental period. Therefore, there is possibility of them facing a relatively 'slack' labor market when they started looking for jobs. Although we look at the labor market outcomes almost 10 years after their graduation, there are evidences showing that initial labor market conditions can have longer term influences (Altonji, 2005; Brunner and Kuhn, 2010).

The fourth explanation relies on the change in anonymity between regimes. Prior to the change in exam standards from subjective to objective assessment in 1992, the examiners knew the name of examinees and the schools that they attended. If the examiners are biased in favor of good schools, weaker students from better schools could receive preferential treatment in grading. Moreover, students with greater family connections could potentially track their exam papers and influence the outcome of their exams. This would create a positive correlation between passing the exam and getting a good job. With the introduction of anonymity, this link is broken.

This section examines these alternative interpretations of the main results, and focuses on the male sample, for which the previous effects could be precisely estimated. The same analyses were conducted for female sample, but not presented in this paper. The directions of the estimates were found to be similar between the male and female samples.

6.1 Alternative definition of exposure

We can use different age cut-offs for exposure to reflect on the four interpretations. Among the exposed cohorts, students who took the exam in later years were more aware about the 'usefulness' of

[13] Duflo (2001) discusses the general equilibrium effects of greater supply of educated laborers in the context of Indonesian labor market.

question banks. Even though all the examinees between 1992 and 1995 took the SSC examination under the question bank regime, by 1994 it was obvious that question banks were the sole source of questions for the MCQ part of the exam. There was also an official circular from the Education Boards of this fact, which was not available in the earlier two years. Therefore, the late exposed group had to study the least amount to pass the exam while both groups earned the same signal. In order to test these two explanations, the exposed group has been sub-divided into early exposed (26-28 years old) and late exposed (29-31 years old) categories. Table 6 presents the estimates of the following equation using HIES-2005 data.

$$y_i = \alpha + \beta_1 SSC_i + \beta_2 SSC_i * EXPOSED\_EARLY_i + \beta_3 EXPOSED\_EARLY_i + \beta_4 SSC_i * EXPOSED\_LATE_i + \beta_5 EXPOSED\_LATE_i + \gamma_k X_{ki} + u_i \quad (2)$$

In this equation, y_i is a dummy for salaried employment. β_2 and β_4 estimate the effects of the question bank experiment on early exposed and late exposed groups respectively. The effects appear to be much stronger and more significant for the late exposed group (Table 5). The differences between β_2 and β_4 in regression 1-6 are significant at 12-13 percent level. The early and late exposed cohorts obtained similar signal by taking exam within question bank structure. They also took the exam under the same anonymity system. The results of Table 5, therefore, are relatively more consistent with both human capital and general equilibrium hypotheses compared to signalling or social connection interpretations. The arguments have been elaborated below.

If greater awareness about the question bank regime is the major driver of the differential effects, the results can be interpreted as supporting the human capital hypothesis. Since both the early and late exposed group had the same signal but different level of incentives to learn, the difference in labor market performance could be due to lower amount of human capital accumulation. General equilibrium effects can also explain the results equally well. The effect of the experiment may have been stronger for the late exposed cohort since both the pass rates and the number of examinees were higher in the later years of the experiment. Cumulative oversupply of SSC graduates may have also created a stronger effect on the late exposed cohort.

The effects on the late exposed cohort relative to their younger cohort are of particular relevance to the social connection explanation. Although the estimates are not significant at conventional levels, the high estimates (14 percentage points significant at 12 percent level) caste doubt over the social connection hypothesis. The change to anonymity in exam papers occurred in 1992, and the younger cohort took their exams under the same anonymous condition. If social connection is the primary driver of the results, we should not observe such large estimates of the exam experiment effect relative to the younger cohort.

There are possible other explanation of these differential effects on the early and late exposed groups. It can be argued that the employers use the specific years of passing the SSC, in addition to using the question bank period, for sorting candidates. However, it is difficult to imagine that the employers keep track of each year's exam standard while SSC graduates comprise of a relatively small share in the total

pool of people with salaried employment. Another possible reason of the different results could be disparity in measurement errors of exposure between the two groups. We have used age as the exogenous proxy for actual exposure. If there are higher amount of measurement errors among the early exposed group than the late exposed, then they will have higher amount of attenuation bias (i.e. the coefficient being biased towards zero). As it has described, there is a strong tendency in reporting rounded figures for age in the sample. Since the late exposed group contains those individuals who are 30 years old (as of 2005), this group (rather than the early exposed group) is likely to have higher measurement errors. The possibility of different levels of attenuation bias, therefore, is unlikely to explain the results.

6.2 Effects on HSC graduates

The base analysis of this paper uses difference-in-difference estimates between 9th graders and SSC graduates across different cohorts. We excluded those who completed HSC (higher secondary level - the next qualification after SSC) and above with an underlying assumption that there was no change in the ability of this pool of students since there was no change in the HSC exam structure.

According to the screening argument, however, there should be a change in the return to HSC graduates for the exposed cohort. As additional low ability students acquire SSC certificates due to easier exams, this should make employers readjust the salaries for SSC graduates downward. This, in turn, will induce the relatively higher ability students to exert additional effort in achieving HSC, who would otherwise have been satisfied with only SSC certificates (Bedard, 2001). Moreover, obtaining the SSC certificate may have had motivational effects on the students to continue their education. It is possible that such motivational effects are stronger for relatively better students among those who would have failed to obtain the SSC under conventional standards, but passed the exam due to its lower standard. Therefore, it is possible that the final pool of both SSC and HSC graduates in exposed cohort contains individuals with relatively lower inherent abilities compared to the other two cohorts. In such a scenario, we should observe similar negative effects on the HSC graduates from the exposed cohorts.

Similar prediction can be made for social connection and general equilibrium effects. Since 1994, anonymity of examinees has been introduced at the HSC exam as well. If our main results are driven by social connections, we should observe lower return for HSC graduates relative to 9th graders. Greater competition from SSC graduates for jobs can also reduce the likelihood of getting salaried employment by the HSC graduates.

On the other hand, if competencies achieved during SSC study are associated with later performance in the HSC exam, some of the higher ability students will fail to acquire HSC. According to the national aggregate figures, annual average pass rate in HSC (which happens two years following the SSC) during 1994-1997 was 37% compared to 46% in the previous 9 years and 52% in the subsequent 10 years. It is, therefore, quite likely that a good portion of the SSC graduates taking advantage of the lower exam standards may have failed to make it beyond SSC. These national statistics are not enough to make a

clear case of whether there were changes in inherent abilities among the different pools of SSC graduates.

Moreover, any other educational policy that is correlated with the exposed group can also influence our main results. If the age cohorts are orthogonal to ability distributions and other important educational policies, there should be no effects of exposure to question banks on the labor market performance of HSC graduates. Table 6 presents double difference estimates of the likelihood of salaried employment by HSC graduates (the same as equation 1, but replacing SSC_i with HSC_i).

$$y_i = \alpha + \beta_1 HSC_i + \beta_2 HSC_i * EXPOSED_i + \beta_3 EXPOSED_i + \gamma_k X_{ki} + u_i \qquad (3)$$

Here HSC_i is a dummy variable, equal to 1 if individual i has completed HSC, and equal to 0 for those who completed 9th grade. We do not observe any effect on the students from exposed cohort who have completed HSC. The coefficients for $HSC_i*EXPOSED_i$ are not significantly different from zero. Therefore, HSC graduates from exposed cohort are likely to obtain equal amount of return to HSC, in terms of salaried employment, compared to the other cohorts. These results caste serious doubts on the social connection story of the correlation between labor market performance and passing SSC during the exposed period. If social connection hypothesis is the key underlying mechanism, this would indicate that social connection is used by SSC graduates and not by the HSC graduates in securing formal employment. It is difficult to argue in favor of such a case.

While the same results are also contrary to the predictions of signalling and general equilibrium hypotheses, both the arguments rely on the spillover effects of the exam experiment on HSC. If the spillover effects are weak, we may not be able to capture that from our data. Nonetheless, the lack of any impact on the HSC graduates builds confidence on the causal effect of the exam experiment on the labor market performance of the SSC graduates.

6.3 Sectors of employment

Distribution of employment in different sectors can potentially shed light on the relative importance of the four possible mechanisms. About 40% of the formal sector employment is concentrated in the public sector and the rest 60% in private sector. Between public and private sectors, there are reasons to expect that social network could be relatively more important in public sector due to corruption in recruitment. There is widespread inefficiency in public sector of Bangladesh due to lack (or complete absence) of promoting and firing based on performance than connections (Mukharjee et al, 2001). On the other hand, private sector employers are more likely to be sensitive to human capital and productivity of their employees. Therefore, we should observe the effects of exam experiment to be concentrated more in public sector if social network or signalling are relatively more forceful mechanisms, and reduced human capital accumulation should reveal greater effects in the private sector.

Table 7 gives the estimates of equation 1, where the dependent variables are engagement in salaried employment in public and private sector (regression 1 and 2 respectively), non-farm self employment (regression 3) and informal day labor (regression 4). We find that completion of SSC is positively associated with salaried employment in both government and private sector for the non-exposed sample, albeit the point estimate for private sector is significant at less than 11 percent level. More importantly, the exam experiment reduced employment for SSC graduates in private sector and there is no significant effect in government sector employment. This indicates that social connection and signalling are not necessarily the main drivers of the results. Regression 3 shows that greater proportion of the SSC graduates of the exposed cohort took up employment in non-farm businesses. Finally, day labor is a sector that requires the least amount of human capital and we do not observe any significant effect on this form of employment.

The results could also be explained through general equilibrium effect of greater competition for employment in private sector due to increased supply of SSC graduates. However, we should observe similar general equilibrium effects in the government sector as well. In fact, growth in employment in public sector in Bangladesh has stagnated since 1992 (Mukherjee et al, 2001), and private sector has been the main provider of salaried employment in Bangladesh (Rahman, 2005). In this scenario, greater supply of graduates should reduce possibility of getting salaried employment in the government sector than in the private sector. Nonetheless, it is difficult to make accurate prediction of the general equilibrium effects.

7. Discussion on the robustness of the results

While the mechanisms of the effects are not conclusive, the main results of the negative effect of lowered exam standards on labor market performance are quite consistent. The possibility of omitted policy variables is a common concern in assessing the causal effects of a particular policy in quasi-experimental settings. It is essential to review whether our estimates of the effects of lower exam standard are picking up any other effects, which are correlated with the question bank experiment. Amin (2007) lists the major policies that took place in Bangladesh since independence. The major policy during the question bank period is the stipend programme for female students at secondary level. However, this national programme was introduced in 1994 and its influence on SSC pass rate could have happened only in 1999 or later. Moreover, this programme is for female and their labor force participation is very low for all three cohorts. In this section we discuss the robustness of the main results with additional data and analysis, and the limitations.

7.1 Effects on salary and salary progression

Our base analysis uses obtaining formal employment as the outcome variable. As discussed earlier, wage information from HIES-2005 could not be used because of the very small sample of individuals in the data who completed 9th grade or SSC and were earning a salary. Moreover, descriptive statistics of HIES-2005 sample revealed quite a few outliers, which could otherwise influence the results, especially given the small sample size. To complement the main findings, employee records of an NGO have been used, which contain information on salary and its progression. The employee records that have been

analyzed were being used by the organization to pay these employees, which avoids possible measurement errors in reporting. Moreover, employer records allow assessment of different outcomes (including initial salary, salary progression and current salary) to shed light on the relative importance of learning and sorting mechanisms.[14] Table 8 (regression 1, 2 and 4) presents estimates of the following of equation.

$$y_i = \alpha + \beta_1 SSC_i + \beta_2 SSC_i * EXPOSED_i + \beta_3 EXPOSED_i + \gamma_k X_{ki} + u_i \qquad (4)$$

This is the same difference-in-difference as equation 1, except the dependent variables are different. The dependent variables are the log of initial salary, salary growth and current salary. Since the organization started keeping digital records of employees in 2003, we do not have the data of initial salary for those employees who joined the organization prior to this. Since a neater comparison can be made amongst those who joined in 2003 or after, observations included in the first three regressions are restricted to those employees. The fourth regression includes all current employees who have completed either 9th grade or SSC.

In the first regression, the log of initial monthly salary is the dependent variable. Among non-exposed cohorts, those who have the SSC joined the organization at a 10% higher salary compared to those who completed grade 9. This is a relatively high return for achieving the certificate. For exposed cohorts, however, the premium for having an SSC is 11% lower than that of the other two cohorts. This evidence shows that having SSC certificate had no significant return in terms of starting salary for the exposed cohort working in the organization. Evidence from this data reveals that the lower exam standard has significantly reduced labor market gains for the SSC graduates, which is consistent with the findings from HIES-2005.

Results from the first regression do not explain whether the SSC graduates and 9th graders of the exposed cohort were being paid the same salary due to acquiring similar level of human capital from schools or because of the question bank itself being used as a signal by the employers. The lack of difference in initial salary between SSC and 9th graders of the exposed cohort could also be explained by the greater supply of SSC graduates during this period. Growth in salary can potentially differentiate between human capital and signalling mechanisms. Since the employers get to know the 'true' ability and productivity of employees, they adjust salary over time. If those employees with and without an SSC certificate had, on average, different levels of human capital, and the initial lack of difference was due to screening by the employers, SSC graduates will display a higher salary progression. On the other hand, if these two groups have the same level of human capital, there will be no such divergence in salary over time.

[14] There is a caveat in interpreting the result from this data. The employees of this organization do not necessarily represent the cohorts who are engaged in salaried employment since there could be selection biases by the organization or among employees interested to work for this organization.

In the second regression, the dependent variable is the difference between the log of current (as of June, 2008) and the log of initial salary, which measures growth in salary. For the non-exposed cohorts, we observe a divergence in salary between employees with and without SSC certificate. The SSC graduates of non-exposed cohorts have experienced around 5% higher growth over 5 years in their salary compared to those who had only completed 9th grade from the same cohort. However, there is no divergence visible for the exposed cohort. This indicates that the SSC graduates of exposed cohort did not acquire any human capital to attain any labor market gain. Interestingly, among those employees who completed grade 9, the exposed cohort had a higher salary progression than the other two cohorts. A plausible explanation of these results is that both the SSC and non-SSC groups of the exposed cohorts were being offered lower initial salaries, and both groups are catching up with the non-SSC group of other cohorts. If the SSC graduates of exposed cohort did not acquire any additional cognitive ability (unlike SSC graduates of other two cohorts), they are not likely to be doing any better than 9th graders of the same cohort.

Interpretation of regression 2 is based on the assumption that employers become aware about the actual productivity of the employees. It is difficult to make an assumption about the amount of time required by the employers to get full information about productivity. Therefore, regression 3 looks at heterogeneity in salary growth by job tenure.

$$y_i = \alpha + \beta_1 SSC_i + \beta_2 SSC_i * EXPOSED_i + \beta_3 EXPOSED_i + \beta_4 TENURE_i + \beta_5 TENURE_i * SSC_i + \beta_6 TENURE_i * EXPOSED_i + \beta_6 TENURE_i * SSC_i * EXPOSED_i + \gamma_k X_{ki} + u_i \quad (5)$$

Here $TENURE_i$ is the number of completed years that employee i is working in the same organization. While an additional year of job tenure is associated with around 9 percent higher growth in salary, none of the interaction terms of $TENURE$ is significant (regression 3 in Table 5). This indicates that the employer made the salary adjustments relatively fast.

The last regression (regression 4 in Table 8) reports results of equation 1 with log of current salary as the dependent variable. This regression includes employees who joined before 2003, majority of whom belong to the older cohort. Although the measure of return to SSC certificate becomes insignificant, the main result of lower returns for SSC graduates of the exposed cohort persists.

7.2 Linear returns to schooling

So far the analyses include the individuals who have completed 9th grade as the base category to estimate payoffs from the SSC certificate and excludes all other observations. Here we do an alternative estimate of linear returns to additional grades completed. The specification is

$$y_i = \alpha + \beta_0 EDU_i + \beta_1 SSC_i + \beta_2 SSC_i * EXPOSED_i + \beta_3 EXPOSED_i + \gamma_k X_{ki} + u_i \quad (6)$$

where y_i is whether individual i is engaged in salaried employment or not, and EDU_i is years of education completed. Therefore, β_0 is the returns to an additional year of schooling. β_1 and β_2 are the

deviation from linear returns for completing SSC for the non-exposed cohorts and the differential deviation for the exposed cohort. Table 9 (regression 1 – 3) shows the results of this specification using HIES-2005 data. Completing an additional year of education is associated with around a 3.5 percentage point increase in the probability of salaried employment. While the other cohorts do not show any significant deviation from this linear return for achieving SSC, there are significantly lower returns for the exposed cohort.

In regression 4 to 6, we add dummies for Grade 9 and HSC, and their interactions with exposed cohort. The results show that the exposed cohort is performing worse than the other two cohorts only if they completed SSC. The likelihoods of obtaining salaried employment by Grade 9 or HSC graduates are not different between the exposed and non-exposed cohorts. Since we observe the effect of exposure to the exam experiment only among the SSC graduates, this builds confidence in interpreting the results of poorer labor market performance as a causal effect of the policy.

7.3 Exam performance beyond pass-fail

We find that students of the exposed cohort are more likely to pass the SSC exam (Figure 1 and Table 1) and perform poorly in labor market, but we do not explore any intermediary outcome variable. There are two major intermediary outcomes, namely students' learning (which could be measured by tests of cognitive abilities) and their exam performance beyond pass-fail classification, in terms of division/grade achieved.[15] Although we do not have data on these outcomes, their implications on labor market performance deserve some reflections.

The division achieved in SSC is often used as an entry requirement and an explicit screening device by employers. It can be argued that while the experimental examination system may have made it easier to pass, it could also have made it more difficult to achieve better grades. In such a scenario, the exposed cohort is put into a disadvantaged position relative to the other cohorts, and the estimated effects may reflect this disadvantage rather than lower standard in passing. However, the reality is quite the opposite. The graduates in the exposed cohort had an advantage over the other two cohorts in terms of division because of the question banks. For example, 50% of those who passed SSC in 1994 got 1st division compared to 34% in 1997 and 13% in 1991. Only 1.4% of the students passed with 3rd division in 1994 compared to 46% in 1991.

Besides this advantage in grades, SSC graduates of the exposed cohort had more time to find salaried employment than their younger cohorts. As noted earlier, graduates do not necessarily enter the labor market immediately after passing the SSC exam. Since we are looking at employment status in 2005, the

[15] It could be argued that human capital accumulation or academic performance is determined by students' fixed effects and not affected by changes in exam standards. However, Stinebrickner and Stinebrickner (2008) find that academic performance is not predetermined by student characteristics, and they find influence of roommate characteristics influencing study time and exam performance. Though their result may not be generalizable, it is difficult to imagine that study effort and exam performance are fully predetermined.

exposed graduates had more time to find a job. Despite the grade and time advantage, they are performing worse than their younger cohorts.

7.4 Limitations

Overall, the main finding of a lower exam standard leading to worse labor market performance, in terms of the probability of attaining salaried employment and lower salary, by the graduates is robust for the male sample. The results of lower return to SSC are not as consistent for the female sample as it is for male. In terms of the underlying mechanism(s), both lower human capital accumulation and general equilibrium effects due to greater supply of SSC graduates seem plausible, which could not be disentangled.

A second limitation of this paper is not exploring the intermediary outcomes (e.g. cognitive ability or learning) due to lack of data. Moreover, we primarily focus on engagement in formal employment as the measure of labor market performance, except for the analysis of employee records data. Although this is a useful indicator of labor market performance, effects on actual earning need to be assessed for more efficient policy suggestion.

Finally, we could not assess the distributional aspects of a lower exam standard. Lower standards are sometimes rationalized by policy makers on the grounds that higher standards can cause the weaker students, especially the ones from poorer socio-economic backgrounds with limited family supports for their education, to dropout. We could not measure heterogeneity of the effects across different socio-economic groups because of lack of adequate data. Further study can potentially exploit this experiment to look into the distributional issue by collecting richer data.

8. Conclusion

Using a natural experiment in the structure of the secondary education final exam in Bangladesh, this paper shows that lower academic standards have a negative effect on the labor market performance of the graduates. We find that acquiring a SSC certificate under conventional standards is associated with 12-13 percentage points increase in the likelihood of getting salaried employment for male and about 7 percentage points for female. However, lower standards during the experimental period eliminated these labor market gains of successfully passing the exit exam. We also find that greater supply of SSC graduates and lower human capital accumulation during lower standard period are potential drivers of their poorer labor market performance.

In recent years, education has become a major area of investment in developing countries. Similar emphasis on education has enabled Bangladesh to achieve substantial increases in access to education. However, the quality of both primary and secondary education has repeatedly been raised as a cause of concern. Given the size of investments in the education sector, these concerns are of utmost importance for policy makers. Discussions on improving the quality of education in developing countries are primarily concerned with inputs and delivery mechanisms. While standards in curriculum based exit examination are widely discussed in developed countries, this is yet to receive any noteworthy academic

attention for developing countries. Consequently, the policies on academic standards in developing countries often suffer from decisions based on hunches rather than evidence. Such decisions are also prone to political considerations. For example, a high pass rate in public examination is considered as a success by the education ministry. In recent years, the government of Bangladesh has been considering a number of drastic measures to reform the structure of education. Maintaining a high standard in exit exams should be given priority in this attempt.

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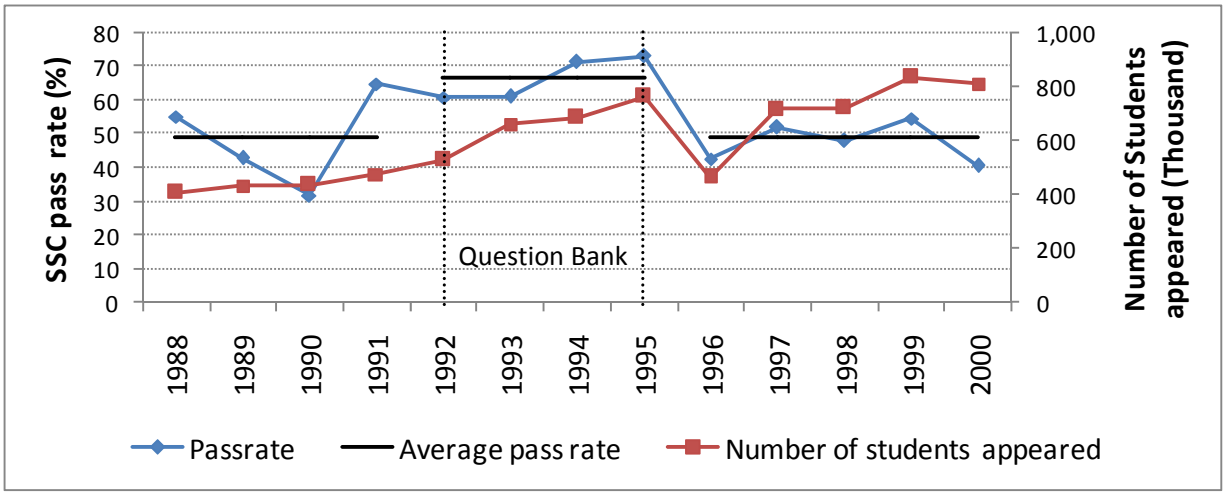
Figure 1. Trend in pass rate and number of student appeared in SSC exam



Source: Compiled from annual reports of BANBIES

Figure 2. Measure of Exposure

	← Older		Exposed						Younger →		
Age in 2005	33	32	31	30	29	28	27	26	25	24	23
Year of SSC at age 16	’88	’89	’90	’91	’92	’93	’94	’95	’96	’97	’98
					Question Bank						

Table 1. Descriptive statistics

	Older cohort	Exposed cohort	Younger cohort
	Mean (SD)	Mean (SD)	Mean (SD)
Panel A. National Household Income Expenditure Survey (HIES-2005)			
Male (Male=1, Female=0)	0.545 (0.498)	0.465 (0.499)	0.492 (0.500)
Passed SSC exam (among male)	0.574 (0.495)	0.601 (0.491)	0.488 (0.501)
Age in years (in 2005 for male)	34.41 (1.612)	28.25 (1.691)	24.25 (0.824)
Labor force participant (male)	0.951 (0.217)	0.929 (0.258)	0.858 (0.350)
Salaried employment (male)	0.351 (0.478)	0.286 (0.452)	0.313 (0.465)
Day laborer (male)	0.102 (0.303)	0.091 (0.288)	0.073 (0.261)
Non-agri self employment (male)	0.308 (0.463)	0.321 (0.468)	0.252 (0.435)
Passed SSC exam (among female)	0.486 (0.501)	0.610 (0.488)	0.492 (0.501)
Age in years (in 2005 for female)	34.20 (1.677)	28.13 (1.654)	24.11 (0.860)
Labor force participant (female)	0.122 (0.327)	0.076 (0.266)	0.055 (0.229)
Salaried employment (female)	0.063 (0.243)	0.051 (0.220)	0.055 (0.229)
Day laborer (female)	0.008 (0.088)	0.008 (0.092)	0.008 (0.089)
Non-agri self employment (female)	0.027 (0.164)	0.025 (0.158)	0.016 (0.125)
Number of observations	560	662	500
Panel B. Employer's records of 2008			
Male	0.831 (0.376)	0.790 (0.408)	0.776 (0.419)
Years worked in the organization	9.254 (4.827)	6.662 (3.278)	3.784 (2.085)
Passed SSC exam	0.415 (0.494)	0.621 (0.486)	0.632 (0.484)
Age in years (2008)	36.76 (1.751)	30.52 (1.572)	24.55 (1.583)
Monthly salary	6,864 (1740)	5,945 (1612)	4,552 (1022)
Monthly salary of 9^{th} graders	6,572 (1546)	5,953 (1519)	4,177 (860)
Monthly salary of SSC graduates	7,390 (1948)	5,939 (1684)	4,787 (1051)
Number of observations	248	219	125

Table 2. Difference-in-difference estimate of participation in salaried employment (male)

	Linear probability model			Marginal effect of logit		
	(1)	(2)	(3)	(4)	(5)	(6)
SSC (1=Yes, 0=Grade 9)	0.128*** (0.0397)	0.126*** (0.0398)	0.119*** (0.0396)	0.124*** (0.0383)	0.122*** (0.0384)	0.118*** (0.0389)
SSC X Exposed	**-0.112* (0.0658)**	**-0.110* (0.0661)**	**-0.115* (0.0654)**	**-0.103* (0.0602)**	**-0.101* (0.0604)**	**-0.106* (0.0602)**
Exposed	0.011 (0.0490)	0.002 (0.0606)	0.004 (0.0595)	0.011 (0.0534)	0.003 (0.0636)	0.004 (0.0635)
Age (in years)	-	0.032 (0.0852)	0.008 (0.0841)		0.031 (0.0839)	0.008 (0.0849)
Age-square	-	-0.001 (0.0014)	-0.000 (0.0014)		-0.0004 (0.0014)	-0.0001 (0.0014)
Rural (1=Yes, 0=No)	-	-	-0.057* (0.0320)			-0.059* (0.0329)
Married (1=Yes, 0=No)	-	-	0.049 (0.0383)			0.051 (0.0391)
Religion (1=Islam, 0=Else)	-	-	0.009 (0.0413)			0.011 (0.0424)
Division dummies	No	No	Yes	No	No	Yes
Constant	0.266*** (0.0277)	-0.219 (1.2231)	0.173 (1.2103)	-	-	-
Observations	859	859	859	859	859	859
R-squared	0.015	0.015	0.052	0.012	0.012	0.043
F-statistics for $\beta_1 + \beta_2 = 0$ (p-value)	0.09 (0.768)	0.09 (0.879)	0.01 (0.933)	0.09 (0.879)	0.09 (0.879)	0.01 (0.904)

Note: Robust standard error in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%
Dependent variable is whether engaged in salaried employment. Estimates use HIES-2005 data.

Table 3. Difference-in-difference estimate of participation in salaried employment (female)

	Linear probability model			Marginal effect of logit		
	(1)	(2)	(3)	(4)	(5)	(6)
SSC (1=Yes, 0=Grade 9)	0.065*** (0.0210)	0.065*** (0.0210)	0.067*** (0.0212)	0.062*** (0.0202)	0.061*** (0.0201)	0.060*** (0.0200)
SSC X Exposed	**-0.053* (0.0314)**	**-0.053* (0.0316)**	**-0.055* (0.0311)**	**-0.041* (0.0213)**	**-0.041* (0.0214)**	**-0.040* (0.0200)**
Exposed	0.017 (0.0201)	0.028 (0.0247)	0.033 (0.0242)	0.025 (0.0294)	0.036 (0.0331)	0.040 (0.0327)
Age (in years)	-	-0.026 (0.0439)	-0.024 (0.0430)	-	-0.020 (0.0353)	-0.022 (0.0328)
Age-square	-	0.000 (0.0007)	0.000 (0.0007)	-	0.000 (0.0005)	0.000 0.0006
Rural (1=Yes, 0=No)	-	-	-0.010 (0.0160)	-	-	-0.011 (0.0142)
Married (1=Yes, 0=No)	-	-	-0.146** (0.0602)	-	-	-0.156*** (0.0707)
Religion (1=Islam, 0=Else)	-	-	-0.029 (0.0230)	-	-	-0.032* (0.0231)
Division dummies	No	No	Yes	No	No	Yes
Constant	0.027*** (0.0101)	0.381 (0.6264)	0.502 (0.6155)	-	-	-
Observations	863	863	863	863	863	822
(Pseudo) R-squared	0.013	0.014	0.040	0.029	0.031	0.076
F-statistics for $\beta_1 + \beta_2 = 0$ (p-value)	0.27 (0.606)	0.27 (0.604)	0.24 (0.624)	0.25 (0.615)	0.25 (0.614)	0.16 (0.686)

Note: Robust standard error in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%
Dependent variable is whether engaged in salaried employment. Estimates use HIES-2005 data.

Table 4. Effect on participation in salaried employment relative to older and younger cohorts

	Relative to older cohort		Relative to younger cohort	
	(1)	(2)	(3)	(4)
Panel A: Male sample				
SSC (1=Yes, 0=Grade 9)	0.156***	0.152***	0.088	0.088
	(0.0538)	(0.0540)	(0.0592)	(0.0594)
SSC X Exposed	**-0.140***	**-0.133***	**-0.073**	**-0.072**
	(0.0752)	**(0.0753)**	**(0.0792)**	**(0.0795)**
Exposed	0.015	-0.027	0.007	0.029
	(0.0560)	(0.0886)	(0.0567)	(0.0826)
Age	-	-0.006	-	-0.006
		(0.0113)		(0.0142)
Religion	-	0.057	-	0.001
(1=Islam, 0=else)		(0.0479)		(0.0508)
Constant	0.262***	0.430	0.270***	0.408
	(0.0387)	(0.3945)	(0.0397)	(0.3498)
Observations	613	613	554	554
R-squared	0.019	0.021	0.005	0.005
F-statistics for $\beta_1 + \beta_2 = 0$	0.09	0.13	0.09	0.09
(p-value)	(0.769)	(0.714)	(0.769)	(0.768)
Panel B: Female sample				
SSC (1=Yes, 0=Grade 9)	0.082***	0.081***	0.049*	0.051*
	(0.0306)	(0.0307)	(0.0288)	(0.0287)
SSC X Exposed	**-0.070***	**-0.072***	**-0.037**	**-0.043**
	(0.0385)	**(0.0380)**	**(0.0371)**	**(0.0368)**
Exposed	0.021	0.063	0.012	0.001
	(0.0218)	(0.0420)	(0.0232)	(0.0315)
Age	-	0.007	-	0.004
		(0.0056)		(0.0070)
Religion	-	-0.016	-	-0.058*
(1=Islam, 0=else)		(0.0283)		(0.0315)
Constant	0.023*	-0.195	0.031**	-0.022
	(0.0131)	(0.1953)	(0.0153)	(0.1767)
Observations	609	609	608	608
R-squared	0.014	0.017	0.006	0.015
F-statistics for $\beta_1 + \beta_2 = 0$	0.09	0.13	0.09	0.09
(p-value)	(0.769)	(0.714)	(0.769)	(0.768)

Note: Robust standard error in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%
Dependent variable is whether engaged in salaried employment. OLS estimates using HIES-2005 data.

Table 5. Effects on participation in salaried employment with different age brackets (Male Sample)

	Full sample		Relative to older cohort		Relative to younger cohort	
	(1)	(2)	(3)	(4)	(5)	(6)
SSC (1=Yes, 0=Grade 9)	0.128***	0.126***	0.156***	0.153***	0.088	0.089
	(0.0398)	(0.0398)	(0.0539)	(0.0542)	(0.0593)	(0.0595)
SSC X Early Exposed	**-0.020**	**-0.017**	**-0.048**	**-0.043**	**0.019**	**0.019**
	(0.0861)	**(0.0862)**	**(0.0935)**	**(0.0937)**	**(0.0968)**	**(0.0972)**
Early Exposed	-0.062	-0.063	-0.058	-0.074	-0.066	-0.091
	(0.0616)	(0.0616)	(0.0673)	(0.0968)	(0.0680)	(0.1572)
SSC X Late exposed	**-0.183****	**-0.181****	**-0.211****	**-0.204****	**-0.144**	**-0.145**
	(0.0818)	**(0.0822)**	**(0.0896)**	**(0.0901)**	**(0.0930)**	**(0.0937)**
Late exposed	0.068	0.074	0.072	0.042	0.063	0.052
	(0.0633)	(0.0643)	(0.0689)	(0.1339)	(0.0695)	(0.0969)
Age	-	0.003	-	-0.003	-	0.004
		(0.0038)		(0.0150)		(0.0249)
Religion	-	0.009	-	0.055	-	-0.002
(1=Islam, 0=else)		(0.0412)		(0.0478)		(0.0507)
Constant	0.266***	0.183	0.262***	0.332	0.270***	0.163
	(0.0277)	(0.1205)	(0.0387)	(0.5206)	(0.0398)	(0.6064)
Observations	859	859	613	613	554	554
R-squared	0.018	0.018	0.023	0.025	0.010	0.010

Note: Robust standard error in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%
Dependent variable is whether engaged in salaried employment. OLS estimates using HIES-2005 data.
Early exposed are those who are 29 to 30 years old, and late exposed are 26 to 28 years old.

Table 6. Difference-in-difference estimates for return to HSC (relative to grade 9) for male sample

	(1)	(2)
HSC (1=Yes, 0=Grade 9)	0.141***	0.142***
	(0.0484)	(0.0488)
HSC X Exposed	**-0.003**	**-0.002**
	(0.0786)	**(0.0790)**
Exposed	0.008	0.010
	(0.0492)	(0.0496)
Age		-0.001
		(0.0043)
Religion		-0.069
(1=Islam, 0=else)		(0.0492)
Constant	0.269***	0.359***
	(0.0280)	(0.1370)
Observations	643	643
R-squared	0.022	0.025
F-statistics for $\beta_1 + \beta_2 = 0$	4.96**	5.06**
(p-value)	(0.026)	(0.025)

Note: Robust standard error in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%
Dependent variable is whether engaged in salaried employment. OLS estimates using HIES-2005 data.

Table 7. Effects on participation in different types of employment for male sample

	Salaried Government	Salaried Private	Non-farm Self-employment	Day labor
	(1)	(2)	(3)	(4)
SSC (1=Yes, 0=Grade 9)	0.067***	0.058	-0.074*	-0.069***
	(0.0220)	(0.0363)	(0.0386)	(0.0247)
SSC X Exposed	**0.019**	**-0.102***	**0.163****	**-0.009**
	(0.0363)	**(0.0588)**	**(0.0658)**	**(0.0435)**
Exposed	-0.003	-0.014	-0.067	0.035
	(0.0277)	(0.0555)	(0.0595)	(0.0417)
Age	0.004	0.027	0.063	-0.038
	(0.0483)	(0.0752)	(0.0855)	(0.0513)
Age-square	-0.000	-0.000	-0.001	0.001
	(0.0008)	(0.0013)	(0.0014)	(0.0009)
Constant	-0.072	-0.128	-0.675	0.604
	(0.6899)	(1.0808)	(1.2247)	(0.7344)
Observations	859	859	859	859
R-squared	0.022	0.009	0.012	0.020

Note: Robust standard error in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%
Dependent variable is whether engaged in different type of employment. OLS estimates using HIES-2005 data.

Table 8. Determinants of initial salary and salary progression from employee records

	Initial salary	Salary progression		Current salary
	(1)	(2)	(3)	(4)
Completed SSC (1=Yes, 0=Grade 9)	0.103**	0.049*	0.035	0.042
	(0.0415)	(0.0271)	(0.0793)	(0.0334)
SSC X Exposed	**-0.113***	**-0.075****	**-0.187***	**-0.088***
	(0.0616)	**(0.0354)**	**(0.0999)**	**(0.0516)**
Exposed	-0.096	0.096***	0.203**	0.103**
	(0.0598)	(0.0301)	(0.0970)	(0.0462)
Tenure (years worked in the organization)	-	0.091***	0.085***	0.012***
		(0.0222)	(0.0291)	(0.0032)
Tenure X SSC	-	-	0.005	-
			(0.0182)	
Tenure X exposed	-	-	-0.025	-
			(0.0202)	
Tenure X SSC X exposed	**-**	-	0.027	**-**
			(0.0234)	
Age	0.047	-0.067**	-0.070**	0.019
	(0.0530)	(0.0273)	(0.0291)	(0.0424)
Age-squared	-0.000	0.001**	0.001**	0.000
	(0.0009)	(0.0004)	(0.0005)	(0.0007)
Joining year	0.128***	-0.010	-0.013	-
	(0.0090)	(0.0184)	(0.0191)	
Female (1=Yes, 0=No)	-0.109*	-0.079***	-0.082***	0.101**
	(0.0584)	(0.0272)	(0.0294)	(0.0396)
Log of initial salary	-	-0.209***	-0.226***	-
		(0.0430)	(0.0449)	
Constant	-249.443***	21.960	29.384	7.729***
	(18.1659)	(37.1378)	(38.5261)	(0.6296)
Observations	245	195	195	408
R-squared	0.484	0.750	0.754	0.377
F-statistics for $\beta_1 + \beta_2 = 0$	0.05	1.02	NA	1.07
(p-value)	(0. 832)	(0.314)		(0.301)

Note: Robust standard errors in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%
OLS estimates using employee records. Observations in the first three regressions include those who have joined the organization in 2003 or later. Dependent variable of the first regression is log of salary in 2003. For the 2nd and 3rd regression, the dependent variable is the difference between log of current salary (i.e. in 2008) and log of initial salary.

Table 9. Return to years of schooling (male sample)

	Full sample	Relative to older cohort	Relative to younger cohort	Full sample	Relative to older cohort	Relative to younger cohort
	(1)	(2)	(3)			
Education (Years of schooling completed)	0.036***	0.038***	0.034***	0.039***	0.040***	0.036***
	(0.0017)	(0.0019)	(0.0023)	(0.0020)	(0.0021)	(0.0026)
SSC (1=Yes, 0=Else)	0.025	0.015	0.040	-0.003	-0.013	0.015
	(0.0320)	(0.0405)	(0.0508)	(0.0332)	(0.0416)	(0.0526)
SSC X Exposed	**-0.114****	**-0.108****	**-0.122****	**-0.106****	**-0.101***	**-0.114***
	(0.0474)	**(0.0533)**	**(0.0615)**	**(0.0478)**	**(0.0537)**	**(0.0623)**
Exposed	0.007	0.018	-0.007	-0.002	0.012	-0.014
	(0.0164)	(0.0343)	(0.0345)	(0.0177)	(0.0350)	(0.0358)
HSC (1=Yes, 0=Else)	-	-	-	-0.047	-0.043	-0.060
				(0.0429)	(0.0510)	(0.0759)
HSC X Exposed	**-**	**-**	**-**	**0.018**	**0.008**	**0.044**
				(0.0634)	**(0.0689)**	**(0.0886)**
Grade 9 (1=Yes, 0=Else)	-	-	-	-0.103***	-0.133***	-0.068
				(0.0314)	(0.0420)	(0.0462)
Grade 9 X Exposed	**-**	**-**	**-**	**0.015**	**0.041**	**-0.012**
				(0.0521)	**(0.0588)**	**(0.0618)**
Age (in years)	0.004**	0.006	0.007	0.003*	0.006	0.007
	(0.0018)	(0.0048)	(0.0067)	(0.0018)	(0.0048)	(0.0067)
Religion (1=Islam, 0=else)	-0.017	-0.018	-0.020	-0.018	-0.019	-0.021
	(0.0203)	(0.0230)	(0.0264)	(0.0202)	(0.0229)	(0.0264)
Constant	-0.082	-0.161	-0.148	-0.063	-0.158	-0.137
	(0.0597)	(0.1679)	(0.1649)	(0.0600)	(0.1676)	(0.1648)
Observations	3,448	2,733	2,040	3,448	2,733	2,040
R-squared	0.112	0.124	0.092	0.116	0.129	0.094

Note: Robust standard error in parenthesis; * significant at 10%; ** significant at 5%; *** significant at 1%
Dependent variable is whether engaged in salaried employment. OLS estimates using HIES-2005 data.

Does asset increase affect school enrolment for the poor: Evidence from an experiment in Bangladesh

Abstract

Access to education is often found to be highly correlated with household income and wealth. This correlation does not necessarily imply that increases in income/wealth of the poor households will lead to an increase in children's school enrolment. This paper exploits the randomized roll-out of a large-scale asset transfer program in Bangladesh to test for the causal effect of asset transfer on children's schooling. We do not find any significant impact on enrolment although this program has been extremely successful in transforming the economic lives of the ultra-poor and causing substantial increases in their income and productive assets. We also find that the programme increased the extent of child labor. The increase in child labor is found to be concentrated in activities related to livestock rearing, which is the primary type of asset being transferred in this program. The evidence suggests that asset transfer may not be sufficient to increase school enrolment among households in extreme poverty and may have unintended effects on child labor.

Keywords: Asset transfer, enrolment, Bangladesh
JEL Classification: D04, I24, I38.

1. Introduction

Fostering human capital accumulation by the poor is one of the priorities in development. Besides its intrinsic values, human capital accumulation through education is important for breaking intergenerational poverty traps (Barham et al, 1995; Solon, 1999; Chadwick and Solon, 2002; Levine and Jellema, 2007). Therefore, how to increase school enrolment, especially for children from poorer socio-economic backgrounds, is an important policy question for developing countries. Since educational achievement is often strongly associated with the household income and wealth (Glewwe and Jacoby, 2004; Grimm, 2011; UNESCO, 2011), anti-poverty programs that are successful in increasing income of poor households have the potential of creating a positive spillover effect on school enrolment of their children. On the other hand, such programs can also have adverse effects on children's education depending on the form of support in these programs. For example, an increase in asset of the poor can increase the demand for child labor and reduce school enrolment (Cockburn and Dostie, 2007). This paper provides experimental evidence on the impact of an asset transfer program for the ultra-poor in Bangladesh on children's schooling.

The program is called 'Challenging the Frontier of Poverty Reduction/Targeting Ultra-Poor' (CFPR/TUP), which has been designed and being implemented by BRAC[1] in Bangladesh since 2002. Consultative Group to Assist the Poor (CGAP), in association with Ford Foundation, has undertaken a major initiative to test and scale-up this approach in over 10 countries.[2] In this program, ultra-poor households are provided with income generating assets (mostly livestock) along with a range of other supports – including training, stipends, free health care facility and input supports for their livestock for a specified period – in order to enable them obtaining a more productive livelihood. Early results from evaluation of this approach indicate that the program has substantial effects on increasing income of the participant households.[3] However, it is not clear whether this impact on income has brought any change in school enrolment of their children.

In this paper, we do not find any significant impact on school enrolment for either boys or girls of the program participants. This lack of impact is observed at both primary and secondary levels. There was no effect on academic performance in general, in terms of grade repetition, for the children who were enrolled in both survey rounds. However, girls of primary school age were 7 percentage points more likely to repeat a grade. This indicates that the program deteriorated school performance of these girls. On the other hand, program participation had significant impact on increasing the amount of child labor, especially in activities related to livestock rearing. This clearly shows increased demand for child labor due to livestock transfers. Increase in child labor is also found to be associated with enrolment status, with school dropout children having the highest

[1] BRAC is a non-governmental organization operating in several Asian and African countries. For more information on BRAC, visit www.brac.net

[2] http://www.cgap.org/p/site/c/access/ contains information on the ongoing pilots including program details and evaluation designs. (Accessed on Nov 12, 2011)

[3] There are a number of experimental evaluations being implemented. Bandiera et al (forthcoming) evaluate both the direct and indirect impact of this program on occupational choice, productivity and income in Bangladesh. Banerjee et al (forthcoming) are also evaluating this program in West Bengal in India. While the results of these evaluations are yet to be made public, evaluations of this program in Bangladesh using various non-experimental methods have found large impact on income (Rabbani et al, 2006; Emran et al, 2009; Ahmed et al, 2009).

increase in hours spent in livestock rearing (over 120 hours in a year) and new enrolled children having the lowest increase (about 50 hours in a year). These effects on child labor do not fully explain the lack of effects on school enrolment since we observe significant increases in child labor irrespective of whether they were enrolled or not.

Although the particular approach of asset transfer followed in TUP has been very successful in creating greater economic opportunities for the ultra-poor, this lack of impact on human capital accumulation needs to be given more emphasis. The transfers in TUP are not conditional on school enrolment of the children. Using the case of PROGRESA, de Brauw and Hoddinott (2011) have shown that it is the conditionality rather than the transfers *per se* that influences investment in human capital. Therefore, applying conditionality or providing other educational supports targeting school enrolment for the ultra-poor households needs to be tested to yield longer-term and more consequential impact.

The paper is organized as follows. Section 2 briefly describes the context in terms inequality in access to education across different income groups. Section 3 discusses the TUP program, the experiment design and data. Section 4 presents the results of average effects on school enrolment and child labor along with policy discussions. Section 5 concludes the paper.

2. Poverty and school enrolment in Bangladesh

Access to education is usually associated with the economic well-being of the households. In this section, we briefly examine the variations in school enrolment across different income groups in Bangladesh. In order to explore the extent of inequality in access to education using national Household Income and Expenditure Survey (HIES-2005), the households have been ranked into quintiles by their per capita adult-equivalent expenditure. It is important to clarify here that BRAC uses the term 'ultra-poor' to refer to the poorest 10% in income distribution. Therefore, we also separate the households of the poorest quintile into two deciles to observe existence of any inequality in access to education within the poorest end of income distribution.

Table 1 shows the net primary and secondary enrolment rates for boys and girls across different expenditure quintiles. We observe significant differences in the net enrolment rates, at both primary and secondary levels, between children living in the poorest and the second poorest quintiles. Boys belonging to households of the poorest quintile had a net primary enrolment rate of 57%, which was significantly lower than the second poorest quintile (65%) in 2005. Despite the government initiative of universal primary education policy since early 1990s, this difference in access to education at primary level is quite noteworthy. Significant differences are observed within the bottom quintile as well. When these households are split into two income deciles, enrolment rates for the bottom decile are found to be lower compared to the second poorest decile. Net primary enrolment rates for boys in the poorest and the second poorest deciles were 53% and 61% respectively. Similar differences are observed for net primary enrolment rates of girls (60% vs. 66%), and for net secondary enrolment of boys (14% and 27%) and girls (27% and 35%). This pattern of inequality in education is sometimes presented to argue that increasing household income for the ultra-poor will lead to an increase in the school enrolment rates of their children (Maitra, 2003).

3. BRAC's Ultra-Poor Program and evaluation design

3.1. Programme Description[4]

BRAC has been a pioneer in implementing programs that target extreme poverty in Bangladesh, and the ultra-poor program is one of their more recent initiatives. This paper focuses on the second phase of this program, which started in 2007 with the aim of reaching over 800,000 households in 40 districts of the country by 2011. The program targets ultra-poor women in rural areas who are unable to access and benefit from mainstream poverty reduction programs.

BRAC's ultra-poor program aims to economically, socially and psychologically empower the poorest women in Bangladesh through a multi-faceted intervention package. Women who participate in the program receive a combination of assets, such as cows, goats, poultry or seeds for vegetable cultivation. Although some of the services are provided to all the household members and the program is viewed as household intervention, all the transfers are made through a woman member of each household. They commit to retain the asset for two years but they are allowed to sell it or exchange it for another income generating asset within that period. After that, they are free to dispose of it as they see fit.

The asset transfer is accompanied with skills training, specific to the type of asset provided. A trained asset specialist visits each beneficiary household every 1-2 months during the first year of the program. In addition, BRAC program officers visit them weekly for the first two years to ensure that these ultra-poor women, who have no prior experience of running a business on their own, are fully supported.

To compensate for the short-run fall in income due to the occupational change, a weekly subsistence allowance is provided for the first 40 weeks, which is until the participants learn to manage the assets well enough to generate a regular flow of income. Between 18 and 24 months into the program, the beneficiaries also take part in confidence-building sessions about how to use microfinance and are enrolled in village-level microfinance groups.

Other components of the program include a savings scheme, preventive and curative health care services, and social development support involving training on legal, social and political rights. The ultra-poor households receive monthly visits from a health volunteer and get access to BRAC's legal services. There are also provisions for full cost of treatment if any member of the beneficiary households suffers a major illness. In addition, BRAC initiates the establishment of village committees that bring together representatives from the village elite and ultra-poor households. These committees extend social and financial supports to all the beneficiary households.

3.2. Targeting

At the first stage of selection into the program, BRAC decides which districts to reach. These are chosen to be the most vulnerable in the country in terms of food insecurity mapping conducted by

[4] For a more elaborate discussion of the program components and targeting, see Matin et al (2008).

World Food Program (WFP). BRAC employees from local branch offices then select the relatively poorer communities within a branch. Each selected area is referred to as a 'spot' or a community, which is a cluster of approximately 100 households and is usually smaller than a village. (For simplicity, we use the term 'village' to refer to these spots throughout the rest of this paper.)

The program uses a combination of participatory wealth ranking and survey methods to identify the ultra-poor households in each village. First, a participatory rural appraisal (PRA) is conducted to divide all households in a village into five community-defined wealth categories and identify the poorest households, one being the wealthiest and five the poorest. The households ranked in the lowest wealth rank become the 'community-selected ultra-poor'.

In the final stage of targeting, BRAC workers visit the community-selected ultra-poor households and conduct a brief survey to determine who meets the programs selection criteria. There are three exclusion criteria, all of which are binding. Households who are already borrowing from a microfinance institute, who are recipients of government anti-poverty programs, and who have no adult woman in their members, are excluded from the program. Furthermore, to be selected by the program, a household has to satisfy at least three of the following five inclusion criteria: (i) total land owned including homestead is not more than 10 decimals; (ii) there is no adult male income earner in the household; (iii) adult women in the household work outside the homestead; (iv) school going-aged children have to work; and (v) the household has no productive assets. After further cross-checks by branch managers, to make sure that the information provided in the survey is correct, the households that satisfy at least three of these criteria and none of the exclusion criteria are defined as the Specially Targeted Ultra-Poor (STUP) households. After these screening of eligibility, all these STUP households are targeted by the program and receive the intervention package.

3.3. Evaluation Strategy and Survey Design

The roll-out of the program was randomized to identify the effects of the program by comparing treated to control villages before and after program implementation. Randomization was done at branch office level so that all communities within a branch were treated either in 2007 or in 2011.[5] We survey all ultra-poor households at baseline and every two years until the program roll-out ended in 2011. Data used in this paper covers a total of 40 branches (of which 20 are treated), 1409 villages and 6819 STUP households who have been interviewed both in the baseline (in 2007) and in the first follow-up (in 2009) surveys. A random sample of non-ultra-poor households have also been surveyed to measure spillover effects, and the total number of households surveyed in the main evaluation is over 25,000. However, only the STUP households have been used for this paper.

[5] We stratified the randomization at the sub-district level. To be precise, the choice of sample and the randomization of treatment timing followed the following steps. First, the programme selected all the branch offices they wanted to treat (15 districts, 133 branch offices). Second, we determined districts in which there are upazilas (sub-districts) with more than one branch office. Then, for districts that contained upazilas that have more than one branch office, we drop the upazilas with only 1 branch office in them. Out of the rest, we: (i) randomly selected 2 upazilas for the Northern districts and 1 for the Non-North districts. (ii) Within each upazila, one control and one treatment branch office were randomly selected. For districts that do not have any upazilas with more than two branch offices in them (only one: Kishoreganj), we randomly allocate one branch office to control, and one to treatment.

Ultra-poor households were selected at the same time in both treatment and control branches, using the targeting method outlined above. The only difference between them is that ultra-poor in treated branches received the assets immediately whereas ultra-poor in control branches received them in 2011.

The baseline survey was carried out between April 2007 and February 2008 to measure the pre-program characteristics of treatment and control households. Bandiera et al (forthcoming) find that the STUP households from treatment and control branches had very similar characteristics at baseline. Normalized differences in a range of baseline characteristics between the two groups are lower than 0.15. Imbens and Wooldridge (2009) suggest the normalized differences should be less than 0.25, as a rule of thumb, for consistency of estimates.

The first follow-up survey was carried out on the same households during January-December 2009. There were on average 800 (minimum 511, maximum 987) days in between the baseline and follow-up surveys for a household. This implies that by the time we observed the treated households at follow-up the most intensive part of the program was over.

4. Findings

In this section we present the estimates of average effects of program participation on school enrolment. We estimate the average impact using the following linear probability model[6]

$$y_{ijt} = \beta_0 + \beta_1 treat_j + \beta_2 followup_t + \beta_3 treat_j * followup_t + \delta_k X_{ij} + u_{ijt} \quad (1)$$

where y_{it} is a dummy equal to 1 if the individual *i* from household *j* in time *t* is enrolled in school and 0 otherwise. $Treat_j$ is a dummy for household *j* being assigned to treatment in the randomized experiment, $followup_t$ is a dummy equal to 1 for follow-up survey period, X_{ij} is a vector of individual and household controls from the baseline and u_{ijt} is the error term. While the baseline characteristics are uncorrelated with treatment and unlikely to affect the coefficients, inclusion of these controls can reduce standard errors of the estimates[7]. The coefficient of the interaction between treatment and follow-up interaction (β_3) identifies the impact of the program under two key assumptions of a) common trend between treatment and control groups, and b) no spillover effect on the control group. Since the control households come from villages located within control branch offices, there is almost no possibility of the program having any spillover effect on them.

Before presenting the estimates of the impact on enrolment rates, Figure 1 graphically presents the impact of the program on household income. On the graph, total household income (in 2007 price) is on the horizontal axis and the lines represent the cumulative distribution functions for the STUP households from treatment and control branches at baseline and follow-up. The graph shows that these two groups of households were almost identical in their income distribution during the

[6] We also replicated the results with logit models. The estimates are not different from the OLS estimates, and only OLS estimates have been presented.

[7] Baseline characteristics included as controls are age of the children, household demography (number of members in different age groups, sex of household head, number of members with disability), respondent characteristics (age, religion and tribe), experience of crisis event, and ownership of different types of assets.

baseline period. While the households in control branches experienced growth in real income between baseline and follow-up, the growth was substantially higher for the treatment households. This is a clear demonstration of the impact of the program on household income. Bandiera et al (forthcoming) critically evaluate the impact of the program on income, economic activities and assets. They find very large effects on all these dimensions, and the graph is presented to put the main effects of the program into perspective.

4.1. Effects on schooling

Table 2 presents the impact estimates on school enrolment for boys and girls who are in their primary (6-10 years) and secondary (11-15 years) school age. Enrolment rate for boys of primary school age in control households was 73% at baseline (Column 1). There was no significant difference between the children in treatment and control groups at baseline. There was also no significant change in these enrolment rates between the baseline and the follow-up surveys. The difference-in-difference estimate on the enrolment rate of primary school age boys is -0.04 and insignificant at conventional levels. This implies that the program did not have a significant impact on enrolment rate of these boys. Controlling for baseline differences in observable characteristics of the households does not change the estimate (Column 2).

The same estimates have been presented for girls of primary school age (Column 3 and 4); and for secondary school age boys (Column 5 and 6) and girls (Column 7 and 8). The results show that there was no significant impact on the enrolment rates for any of group of children. These zero effects on enrolment are quite precisely estimated. We can reject a positive effect of 2 percentage points on enrolment of primary school aged boys at 5% significance level. The comparable positive effects that can be rejected at the same statistical significance level are 4 percentage points for primary school age girls, 9 percentage points for secondary school age boys and 3 percentage points for secondary school age girls. Therefore, the evidence clearly shows that the program did not increase school enrolment of the children despite its 'huge success' in increasing income of the beneficiaries.[8]

While enrolment rate is the most basic measure of access to education, this does not reflect the students' performance in schools. It is possible that the program may have affected achievement of the students if households invested more in quality of education than in its quantity. Data on test scores and other schooling outcomes were not collected in this study. However, as a proxy for school achievement, we can test the effect of the program on achievement of the enrolled children, in terms of grade repetition (attending the same grade for more than one year). For this, we have used the following specification

$$y_{ij} = \beta_0 + \beta_1 treat_j + \delta_k X_{ij} + u_{ij} \qquad (2)$$

Where y_{ij} is a dummy equal to 1 if children *i* from household *j* repeated a grade between the baseline and the follow-up surveys. β_1 measures the impact of treatment on grade repetition. Table 3

[8] It can be argued that it takes longer than 2 years for the increases in income or asset to increase school enrolment, and here we look at the short-run effects. Sulaiman (2010) did not find any significant average effect on children's school enrolment after 3 to 6 years of program participation by the beneficiary households. The study uses non-participant ultra-poor from the treatment villages as the comparison group.

presents the estimated impacts on children who were enrolled in both survey rounds. We find that the program did not have any significant impact on grade repetition for boys. Point estimates show that grade repetition increased for primary school age children in the treated households by 3 to 4 percentage points (Column 1 and 2) although not significant at conventional levels. However, the likelihoods of grade repetition significantly increased (by 7 percentage points) for girls of primary school age (Column 3 and 4). The estimates are also positive, meaning an increase in grade repetition due to program participation, for girls of secondary school age (4 to 5 percentage points) although not significant at conventional levels. These results suggest that the program had some negative influence on school performance, in terms of repeating grades, of the children.

4.2 Effects on child labor

Table 4 presents the impact estimates on child labor using specification of equation 1, where the dependent variables are hours spent in livestock rearing and total hours of engagement in any earning activities during the one year preceding the survey.

Estimated impacts on time spent in livestock rearing are 32-38 hours per year for 6-10 year old children (Panel A), and 117-127 hours per year for 11-15 year old (Panel B). On average, the program increased the amount of child labor engaged in livestock rearing by 4 days (assuming 8 hour work-days) for 6-10 year old children. The corresponding increase for 11-15 years old children was 15 days. These effects are substantially large compared to their baseline levels. During the baseline period, 6-10 and 11-15 year old children spent 11.5 and 40.4 hours in livestock rearing respectively. Therefore, the program made an almost three-fold increase in the amount of child labor in livestock rearing.

Point estimates of the impacts on total hours of work are higher than the impacts on hours of work associated with livestock rearing. However, the differences in the impact coefficients for hours spent in livestock rearing and total hours of work are not significant at conventional levels for the four groups of children, except 6-10 year old boys.[9] Nonetheless, a major part of the effects on total work hours are driven by increased hours spent in livestock rearing. Time spent in livestock rearing accounts for 40% of the impact on total work hours for 6-10 year old boys. These ratios are 75% for 6-10 years old girl, 76% for 11-15 year old boys and 56% for 11-15 year old girls. This pattern of greater impact on livestock rearing activities, compared to other earning activities, is natural since over 90% of the program participants received livestock from the program. This increase in child labor, coupled with no impact on enrolment, indicates that the children have substituted some of their leisure for work.

4.3 Association between changes in child labor and enrolment

To explore how the changes in time spent on work are associated with the changes in enrolment, Figure 2 shows the average changes in hours spent in livestock rearing by the changes in enrolment status of children. There are 2 key findings that Figure 2 demonstrates. First, the graph clearly shows that the treatment households had much larger increases in the amount of child labor in livestock

[9] Significance of the differences in coefficients were checked using Chow test.

rearing compared to the control households. Secondly, there are noticeable differences across the four enrolment groups in treatment households. Children from treatment households who dropped out between the two survey rounds had the highest increase in time spent on livestock rearing. On the other hand, the new enrolled children – who got enrolled after the baseline but before the follow-up survey – experienced the lowest increase in their hours of work in livestock rearing. Always enrolled and never enrolled observed similar increase in their work hours between the two rounds.

Table 5 measures this association (equation 3) between the impacts on time spent for livestock rearing and the changes in enrolment, controlling for baseline characteristics. These are mere associations between the changes in enrolment and the changes in child labor (rather than heterogeneity in the impact of treatment) since both the dimensions of change occurred simultaneously and are likely to be results of program participation.

$$\Delta y_{ij} = \beta_0 + \beta_1 treat_j + \beta_2 dropout_{ij} + \beta_3 new_{enrol_{ij}} + \beta_4 never_{enrol_{ij}} + \beta_5 dropout_{ij} * treat_j + \beta_6 new\_enrol_{ij} * treat_j + \beta_7 never\_enrol_{ij} * treat_j + \delta_k X_{ij} + u_{ij} \quad (3)$$

In this specification, Δy_{ij} is the change in time spent in livestock rearing by individual *i* from household *j*; $dropout_{ij}$, new_enrol_{ij} and $never\_enrol_{ij}$ are dummies, equal to 1 if dropout, newly enrolled in the follow-up and never enrolled respectively, and 0 otherwise. Therefore, β_2, $\beta_3$and β_4 compare the differences in change in child labor from always enrolled (the base category) for the control households. The interaction terms (β_5, $\beta_6$and β_7) measure the differential changes for these three groups of children for the treatment households.

Coefficients of Table 5 reflect the pattern that we already observe in Graph 2. Column 1 reports the regression result for the whole sample of children (aged 6-15 years). In this sample, the impact on child labor is 83 hours of work per year for those who are always enrolled. The impacts are larger for dropout and never enrolled children, and lower for newly enrolled children. These associations indicate that a part of the lack of impact on enrolment could be due to increased child labor. The size of increase in work hours for the always enrolled children of treatment households indicate that increased child labor cannot be the only explanation of lack of impact on enrolment. It is worth reemphasizing that these are only associations between child labor and enrolment, and not tests of causality between them.

4.4 Discussion on policy implications

The results clearly show that the ultra-poor program had no significant impact on school enrolment of children and increased the extent of child labor. Despite its laudable successes in accelerating growth in income of the ultra-poor households, this is an important concern in measuring success of this approach of poverty reduction, especially in the context of breaking inter-generational poverty traps through increased human capital accumulation. Since household poverty is found to be a key determinant of access to education, progress in poverty reduction is often expected to yield improvement in education. For example, Maitra (2003) claims that educational attainment of children increases with an increased permanent income of the household in Bangladesh. This paper has demonstrated that improvement in schooling does not automatically follow an increase in

income and wealth. However, it is possible that a positive effect of income growth on school enrolment has been balanced out by a negative impact of asset increase.

In recent years, there has also been a surge in the number of conditional transfer programs to promote schooling and other human capital accumulation by the poor. Triggered by the success of PROGRESA, a substantial number of conditional cash transfer (CCT) programs have been initiated in many developing countries, especially in Latin America. A large and growing literature has concentrated on measuring the effects of CCTs on school enrolment and human capital accumulation. Skoufias (2005) finds that CCT increased secondary school enrolment by 7-9 percentage points for girls and 3-5 percentage points for boys in Mexico. In Colombia, Attanasio et al (2006) find 5 and 7 percentage points increase in enrolment of youth (aged 14-17 years) in urban and rural areas respectively.

While there is a general consensus across studies on the positive effects of CCTs on school enrolment, there is less consistency in the evidence on children's performance in schools (Garcia and Hill, 2010). Our evidence shows that the ultra-poor program had zero or negative effect on performance in schools, measured by the likelihood of repeating grades. Nonetheless, the differences in effects on school enrolment between the two types of programs indicate the possibility of using conditionality in the ultra-poor program, which is not conditional on enrolment, as a mechanism of influencing children's schooling. Using the case of PROGRESA, de Brauw and Hoddinott (2011) have shown that it is the conditionality rather than the transfers *per se* that influences investment in human capital. Therefore, this supports the policy suggestion of piloting conditionality in the ultra-poor program.

These two approaches are different not only in imposing conditionality but also in many other important aspects. The form of transfer (cash vs. productive assets) is one of the other key important differences. While asset transfer in the ultra-poor program has been found successful in increasing income of the poor, this approach has also increased the demand for child labor in the beneficiary households. Change in the demand for child labor is often identified as an important link in the 'poverty-education nexus' (Amin et al, 2004) although there is weak correlation between child labor and school enrolment (Ravallion and Wodon, 2000) in Bangladesh. Using very detail information of time use by the children in rural Ethiopia, Cockburn and Dostie (2007) find that demand for child labor increases with asset accumulation. According to their findings, an increase in the ownership of livestock increases demand for child labor while increase in income reduces child labor and increase demand for education. The net effect depends on the relative magnitudes of the income and asset effects. Therefore, it is possible that the net zero effect on enrolment in the ultra-poor program is due to these opposing effects balancing out each other.

Evaluations of CCTs, in general, observe decrease in child labor (Schultz, 2004; and Skoufias, 2005). In their evaluation of a CCT program in rural Colombia, Attanasio et al (2006) find that time spent in education and work are not completely substitutes to each other. Children may draw some of their time from leisure to increase time spent in schools. Through conditionality in asset transfer programs, it might be possible to influence enrolment with/without a simultaneous increase in child labor. The process of imposing conditionality, however, needs to be thought through. While cash transfers can stop when the household fails to meet the conditionality, asset transfers are mostly

done in one go. Other supports in this program (such as weekly stipend) can potentially be tied to children's school attendance.

5. Conclusion

Currently there are a number of social protection programs that are designed and implemented to reduce extreme poverty. In this paper, we evaluate the effects of an asset transfer program on school enrolment and child labor. Despite being extremely successful in transforming economic lives of the ultra-poor in Bangladesh, this program has not affected schooling of the children in beneficiary households. This lack of impact is observed for both boys and girls, who are of either primary or secondary school age. There was a negative impact on the performance in schools for secondary school age girls, who were 7 percentage points more likely to repeat a grade within the 2 years of evaluation period.

On the other hand, asset transfers significantly increased the amount of time that the children spent in livestock rearing. The effects are about 4 additional days of work in a year for 6-10 year old children and about 15 days for 11-15 years old children. The impact on total amount of work was larger than the impact on livestock rearing, but livestock rearing alone accounts for up to 76% of the increase in total work hours. We also find that these changes in the amount of child labor are associated with the changes in enrolment. However, the increase in child labor does not fully explain the reason for lack of impact on enrolment. We conclude that asset transfer may not be sufficient to make significant improvement in school enrolment of children living in extremely poor households, and there could be unintended consequences on child labor. Drawing on the lessons from CCTs, we recommend testing conditionality within the asset transfer programs.

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Figure 1. Cumulative distribution of household income of treatment and control households

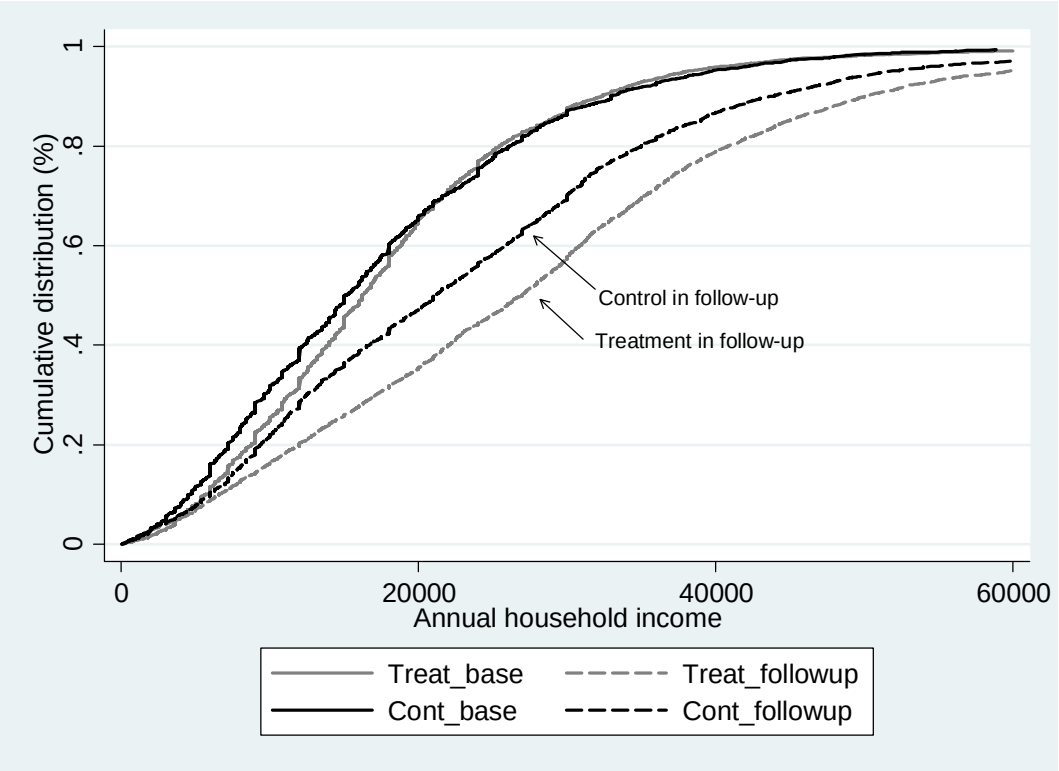
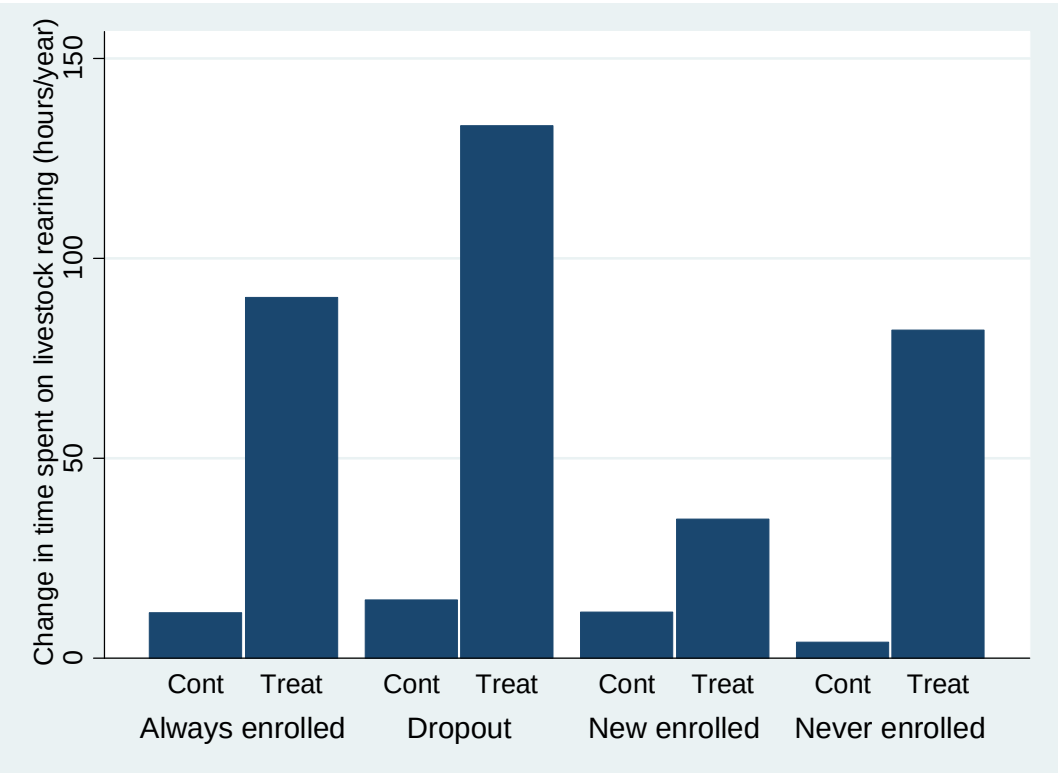


Figure2. Change in child labor in livestock rearing by enrolment change



Note: The figures are average for children aged 6 to 15 years.

Table 1. Net Enrolment rates among household expenditure groups (2005)

Household category	Net primary		Net Secondary	
	Boys	Girls	Boys	Girls
Poorest Quintile	57	63	20	31
2nd Quintile	65	68	31	43
3rd Quintile	72	70	38	54
4th Quintile	74	77	54	63
Richest Quintile	81	80	69	72
Total	68	71	41	51
t-test (Bottom two Quintiles)	3.09***	1.88**	4.71***	4.47***
2nd Decile	61	66	27	35
Bottom Decile	53	60	14	27
t-test (Bottom two deciles)	2.58**	1.69*	4.14***	2.24**

Source: calculated from HIES-2005; [a] Difference between the first and second quintile
*** p<0.01, ** p<0.05, * p<0.1

Table 2. Average effect on school enrolment

	Primary school age				Secondary school age			
	Boys		Girls		Boys		Girls	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.023	0.018	0.036	0.022	-0.037	-0.037	0.020	0.017
(1=Yes, 0=No)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Follow-up	0.012	-0.005	0.024	-0.014	0.055	0.016	0.037	0.043
(1=2009, 0=2007)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)**	(0.02)	(0.03)	(0.03)*
Treatment X Follow-up	**-0.037** **(0.03)**	**-0.040** **(0.03)**	**-0.018** **(0.03)**	**-0.010** **(0.03)**	**0.043** **(0.03)**	**0.034** **(0.03)**	**-0.037** **(0.04)**	**-0.035** **(0.03)**
Constant	0.730	-2.846	0.738	-2.471	0.399	1.103	0.525	1.970
	(0.02)***	(0.26)***	(0.02)***	(0.26)***	(0.02)***	(0.86)	(0.02)***	(0.93)**
Baseline controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	3671	3671	3258	3258	2641	2641	2390	2390
R-squared	0.001	0.131	0.001	0.126	0.007	0.272	0.001	0.217

Standard errors in parenthesis clustered at village level; *** p<0.01, ** p<0.05, * p<0.1

Table 3. Average effect on grade repetition

	Primary school age				Secondary school age			
	Boys		Girls		Boys		Girls	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	**0.040**	**0.032**	**0.072**	**0.068**	**-0.017**	**-0.006**	**0.052**	**0.037**
(1=Yes, 0=No)	**(0.03)**	**(0.03)**	**(0.03)****	**(0.03)****	**(0.05)**	**(0.05)**	**(0.04)**	**(0.04)**
Constant	0.309	1.244	0.278	1.369	0.179	-6.151	0.140	5.069
	(0.02)***	(0.55)**	(0.02)***	(0.57)**	(0.04)***	(2.08)***	(0.03)***	(2.41)**
Baseline controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1142	1142	1063	1063	242	242	319	319
R-squared	0.002	0.044	0.006	0.055	0.001	0.098	0.005	0.091

Standard errors in parenthesis clustered at village level; *** p<0.01, ** p<0.05, * p<0.1

Table 4. Average effect on time spent in earning activities

	Boys		Girls	
	Livestock	Total	Livestock	Total
	(1)	(2)	(3)	(4)
Panel A. Primary school age (6-10 years)				
Treatment	-4.522	-40.253	-4.335	-9.726
(1=Yes, 0=No)	(6.01)	(22.54)*	(3.41)	(20.16)
Follow-up	-6.380	-44.430	-0.016	-30.915
(1=2009, 0=2007)	(5.20)	(19.18)**	(4.23)	(21.86)
Treatment X	**32.112**	**79.665**	**38.841**	**51.581**
Follow-up	**(7.61)*** **	**(25.35)*** **	**(6.28)*** **	**(28.97)***
Constant	86.759	923.371	58.730	703.891
	(62.28)	(194.41)***	(51.25)	(218.69)***
Baseline controls	Yes	Yes	Yes	Yes
Observations	3671	3671	3258	3258
R-squared	0.043	0.095	0.072	0.139
Panel B. Secondary school age (11-15 years)				
Treatment	1.227	-113.795	-6.948	-61.571
(1=Yes, 0=No)	(11.27)	(68.83)	(11.32)	(54.70)
Follow-up	3.327	-73.326	9.170	-257.217
(1=2009, 0=2007)	(9.92)	(67.65)	(12.61)	(58.20)***
Treatment X	**127.474**	**167.555**	**117.843**	**211.843**
Follow-up	**(16.03)*** **	**(90.00)***	**(17.35)*** **	**(70.81)*** **
Constant	-1,225.844	6,456.582	-927.634	-691.667
	(447.05)***	(2,224.01)***	(451.42)**	(1,643.05)
Baseline controls	Yes	Yes	Yes	Yes
Observations	2641	2641	2390	2390
R-squared	0.107	0.283	0.100	0.194

Standard errors in parenthesis clustered at village level; *** p<0.01, ** p<0.05, * p<0.1

Table 5. Association between enrolment and time spent in livestock rearing

	All children	Primary school age		Secondary school age	
		Boys	Girls	Boys	Girls
	(1)	(2)	(3)	(4)	(5)
Treatment	**83.354**	**67.186**	**70.759**	**155.794**	**122.242**
(1=Yes, 0=No)	**(8.18)*** **	**(9.33)*** **	**(10.73)*** **	**(26.19)*** **	**(31.93)*** **
Dropout	-11.877	-16.367	13.851	15.771	-6.905
(1=Yes, 0=No)	(10.25)	(16.18)	(19.41)	(21.88)	(26.51)
New enrolled	20.081	9.151	30.598	43.991	-60.100
(1=Yes, 0=No)	(9.28)**	(7.62)	(13.44)**	(62.74)	(62.20)
Never enrolled	-39.716	-23.630	-13.287	-15.234	-40.174
(1=Yes, 0=No)	(11.73)***	(25.51)	(13.49)	(23.81)	(31.73)
Dropout X treatment	**40.964**	**37.823**	**12.877**	**-25.035**	**25.227**
	(17.68)**	**(27.61)**	**(31.46)**	**(40.80)**	**(42.43)**
New enrolled X treatment	**-34.655**	**-38.976**	**-15.137**	**-35.327**	**-50.393**
	(13.49)**	**(12.61)*** **	**(22.05)**	**(83.47)**	**(89.52)**
Never enrolled X treatment	**52.620**	**38.086**	**37.184**	**-7.739**	**16.270**
	(16.30)* **	**(43.75)**	**(29.60)**	**(34.97)**	**(42.35)**
Constant	-246.113	-231.954	-145.309	1,694.897	1,278.941
	(47.96)***	(138.40)*	(164.97)	(949.53)*	(1,036.42)
Baseline controls	Yes	Yes	Yes	Yes	Yes
Observations	5784	1835	1627	1221	1101
R-squared	0.075	0.070	0.099	0.103	0.096

Standard errors in parenthesis clustered at village level; *** p<0.01, ** p<0.05, * p<0.1