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

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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
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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.

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

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 (1=yes, 0=No)	0.733 (0.022)***	0.820 (0.046)***	0.683 (0.056)***	0.810 (0.043)***	0.271 (0.085)***	0.829 (0.047)***	0.918 (0.031)***
Control mean	0.141 (0.016)***	0.071 (0.028)**	0.187 (0.040)***	0.086 (0.031)***	0.358 (0.059)***	0.108 (0.036)***	0.068 (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)
Treatment X follow-up	**75.03** **(127.22)**	**-175.38** **(151.71)**	**21.10** **(106.94)**	**190.09** **(170.67)**	**-1.86** **(87.84)**	**-29.45** **(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 follow-up	**-85.28** **(47.42)***	**-105.69** **(42.20)****	**-25.62** **(32.87)**	**-43.50** **(28.27)**	**-59.66** **(33.35)***	**-62.19** **(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 follow-up	**2.56** **(53.03)**	**33.92** **(28.34)**	**47.79** **(123.71)**	**-9.24** **(40.70)**	**-0.19** **(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

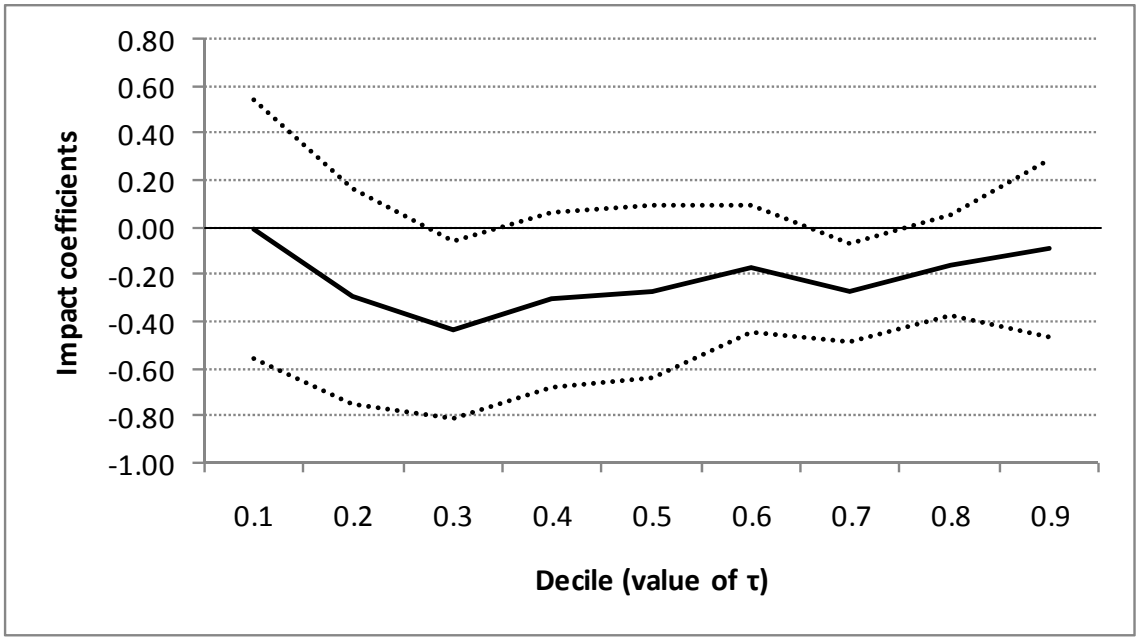
	Log of per capita income			Received transfers		
	25 meter	50 meter	100 meter	25 meter	50 meter	100 meter
Follow-up (1=2009, 0=2008)	-0.132 (0.126)	-0.203 (0.134)	-0.176 (0.145)	0.069 (0.028)**	0.065 (0.030)**	0.041 (0.033)
Treatment density	-0.065 (0.060)	-0.034 (0.036)	-0.004 (0.018)	0.005 (0.015)	-0.000 (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%