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

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Abstract

Food assistance is one of the most common forms of safety net programs in post-conflict situations. Besides the humanitarian and promotional roles, there are widespread scepticisms of food assistance regarding its possible influence on disincentive to work and on crowding out of private transfers. While there is a relatively large amount of empirical research on social protection in stable context, it is less researched in post-conflict situations. Based on randomized evaluation of a food-for-training program implemented in Southern Sudan, this paper estimates these effects. We observe a significant negative impact (about 13%) on per capita household income. However, there is no effect on the hours of work or the type of the economic activities of the adult members. The decline in income mostly happened through a reduction in child labor. There is also a positive effect on school enrolment for girls (about 10 percentage points) and an improvement in their housing status. We also do not find any indication of crowding out of private transfers for the participants. This is most likely due to the extent of private transfers being very low to begin with. However, there is a small but significant impact of the transfers given out by the participants.

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1 Introduction

Food assistance programs as direct food distribution, food-for-training, food-for-work, school feeding or in any other form are widespread in every country of the world. However, the objectives of food aid in fragile and stable settings have a fundamental difference. In crisis situations (either natural or man-made), food aid takes the traditional form of direct distribution driven by the humanitarian intention of saving lives from starvation. On the other hand, food assistance in more stable contexts is often used as a livelihood promotion mechanism. Barrett and Maxwell (2005) describe these two notions of food assistance as 'emergency' and 'developmental'.

There is a long literature discussing different aspects of food assistance including (and certainly not limited to) the incentive effects of such transfers on labor supply (Abdulai et al, 2005), change 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. However, most of the empirical works, especially with micro data, are limited to relatively stable contexts. Moreover, the findings are not unequivocal. For example, Lentz (2003) in her careful review of the 25 years of literature did not find any incidence of a clear evidence of (dis)incentive effects of food assistance. In this paper, we use experimental data of a food-for-training program in Southern Sudan to assess its 'developmental' impact.

Effect of transfers on labor supply has been a subject of critical scrutiny in developed countries (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 would consume more of both leisure and other commodities. Therefore, aid would create a disincentive to work. However, the predictions of the disincentive effect of transfers also depend on the underlying model, preference map of the individuals, size of transfer and the transfer structure.

Moreover, 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 size of transfers is often linked with actual amount of work/income of the individuals. The particular policy (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 7 months period. The transfer is a short-term increase in non-labor income (like winning a lottery or similar windfall gain). Based on life-cycle model, a one-off transfer like this will reduce income but the effect will be distributed over the life-span of the decision making unit. Therefore, the effect on the particular year may not be high enough to be detected. Such a model also 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).

A large part of the empirical research on incentive effects 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). However, the evidence of disincentive 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. However, 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. However, they also rely on endogenous program participation to identify impact estimates. Barrett (2002) argues that much of the observed negative impact of food transfers on labor supply is primarily due to targeting errors. He claims that carefully designed and implemented food assistance can play a crucial developmental role. In a randomized evaluation of cash and in-kind (food) transfer in Mexico, Skoufias et al (2008) find no effect on labor market participation of either form of transfers.

Unlike disincentive effects, theoretical prediction of transfer programs on crowding out of private transfers is less univocal. If private transfers are motivated by altruism, public transfers are likely to create crowding out effects. However, the effects are quite ambiguous when the private transfers embed risk-sharing (Cox et al, 1998). Formal transfers can in fact play a complementary role in informal transfers and facilitate risk-sharing. Despite ambiguous predictions, crowding-out is one of the major concerns for any transfer program because of broad regularity in empirical evidences.

Empirical evidences demonstrate the existence of crowding out effect in different magnitudes. One dramatic example is presented in Cox and Jimenez (1995). According to their estimates in 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 20-40% decline in private transfer. Albarran and Attanasio (2005) present the strongest evidence of public safety nets undermining informal support system in Mexico. Ethnographic evidence in Heemskerk et al (2004) shows a qualitative change in informal safety net mechanisms despite a decline in the amount of transfers. Crowding out can be an acceptable cost since reduced private transfer is not a pure wastage (Morduch, 1999) and even desirable if it helps to break poverty traps arising from ethnic or social class solidarity (Hoff and Sen, 2005).

Regardless of the theory and evidence of transfers, food aid has been viewed as one of the key factors in Sudan's development and famine control for almost half a century (Gelsdorf et al, 2007). Keen (2008) argues that famine has deliberately been used as part of political strategy in Sudan by obstructing aid efforts and plundering. Following the peace agreement in 2005, Sudan has become one of the top three countries of World Food Program's (WFP) operation. In 2008, Sudan alone received more than 11% of the total food aid delivered by WFP. While there is hardly any argument against transfers during emergencies, food aid is often criticized for failing to embed any 'exit strategy' even in the context of Sudan (Pantuliano, 2007). More recently, WFP's food assistance in Southern Sudan is being promoted as an instrument of reintegration to enhance "the ability of returnees to secure the political, economic and social conditions to maintain their life, livelihood and dignity" (Bailey and Harragin, 2009).

The predominant form of food aid by WFP in Sudan is general distribution (almost 90% of total food aid). Unlike general food distribution, food-for-training may have greater potential of acting as a developmental tool as it is directly linked to livelihood development. However, our results show that the program did not make any substantial change in the structure of economic activities of the participants. We find about 13% reduction in the income of the participants but no significant reduction in labor supply of the adult members. However, we find a reduction in child labor and about 10 percentage points increase in enrolment of girls. There is also no evidence of crowding out and participation tends to increase transfers given out. Food assistance helped the participants to improve their housing

conditions in terms of increased ownership of homestead (about 9 percentage points) and improvement in house materials. This is consistent with their increase in non-food expenditures. However, no major impact is observed on accumulation of other physical assets.

Following this introduction, the next section gives a brief description of the context, the program and the data. The results are discussed in Section 3. Some robustness checks of the main results with alternative specifications and sampling restrictions are conducted in section 4. Section 5 concludes the paper.

2 Program description and the experimental design

BRAC-Southern Sudan (BRAC-SS) initiated this pilot program called Food-for-training and income generation (FFTIG) in Juba in collaboration with World Food Program (WFP) and Consultative Group to Assist the Poor (CGAP). The program was designed following the structure of the Income Generation for Vulnerable Group Development (IGVGD) program in Bangladesh and took food transfer as the major entry point for a "sustainable" change in the livelihoods of the beneficiaries. The basic premise of the program is that food transfer alone, while critical to meet the immediate needs, fails to generate entrepreneurship among the target population and may lead to long term dependency. Therefore, the key thrust of the program is to combine 'protection' and 'promotion' aspects of safety net. It is expected that combing food transfers with skill development and financial services will enable the households to move into a regular source of income and to build an asset base to cope with minor shocks.

2.1 Brief description of the context

Five decades of civil conflict took a serious toll on the lives and livelihoods of Southern Sudanese. 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 8 million populations. Though things started to improve with the peace agreement in 2005, life in Southern Sudan remains one of the harshest in present world.

There is no representative statistics available for Juba on social indicators. However, the first ever official statistical record in fifty years in Southern Sudan, Statistical Yearbook for Southern Sudan – 2009 (SSCCSE, 2009), shows only a glimpse of this harshness. One in every 10 children in Central Equatoria does not live through the first year of their lives (Table 1). Average life expectancy at birth is only 42 years, which is the lowest in the world. It also has the highest maternal mortality. Although poverty in Southern Sudan cannot easily be measured in economic terms, UNDP (2004) estimates show that over 90 percent of its population is living below 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 is the major economic sector and is the most commonly used indicator of wealth. For example, according to local perceptions, the 'poor' households have 2-5 sheep/goats (shoats) and 2 wives; and the 'middle class' or 'better-off' households have 10-20 shoats and 2-4 wives (WFP, 1999). Other local measures of poverty include the amount of land the household uses to cultivate cassava.

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

annual income of the sample households in 2008 was SDG 595. According to these measures of poverty, the households in our study are clearly worse-off than the general population of Juba, showing effective targeting by the program. About 44% of these households have 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 is self-employment in non-farm sector. This mostly includes primitive 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 are highly scarce, and only 12% female heads were involved in any work for wage in the baseline.

2.2 The intervention

BRAC-SS identified the potential participants, and each participant households received food for 9 month starting from March 30, 2008. The amount of food transferred followed the WFP guideline of 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. The transfers took place at the branch offices, which are mostly within walking distance from the participants' house, to minimize the cost of collection.

From mid April of 2008 the main women of the participant households (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. Though five different IGA trainings were provided, 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 weeklong 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 for 5 days. Though the food aid is designed to allow the households getting engaged in new earning activities, the actual transfers took place irrespective of whether they took up the new activity. In this sense, the food aid was unconditional.

2.3 Participant Selection

The program provided supports to 500 households from 6 branch offices in Juba. We over-selected the number of potential participants to get 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, village elders and local chairmen. Describing 'very poor' in a post-conflict setting such as Juba, with pervasive poverty and a population resettling, is quite challenging. The staffs, therefore, often started by asking the community to identify the poorest households in the village. They stopped collecting names when around 10 households have been identified in a village. In subsequent meetings with other people of the same village, the staffs used the prior list of names as the reference group and asked for any other households whom they consider to be poorer or equally poor.

Once the households list of a village was prepared, the staffs visited each household to collect information of a few simple 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 has 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/hey=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 the staffs at the branch office. The program manager from the country office visited each of the primarily selected households to verify their status and made final selection. The households already participating in microfinance were excluded in the final selection. Out of 1,250 primarily selected households, 1,058 households were finally selected and were eligible to participate in the program.

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 rest 549 households are assigned as 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 after 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). Treatment status does not seem to have significant influence on the likelihood of being interviewed in the follow-up survey though attrited households were 7 percentage points less likely to be in treatment group compared to the households in the panel. Therefore, sample attrition is very unlikely to have a large influence on the results.

However, the actual intervention did not fully comply with the randomized treatment and control group and 14% of the control households in the panel wrongly received intervention and 88% of the treatment group actually participated (Table 2). Confusion with the names of the respondents is one of the major identified 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. However, such misidentification accounts for 20% of the erroneous inclusion/exclusion. There is considerable variation across the branches in the level of 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 six are located the farthest away from the central office, where the program manager was based. In the other 4 branches, non-compliance was very low.

3 Impact results

The fact that there are control households who received food assistance poses a significant challenge in impact estimation. Because of this contamination and non-compliance, we cannot estimate the average treatment effect on the treated (TOT). 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 apparently selectivity 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 analyses, we primarily use households' initial treatment-control status in random assignment to estimate the impact of program participation. To minimize the influence of contamination, we leave all the observations from Jabel Kujur branch out of our analysis.

Table 3 presents a balance check in the baseline of this restricted sample. In this sample, 10.6% of the control households and 91.3% of the treatment households actually participated in the program (first 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). The rest of Table 3 shows the random assignment of treatment and control groups as they are not significantly different in their baseline characteristics. 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).[1] Our base estimation is the standard double-difference

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

where y_{it} is various outcome variables of household *i* in time *t*, $treat_i$ is the dummy for household being assigned for treatment in the randomized experiment, $followup_t$ is dummy for follow-up survey period, X_{ki} is a vector household controls from the baseline and u_{it} is the error term. Coefficient of the interaction treatment and follow-up interaction (β_3) is the impact measure. Subsequently estimation from alternative specification and sample restriction are also presented to check robustness of the base results.

3.1 Income and economic activities

Table 4 presents the regression results for per capita annual income (nominal) and log of per capita income. Baseline characteristics included in the regression of Column 2 and Column 5 are number of household members in different age groups, sex of household head, number of members with disability; sex of household head; age, religion and tribe of the respondents; and variables on migration and incidence of crisis event. In Column 3 and Column 6, branch dummies have been added. Subsequent regressions include all these controls unless specified otherwise.

There is indication of a general decline in per capita income of the households over the one year period. According to monthly CPI of Juba, price declined marginally after September (six months after initiation of food transfers). Furthermore, there has been a surge of returnees from IDP camps during this period. Only about 13% of the returnees come through the official reintegration process and the rest are spontaneous returnees (Bailey and Harragin, 2009). Most of the returnees are flocking into the urban areas. There are reports of violence between the returnees and guests as the unskilled returnees are intensifying pressure on already saturated labor market. Alongside this general decline, we observe a higher reduction in the per capita income for the treatment group. Addition of control variables improves precision of our estimates, and the size of the estimated negative impact remains similar. Participation in the program reduced per capita annual income by SDG 120-130.

However, the negative impact on income does not necessarily display a disincentive effect on labor supply. In fact, we do not observe any significant effect on the total hours spent by the respondents on different earning activities (Table 5). Similarly, there is no apparent disincentive effect for other adult members. Significant effect is observed on both hours worked and earning of children. This is in line with "luxury axiom" of child labor (Basu and Van, 1998), where children's nonwork is a luxury good in the household's consumption.

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[1] Balancing check for the full sample (i.e. not dropping observations from Jabel Kujur branch) yields similar result to that of this restricted sample.

Though statistically insignificant at conventional levels, program effect on income by the respondent (the main earner) is negative. The opposite direction 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. However, we do not observe any major shift in their economic activities (Table 6). As noted earlier, non-farm self-employment (petty trading of different sorts) is the major activity of the respondents and their involvement in cultivation remains minimal. The marginal increase in farm self-employment shows that the objective of the program to increase farming by the training participants did not materialize.

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 reduce work efforts. However, 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).

Finally, if there is any disincentive effect of transfer, the negative impact on income should be higher for wealthier households. Though our sample consists of extremely poor households, we look at the impact of food transfer on income at different quintile (Figure 1). Estimated effect on income is the largest on the 3rd decile and the effect size declines at higher deciles. This is contrary to wealth effect of transfers.

These results strongly indicate that reduction of child labor is the primary reason for the negative effect of the program on per capita income. While this reduction by itself can increase household welfare, a corresponding increase in enrolment would indicate that households are investing in children. Table 7 shows a positive effect of the program on the enrolment of female children.

3.2 Private transfers

Informal mechanisms are known for their importance in risk-sharing until formal safety net programs are developed. In post-conflict contexts such informal insurance mechanisms can be non-existent as conflict and displacement usually reduce informal risk-sharing to smooth consumption (Maria and Andres, 2010). In such situations, external assistance may allow them to invest in rebuilding these networks. However, similar assistance is still likely to crowd out altruistic private transfers.

Not surprisingly, the extents of private transfers (either received or given out) were very low for the sample households in the baseline (Table 8). There has been a significant increase in transfer receipts, and no impact of the program was observed. Absence of crowding out could be attributed to private transfers being very small to begin with. However, there is a positive impact on both likelihood and value of transfer *given out* by the treatment households. The types of transfers indicate that increased transfers came to a greater extent from transfers in-kind rather than cash transfers. This corroborates that the results are an effect of receiving food transfers. The results also indicate that the households start rebuilding these networks relatively quickly. These transfers could 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 receiving and giving out transfers by households, which suggests reciprocity (results not shown).

3.3 Food and non-food expenditure

An important aspect of food assistance to look at is change in their allocation of out-of-pocket expenses on different consumption item. Food consumption data was 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 (Table 9). Any impact on food consumption and reallocation of expenses during the food assistance period cannot be observed from this data. No impact on current per capita food consumption is observed. The negative impact on income and prior food assistance washes out the effects on food consumption. 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 small on average.

The non-food consumption items are divided into monthly and annual expenditure. Monthly items include transportation, fuel, toiletries etc. and annual item 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 per capita expenditure on monthly non-food items. In terms of specific items, a small positive impact is observed in expenditure on toiletries. In the annual items, no significant impact is observed for any particular items included in the survey though there was a marginally positive impact on ceremonial expenses (marriage/funeral). However, this impact does not come from an increase in expenses for the treatment households but from a lower rate of reduction over the year compared to the control households. The positive coefficient for impact on education expenses is imprecisely estimated and not significant. Estimated effect on total expenses on annual non-food items was SDG 117.

The likelihood of a household's borrowing increased because of participation in the program as microcredit is part of the intervention package. With increased borrowing, likelihood of having savings at home has declined for the participants. Therefore, the food transfer does not seem to have affected liquid savings.

3.4 Housing and 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 are a few evidences 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 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 10). This is expected given the poor housing status in the baseline. There are also some positive effects of food assistance 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 about 5% households had electricity connection in the baseline and there is a small positive effect.

There is no major impact on different types of other household assets (Table 11). Program participation did not necessarily lead to any significant accumulation of physical assets. Significant positive effects, albeit relatively small, are observed on probability of ownership of a shade for livestock, electric fans and insecticide treated nets. 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.

4 Robustness checks

In this section, we discuss several concerns, which could cast doubt on the results; robustness is verified with alternative specification and sample restriction. Table 12 compares estimates of effect sizes discussed in the last section with that by three alternative specifications. In column 2, actual recipients of food assistance instead of their 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 regression are presented. In this estimate, the dependent variables in the second stage are the changes in the variables from baseline to follow-up.

$$y_{it} - y_{it-1} = \beta_0 + \beta_1 \widehat{part_i} + \delta_k X_i + u_{it} \qquad (2)$$

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.

$$part_i = \alpha_0 + \alpha_1 treat_i + \gamma_k X_i + e_{it} \qquad (3)$$

These results also show similar, sometimes slightly larger, effects. 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. If there is indeed 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.

$$y_{it} = \beta_0 + \beta_1 followup_t + \beta_2 density_i + \beta_3 density * followup + \delta_k X_i + u_{it} \qquad (4)$$

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 13, different distance cut-offs (25, 50 and 100 meters) have been used. Treatment density does not show any significant association with the change in income of the control households.

Similarly, there is no spillover in terms of probability of transfers received. 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.

5 Conclusion and policy discussion

Food assistance is one of the most common forms of safety net programs in post conflict situations. Besides the humanitarian objective, it is often believed that such assistance may work as a livelihood promotional mechanism by providing breathing room for the poor. However, skepticism of food assistance is also quite widespread on the grounds that they can create a disincentive to work among the participants and also can crowd out private transfers. This paper investigates both these issues from randomized evaluation of a food-for-training program implemented by BRAC in Southern Sudan.

While there is no evidence of disincentive effects, we find a significant negative impact on household income. However, it is very important to note that the major channel trough which this effect takes place is by reducing child labor in the households. There is also a small positive effect on school enrolment of girls and an improvement in housing status. We also do not find any indication of crowding out. This could be because of the fact that extent of receiving private transfers were very limited to start with. However, there is small but significant impact of transfers given out by the participants. Overall, the program does not appear to have any major developmental effect over the short run. These findings come with the primary caveat that this is an evaluation of a particular, and we did not estimate the elasticity of work to non-labor earnings. Moreover, the impact assessment focused on employment and other objectives of food aid (e.g. nutritional impact) were not evaluated.

BRAC has been experimenting with food transfer (Income Generation for Vulnerable Group Development-IGVGD program) as an indirect method of building assets and enterprises for the very poor in Bangladesh since mid 80s. Evaluation of that IGVGD program showed significant short-term change but much of the gains were not sustained. For example, percentage of beneficiary households with a monthly income of more than Tk. 300 increased from a mere 7% to 64% between 1994 (pre-program) and 1996 (end program), and declined to 31% by 1999 (3 years from the end of program supports) (Hashemi and Hossain, 2001). Lessons learnt from IGVGD led BRAC to design Challenging the Frontier of Poverty Reduction (CFPR) as an intervention to directly build assets and entrepreneurship (Matin and Hulme, 2003). There is growing evidence of the program's ability to successfully transform lives of the poorest, and this initiative is being adapted in a number of developing countries.

In regards to the cost of the program in Southern Sudan, total amount of transfer is 198 MT of cereal and 56 MT of other food items. According to WFP, the costs of getting these foods in Juba are USD 773 for wheat (in May 2008). The cost of distributed food is USD 192,000 with per household cost of USD 384. Including the cost of transportation, training and staffs per beneficiary cost is over 425 dollars. Per beneficiary cost in CFPR, based on the budget figures, is approximately 350 dollars. Since there is a significant price difference between the two countries, costs need to be converted into PPP dollars. Based on this conversion, per beneficiary expenditures are USD 785 for food-for-training in Southern Sudan and USD 975 for CFPR in Bangladesh[2]. Though CFPR is about 25% more expensive than the food-for-training in Southern Sudan, the potential benefits and past experiences indicate the need for piloting the asset transfer approach in Sudan to achieve longer-term promotional impact. This does not refute the effectiveness of food assistance as short term protective intervention.

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[2] This costing exercise is only to get a rough idea of the relative cost of the two. In calculating cost of FFT, an average cost of non-cereal food items at USD 700/mt was used. The ratio of exchange rate to PPP conversion rate was 2.79 for Bangladesh and 1.75 in Sudan.

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Table 1. Selected social indicators for Southern Sudan

Social indicators	Central Equatoria in Southern 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; [b] World Bank online Database

Table 2. Compliance with RCT design

	Full sample	Branch 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
Received food transfers (1=yes)	0.807	(0.021)***	0.106	(0.015)***	814
Household size	0.066	(0.169)	5.377	(0.123)***	813
Number of children (<15 years old)	0.009	(0.118)	1.853	(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

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

Table 4. Impact on household income

	Per capita income			Ln(per capita income)		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	25.10	14.27	14.32	0.14	0.12	0.13
(1=Yes, 0=Control)	(50.20)	(47.51)	(47.56)	(0.09)	(0.09)	(0.09)
Follow-up	-70.36	-67.57	-53.17	-0.185	-0.18	-0.15
(1=2009, 0=2008)	(51.79)	(49.84)	(49.70)	(0.10)*	(0.10)*	(0.10)
Treatment X follow-up	**-118.57**	**-120.63**	**-130.69**	**-0.264**	**-0.27**	**-0.29**
	(67.88)*	**(65.57)***	**(64.64)****	**(0.14)***	**(0.14)****	**(0.13)****
Constant	582.59	635.93	609.82	5.669	5.83	5.73
	(36.34)***	(115.18)***	(117.38)***	(0.07)***	(0.24)***	(0.24)***
Baseline characteristics	-	Yes	Yes	-	Yes	Yes
Branch dummies	-	-	Yes	-	-	Yes
Observations	1,434	1,428	1,428	1,434	1,428	1,428
R-squared	0.01	0.09	0.11	0.02	0.07	0.10

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

Table 5. Engagement in earning activities by the household members

	Respondent		Other adult		Children	
	Hour	Income	Hour	Income	Hour	Income
Treatment	-31.17	180.82	-120.14	-253.19	107.40	196.48
(1=Yes, 0=Control)	(85.82)	(109.62)*	(107.14)	(154.71)	(37.43)***	(57.66)***
Follow-up	79.48	-34.46	-348.65	-452.51	-35.64	-54.16
(1=2009, 0=2008)	(88.44)	(104.79)	(98.02)***	(150.69)***	(20.70)*	(26.44)**
Treatment X follow-up	**72.63**	**-184.12**	**32.03**	**164.38**	**-97.51**	**-143.20**
	(127.22)	**(151.34)**	**(139.14)**	**(217.53)**	**(41.39)****	**(64.89)****
Constant	1,229.76	591.47	-228.42	-480.71	32.20	109.88
	(246.14)***	(255.55)**	(249.28)	(373.26)	(80.89)	(126.14)
Observations	1,618	1,618	1,618	1,618	1,618	1,618
R-squared	0.11	0.06	0.12	0.14	0.05	0.07

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	4.02	-14.48	-13.05	-7.66	0.15
(1=Yes, 0=Control)	(41.38)	(14.78)	(83.42)	(26.38)	(0.15)
Follow-up	-87.16	14.65	110.62	41.37	-0.48
(1=2009, 0=2008)	(38.65)**	(17.54)	(85.81)	(30.49)	(0.13)***
Treatment X follow-up	**2.56**	**33.92**	**45.39**	**-9.24**	**-0.16**
	(53.03)	**(28.34)**	**(123.73)**	**(40.70)**	**(0.19)**
Constant	190.13	44.66	824.90	170.07	1.46
	(109.96)*	(38.24)	(236.42)***	(89.71)*	(0.37)***
Observations	1,618	1,618	1,618	1,618	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%

Table 7. 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 8. Impact on informal 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	0.01	10.00	-0.01	-9.74	-0.01	0.00
(1=Yes, 0=Control)	(0.02)	(8.04)	(0.01)	(4.51)**	(0.01)	(0.00)
Follow-up	0.04	14.40	0.02	-6.05	0.00	0.02
(1=2009, 0=2008)	(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 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	18.12	-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	17.98	228.09	-0.01	0.46
	(2.67)***	(23.08)	(103.99)**	(0.04)	(0.08)***
Observations	1,613	1,618	1,613	1,618	1,618
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. Impact on housing

	Own homestead	Wall made of mud-pole	Earthen floor	Number of rooms	Access to safe water	Electricity connection
Treatment	-0.02	0.02	0.07	-0.12	-0.02	-0.02
(1=Yes, 0=Control)	(0.03)	(0.03)	(0.02)***	(0.08)	(0.02)	(0.01)
Follow-up	0.07	-0.10	-0.05	0.54	0.09	-0.01
(1=2009, 0=2008)	(0.03)**	(0.03)***	(0.03)**	(0.08)***	(0.02)***	(0.01)
Treatment X follow-up	**0.08**	**-0.09**	**-0.03**	**0.18**	**0.04**	**0.03**
	(0.04)**	**(0.05)***	**(0.03)**	**(0.11)**	**(0.03)**	**(0.02)***
Constant	0.59	0.55	0.95	1.03	0.06	0.05
	(0.08)***	(0.09)***	(0.06)***	(0.19)***	(0.06)	(0.04)
Observations	1,618	1,601	1,590	1,520	1,604	1,586
R-squared	0.13	0.10	0.12	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 11. 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 12. Impact estimates with alternative identification

Dependent variable	Treatment in RCT (1)		Actual treatment (2)		Excluding Munuki branch (3)		IV regression (4)	
Log (per capita income)	-0.29	(0.13)**	-0.23	(0.13)*	-0.32	(0.16)**	-0.40	(0.18)**
Hours worked by respondent	72.63	(127.22)	-1.86	(127.22)	27.82	(139.92)	71.29	(151.69)
Hours worked by other adult members	32.03	(139.14)	89.49	(139.05)	13.14	(149.76)	40.65	(162.67)
Hours worked by children	-97.51	(41.39)**	-86.22	(40.92)**	-63.73	(40.84)	-111.87	(51.43)**
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)	11.30	(14.16)
Per capita non-food expenditure (last year)	116.83	(61.53)*	144.97	(60.76)**	179.56	(75.49)**	146.01	(73.22)**
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)**	18.52	(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 branch). F-statistics of first stage is 122.33

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

Table 13. 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	-0.15	-0.21	-0.19	0.069	0.065	0.041
(1=2009, 0=2008)	(0.13)	(0.14)	(0.15)	(0.028)**	(0.030)**	(0.033)
Treatment density	-0.07	-0.03	0.00	0.005	-0.000	-0.003
	(0.06)	(0.04)	(0.02)	(0.015)	(0.008)	(0.004)
Follow-up * Treatment density	0.12	0.07	0.02	-0.007	-0.001	0.004
	(0.11)	(0.05)	(0.02)	(0.020)	(0.010)	(0.005)
Constant	6.23	6.23	6.16	-0.053	-0.049	-0.035
	(0.40)***	(0.40)***	(0.41)***	(0.081)	(0.083)	(0.085)
Observations	557	557	557	649	649	649
R-squared	0.11	0.11	0.11	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 and 100) meter radius.

Figure 1. Quantile regression of log of per capita income
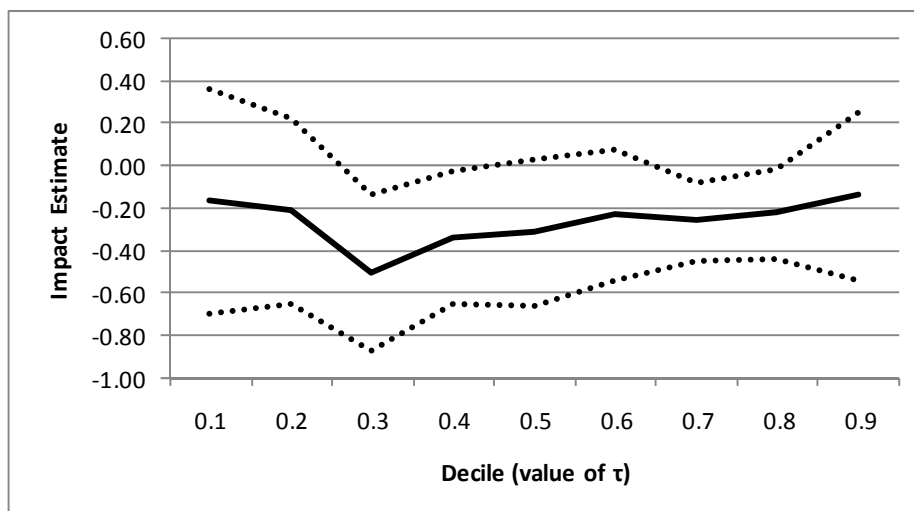
Annex 1. Sample attrition

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

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

Note: Sample includes households assigned as control in RCT.
Robust standard error in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%