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Article

Poverty Alleviation and Interhousehold Transfers: Evidence from BRAC's Graduation Program in Bangladesh

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Abstract

Poor households often rely on transfers from their social networks for consumption smoothing, yet there is limited evidence on how antipoverty programs affect informal transfers. This paper exploits the randomized rollout of BRAC's ultra-poor graduation program in Bangladesh and panel data covering over 21,000 households over seven years to study the program's effects on interhousehold transfers. The program crowds out informal transfers received by the targeted households, but this is driven mainly by outside-village transfers. Treated ultra-poor households become more likely to both give and receive transfers to/from wealthier households within their village; and less likely to receive transfers from their employers. As a result, the reciprocity of their within-village transfers increases. The findings imply that, within rural communities, there is positive assortative matching by socio-economic status. A reduction in poverty enables households to engage more in reciprocal transfer arrangements and lowers the interlinkage of their labor with informal insurance.

JEL classification: J43, I38, I32

Keywords: poverty, interhousehold transfers, social networks

1. Introduction

In rural parts of developing countries, individuals are faced with substantial risks and often rely on transfers from their social networks for consumption-smoothing (Rosenzweig 1988; Udry 1994); yet there is limited evidence on how large-scale antipoverty programs affect interhousehold transfers. This paper provides evidence on the causal effect of a large-scale poverty alleviation program on informal transfers within rural Bangladeshi communities. Implemented by the NGO BRAC, the "Targeting the Ultra Poor"

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program (henceforth "the program") targets the poorest households in rural communities in Bangladesh and provides them with a transfer of productive assets, training, and other support services. Bandiera et al. (2017) show that the program leads to long-run improvements in the socio-economic conditions of beneficiary households. Similar programs targeting ultra-poor households have been evaluated in other contexts and, by and large, have found positive effects on socio-economic conditions of the targeted households (Banerjee et al. 2015, 2016, 2018; Blattman et al. 2016; Collins and Ligon 2017; Bedoya et al. 2019; Brune et al. 2020), but to date, we have limited evidence on these programs' effects on informal transfers. In this paper, I exploit the randomized rollout of the program and panel data covering over 21,000 households over seven years to study the program's effects on interhousehold transfers.

In order to evaluate the effects of the program, potential beneficiary households (henceforth referred to as the "ultra poor") were identified by the program in 40 BRAC branch offices covering 1,309 villages. Within every village, BRAC conducted participatory wealth appraisals whereby community members assigned a wealth class for each household: poor, middle, or upper class. Following this community appraisal, BRAC program officers identified eligible "ultra-poor" households among the poor.[1] A baseline survey was conducted on all of the ultra-poor households, as well as a representative sample of the rest of the community. Importantly, the survey included questions on informal transfer networks of respondents' households. Using the baseline village census, the identity of any household with whom the respondent's household interacted within the village was recorded. After the baseline survey, the program was randomly introduced in 20 branch offices in 2007, and the other 20 remained as controls until 2011. The same sample of households were resurveyed in 2009 and 2011, after which the control communities were also treated. An additional follow-up survey was conducted in 2014, that is, seven years after the baseline and three years after the control branches had also been treated, and I will use this data to examine long-run trends of outcomes for early- versus late-treated ultra-poor households.

I find that the program crowded out informal transfers received by the ultra-poor households in treatment villages, but this was driven mainly by outside-village transfers. The ultra poor in treatment villages were 13 percentage points (ppt) less likely to have received informal transfers (during the 12 months preceding the survey) relative to the control group, while the effect on the intensive margin is imprecisely estimated. When I disaggregate the transfers by location, I find that they were 4 ppt less likely to have received transfers from within their village but the value of transfers received were 29 percent higher relative to the control group. Moreover, they were more likely to report receiving food transfers from other households within the village. On the other hand, outside village transfers were crowded out by 12 ppt on the extensive margin and 23 percent in value.

Examining who the ultra-poor households exchange transfers with, I find that the program increased their likelihood to receive as well as give transfers from/to wealthier households within their village. Here, as a proxy for wealth, I use the socio-economic wealth rank assigned to each household at baseline, during the community wealth appraisal. I find that after the program the likelihood that the ultra poor in treatment villages give transfers to other poor, middle, and upper class households increased. Moreover, middle and upper class households were more likely to report giving transfers to the ultra poor in their communities, and this effect is maintained seven years after the baseline. As a result of these changes, the reciprocity of ultra-poor households' transfer arrangements with higher socio-economic classes increased significantly. In particular, the reciprocity of ultra-poor households' transfer links with middle class households increased by 9 ppt (19 percent relative to control) and by 12 ppt (44 percent) with upper class households.

Finally, I find that the program reduced the likelihood that ultra-poor households receive transfers from their employers. Ultra-poor households in treatment communities were 4 ppt less likely to report an

1 Households who were in the poor class according to the community wealth appraisal, but not selected as potential beneficiaries of the program by BRAC, are referred to as the "other poor" households throughout the paper.

employer as a source of transfers—a large effect considering that in the control group only 9 percent of ultra-poor households reported an employer as a source of transfers. To understand whether this effect was driven by a general shift away from wage-labor, I restrict the sample to ultra-poor households who were engaged in wage-employment at baseline as well as at the two follow-up surveys, and I find that they were also less likely to receive transfers from their employers. While this analysis has a caveat (since it is based on a selected subsample), it nevertheless suggests that the effect is not only due to a shift away from wage employment. Overall, the findings suggest that there may be a causal relationship between poverty and alternative forms of reciprocity in informal insurance networks: poor households may reciprocate transfers from wealthier members of the community by selling their labor cheaply to them, while less poor households may reciprocate with transfers of their own.

The paper contributes to the literature on informal insurance in village economies in a number of ways. First, it is related to the literature on risk-sharing through reciprocal transfers (Townsend 1994; Udry 1994). This literature has demonstrated that risk-sharing networks are likely to be concentrated along dimensions of proximity such as kinship, caste, or geographic proximity.[2] What has been less studied is whether poverty has a causal effect on whom agents are matched with in risk-sharing networks. Previous empirical studies have exploited cross-sectional variation or lab experiments to shed light on this.[3] This study contributes by showing that an exogenous improvement in the socio-economic status of the poorest households in a village (caused by an antipoverty program) increases their engagement in reciprocal transfers with wealthier households within the community. To the best of my knowledge, this is the first study to demonstrate the causal effect of socio-economic status on matching in reciprocal transfer links.

The paper also contributes to the literature on crowding out of informal transfers by public transfers. Early empirical studies have demonstrated a modest negative correlation between household income and transfers received (Cox and Jakubson 1995; Altonji, Hayashi, and Kotlikoff 1997)—although the relationship is likely to be heterogenous and non-linear (Cox 1987; Schoeni 1996; Cox, Hansen, and Jimenez 2004).[4] The program I study leads to sustained long-term improvements in the socio-economic conditions

2 Theoretical reasons for this have been studied by Genicot and Ray (2003); LaFerrara (2003); Bramoullé and Kranton (2007); Bloch, Genicot, and Ray (2008); Ambrus, Mobius, and Szeidl (2014). Empirically, this point has been demonstrated by Fafchamps and Lund (2003); LaFerrara (2003); Dercon et al. (2006); Fafchamps and Gubert (2007); Munshi and Rosenzweig (2016); Angelucci, de Giorgi, and Rasul (2017); Chandrasekhar, Kinnan, and Larreguy (2018).

3 Jalan and Ravallion (1999) show that poorer households in rural China are less well insured against fluctuations in their income. Fafchamps and Gubert (2007) show that the wealth difference between two households is negatively correlated with having a risk-sharing connection in the Philippines, while DeWeerdt (2004) finds the opposite relationship in Tanzania. Schechter and Yuskavage (2011a,b) highlight that the failure to distinguish between reciprocated and unreciprocated links could be one reason for these conflicting findings. They show, using survey data form Paraguay, that reciprocated links are more likely to exist between households who have similar wealth or live in close proximity, while unreciprocated transfers links are more likely between households of different wealth or education levels. Attanasio et al. (2012) show experimentally that among close friends and relatives, individuals with similar risk attitudes group together for risk-sharing. Ligon and Schechter (2012) also use an experimental approach and show that transfers within the experiment are more likely to be motivated by reciprocity (as opposed to other-regarding preferences) when individuals are connected (and exchange transfers) outside of the experiment. Ado and Kurosaki (2014) replicate the same methodology in a different context and find similarly that incentive-related motives (reciprocity and sanction aversion) are more likely to be associated with sharing in the real world.

4 Jensen (2003) and Juarez (2009) find large crowding out effects of increases in public transfers targeted to the elderly in South Africa and Mexico respectively. Albarran and Attanasio (2002) find that beneficiaries of a conditional cash transfer program in Mexico (Progresa) were less likely to receive informal transfers, and conditional on receiving any, they received lower amounts of transfers. On the other hand, Angelucci and de Giorgi (2009) provide evidence that households eligible for Mexico's Progresa program increased transfers to ineligible households residing in treatment villages. Walker (2017) shows that a one-time unconditional cash transfer in Kenya did not lead to any change in transfer giving to family and friends among recipient households in the short run.

of targeted poor households through a one-off transfer of assets and skills. I show that while the program crowds out informal transfers received by the targeted poor households, the effect is relatively small and varies based on geographic proximity. While transfers from outside the village are crowded out, transfers from households living in the same village as the targeted households are not. This suggests that transfers within and outside the local community are driven by different motives. In particular, the findings are in line with within-community transfers being driven by incentive-related motives (such as reciprocal exchange), as opposed to preference-related motives (Leider et al. 2009; Ligon and Schechter 2012).

Finally, the paper is related to the literature on interlinkage in rural labor markets.[5] Previous empirical work has studied interlinkages between trade and financial markets in developing countries (McMillan and Woodruff 1999; Casaburi and Macchiavello 2019; Casaburi and Reed 2020), but empirical work on interlinkage in the rural labor market has been largely descriptive (Bardhan and Rudra 1978, 1981; Anderson 1990). This paper contributes to the literature by showing that the poverty level of the worker may have a causal effect on interlinkage of contracts in the rural labor market with informal insurance arrangements. Importantly, due to the nature of the particular antipoverty program I study (it is designed to increase self-employment among the targeted poor households), the effects may in part be due to the fall in wage-labor caused by the program. It is unclear whether a reduction in poverty caused by another type of intervention would have a similar effect on the interlinkage of employment and transfers.

2. "Targeting the Ultra Poor" Program

The TUP (Targeting the Ultra Poor) program is a multi-faceted intervention with the central aim of lifting households out of extreme poverty. The main beneficiaries of the program are adult women, and to improve their socio-economic conditions, the program combines the transfer of productive assets with complementary training, a cash stipend, and other supporting services. This section describes the selection procedure of the beneficiaries and the details of the program.

2.1. Targeting

In order to identify the potential beneficiaries of the program, BRAC proceeds in four steps. First, BRAC officials in Dhaka select the BRAC branch offices where the program would be implemented. Then BRAC officials at the branches identify which communities would be targeted. In the third step, program officials organize a participatory wealth ranking in each community selected for the program. This exercise places all households into one of several wealth bins corresponding to the poor, middle, and upper classes.[6] Finally, BRAC officers verify which households among the poor satisfy pre-determined inclusion and exclusion criteria. Based on these criteria, they subdivide the poor (as determined by the community appraisal) into "ultra poor," who are eligible for the TUP program, and the rest ("other poor").[7] Eligibility

5 This literature has demonstrated that employers may provide assistance to their poorer workers in times of need, through the provision of consumption loans or transfers (Platteau and Abraham 1987; Platteau 1995a,b). Theoretically, workers with limited outside options may prefer to maintain a long-term relationship with an employer in order to have smoother earnings across the agricultural seasons, even though this may end up providing them with a lower wage relative to casual labor contracts available on the spot labor market (Bardhan 1983; Mukherjee and Ray 1995; Caselli 1997).

6 This procedure is similar to community appraisal methods studied by Alatas et al. (2012) in Indonesia.

7 In particular, the program has three exclusion criteria, all of which are binding. Households who are borrowing from a microfinance institution, who are recipients of any government benefits, or who do not have any able-to-work adult female members are excluded from the program. To be selected, a household has to satisfy at least three of the following five inclusion criteria: (a) total land owned including homestead is no more than 10 decimals (approximately 0.1 acre); (b) there is no adult male income earner in the household; (c) adult women in the household work outside the homestead; (d) school-going-aged children are working in an income-generating activity; and (e) the household has no productive assets.

is determined at baseline, and is not reevaluated within treatment villages over time. The share of treated households relative to the average village population was 6 percent.[8]

2.2. Program Components

The first component of the program is asset transfer. Assets are chosen by the beneficiaries from a menu offered by BRAC. The menu includes assets related to various income-generating activities (such as livestock rearing, vegetable cultivation, setting up small retail shops, production of small crafts) but almost all eligible women in our sample opted for livestock rearing. For this, beneficiaries had the option to choose between six different livestock packages containing either one or two animal types (e.g., only cows or a cow and five goats), and all packages were on average of similar value of BDT 9,500 (Bangladeshi takas; approximately USD 514 in PPP terms) in 2007. BRAC encouraged program recipients to commit to retain the assets for two years, although this commitment was not strictly enforceable. After two years, beneficiaries were under no obligation or no encouragement to retain the livestock asset.

The second key component of the program is skills transfer. In this, the program aims to teach the beneficiaries skills that are complementary to the transferred livestock asset, such as maintaining livestock health, best-practices related to feeding the animals, insemination to produce offspring and milk, rearing calves, and bringing outputs to the market. The initial skills transfer was conducted through classroom training at BRAC regional offices. This was followed with regular assistance and coaching by a livestock specialist and program officers, as well as refresher training that took place periodically to strengthen the initial technical training. During refresher training and visits, further knowledge is delivered to participants, their performance is monitored, and various challenges are discussed; this is followed by troubleshooting. As part of this, every beneficiary was visited by a livestock specialist every one to two months for the first year, and by BRAC program officers weekly for the first two years of the program.

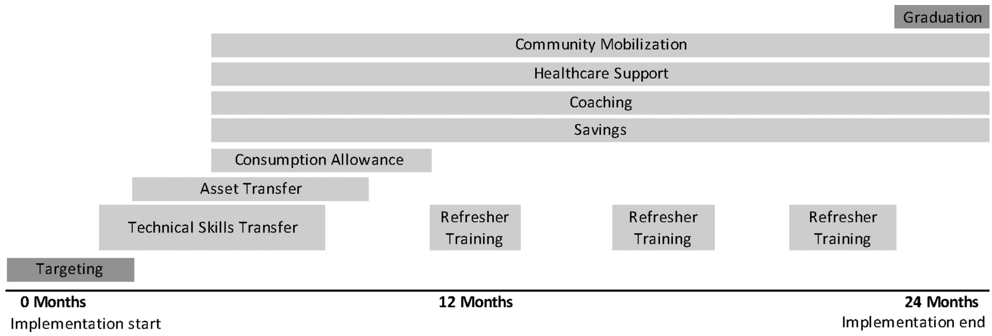
In addition to asset and skills transfer, the program entails a number of supporting services. Program officers organize weekly meetings which bring program beneficiaries from the same village together with one another, and they conduct life-skills awareness sessions. The objective of these sessions is to sensitize participants and to make them more aware of their legal and political rights. The topics covered during these visits include health issues, such as waterborne diseases, de-worming, food, nutrition and anaemia, family planning, immunization, and social issues such as child marriage, dowry, domestic violence, marriage registration, and the importance of children's education.

During their weekly visits, program officers also distribute weekly cash stipends to the program beneficiaries. This is meant to alleviate any short-run fall in earnings due to the occupational change induced by the program and to provide consumption support for ultra-poor households. The exact amount of the stipend varies across regions, but it is meant to be equivalent to half a day's wage for casual laborers (BRAC 2016). These cash transfers typically last for the first 40 weeks after the asset transfer. To ensure proper utilization of this stipend, beneficiaries are also provided with diet charts that indicate affordable and nutrient-rich food items available in their localities.

Another component of the program is to promote savings behavior. To achieve this, program beneficiaries are encouraged to save a portion of their monthly income in BRAC's savings account. The idea behind this is to help the ultra-poor households start building financial security for further enterprise expansion and to lower their vulnerability to shocks. The program officers collect savings during the weekly meetings they organize for the program members.

8 Eligibility is determined at the household, rather than at the individual, level. The program targets the leading woman (female head of household) in eligible households as the person in charge of the assets transferred and to receive the training. If there are multiple female members in the household, then it is up to the household to decide which female member should be the main point of contact with the program officers. This is not to say that other household members do not benefit from the program indirectly, but in terms of the program's operations, the main beneficiary is the female head of the household.

Figure 1. BRAC's Ultra-Poor Graduation Program—Timeline



Source: Adapted from BRAC (2016).
Note: The figure shows the typical timeline of BRAC's graduation program in Bangladesh. The specific timeline may vary depending on the local conditions, as well as beneficiary requirements.

The program also provides health support through BRAC's community-based health workers. Beneficiaries receive guidance about preventative care, treatment, antenatal and prenatal care, contraception, and childcare, as well as financial assistance for surgical cases. In some areas, BRAC also implements public health interventions such as latrine and tube-well installation.

Finally, in order to strengthen the social networks of the ultra-poor households, BRAC establishes local committees called *Gram Daridro Bimochon* (Village Poverty Alleviation) Committees (henceforth "GDBC"). Each committee is comprised of representatives of the program beneficiaries (i.e., ultra-poor women) and community leaders such as wealthier landlords, teachers, priests, local politicians. The GDBC meets monthly and discusses any issues raised by the representatives of the program beneficiaries with the aim of helping them build social networks and leverage community ties. The committee is meant to help participants protect their assets, facilitate access to government services, and offer support in times of need by convening local community support.

The implementation of the components described above lasts for a maximum period of 24 months. After this period, the beneficiary households are "graduated" from the program. Upon graduation, they are invited to participate in BRAC's microfinance program, with the idea that access to microcredit can enable them to maintain and expand their business activities, but they are under no obligation to borrow from BRAC microfinance.

Figure 1 provides a visual summary of the main components of the program and the timeline of its implementation.

3. Conceptual Framework

We are interested in understanding whether and, if so, how a multi-faceted antipoverty program such as the TUP program may affect the informal transfer arrangements of beneficiary households. The program entails many components and there are multiple ways through which it may impact both the supply of informal transfers to the targeted ultra-poor households and the demand for such transfers by them. In this section, I discuss how the various components of the program may affect both the level of transfers received/given by ultra-poor households and who they interact with in transfer networks.

First, the asset and cash transfer components of the program may affect interhousehold transfers received/given by the ultra-poor households. A large literature in public economics discusses alternative mechanisms through which formal transfer programs may crowd out private (informal) transfers, depending on the motivations driving informal transfers. If they are driven by pure altruism (Becker 1974), formal transfers would unambiguously crowd out informal transfers (Barro 1974). Alternatively, informal transfers may take place in exchange for a service provided by the recipient (Bernheim, Shleifer, and Summers 1985; Cox 1987) or in anticipation of reciprocal future transfers (Kranton 1996). In that case, the effect of an increase in the number of formal transfers on the number of informal transfers received is ambiguous.[9] If informal transfers are part of informal insurance arrangements between agents, an increase in formal transfers received by an agent will partly be undone by transfers to insurance partners of the agent.[10]

Second, the program enables the poor to develop new sources of income (mainly livestock rearing) and lowers their dependence on seasonal casual labor.[11] This may improve their resilience to shocks (both by increasing the level of income and by lowering seasonality of earnings) and lower their need for informal insurance.

Third, the program provides health support to the beneficiary households. This may partially lower the need for informal insurance (and lower transfers) both by lowering the risk of health shocks and by enabling treated households to rely on BRAC's health services rather than receiving help from their social networks when faced with a health shock.

Fourth, the program encourages and facilitates saving behavior among the targeted ultra-poor households. These savings may act as an alternative source of insurance, thus lowering the poor's demand for informal insurance and crowd out their interhousehold transfers (Chandrasekhar, Kinnan, and Larreguy 2011; Flory 2018).

Fifth, a unique aspect of the program is the creation of committees that bring together the village elite with representatives of the ultra-poor households (GDBCs). One of the central aims of GDBCs is to encourage the local elites to help vulnerable ultra-poor households in times of need. This may lead to more transfers being funneled from the elites towards the ultra-poor households, thus lowering any crowding out effect that may occur due to the other components of the program.

Sixth, once the initial two years of the program are over and the ultra-poor households graduate, they are invited to participate in BRAC's mainstream microfinance program. Participation in microcredit has an ambiguous effect on participation in informal transfer networks. On the one hand, participation in microfinance group meetings may lead to creation of new social networks, increasing risk-sharing and transfers among the poor (Feigenberg, Field, and Pande 2013). On the other hand, the standard microcredit contract that BRAC offers is rather restrictive, with regular loan repayments (Battaglia, Gulesci, and Madestam 2019) that may help shield the targeted poor from kinship taxes in the form of demands for transfers from their social networks (Baland, Guirkinger, and Mali 2011).

9 Cox, Eser, and Jimenez (1998) show that if transfers occur as a result of bargaining between the two parties, where the recipient provides some services to the donor in exchange for the transfer he receives, then conditional on receiving a positive transfer, an increase in the recipient's income would lead to an increase in his outside option and thus may result in an increase in the informal transfer he receives. Cox, Hansen, and Jimenez (2004) show that the combination of altruism and exchange motives may result in a non-linear response to an increase in the income of the recipient.

10 Under perfect risk-sharing, any increase in the resources available to an agent will enter the resource pool shared with his/her insurance partners and will increase the informal transfers given by the agent. Therefore, if the agent was a net recipient of informal transfers ex ante, the increase in formal transfers they receive would lead to a decrease in the amount of informal transfers they receive. Generalizing the perfect risk-sharing model to allow for imperfect insurance due to, for instance, imperfect enforceability (Coate and Ravallion 1993; Ligon, Thomas, and Worrall 2002) or asymmetric information (Ligon 1998) would yield similar predictions, although the mechanism at work may differ. Attanasio and Rull (2000) show that, under imperfect enforceability, introduction of unconditional formal transfers in a situation where agents have very high marginal utility of consumption will induce a reduction in the amount of equilibrium risk-sharing, which implies a lower level of informal transfers for any given income shock.

11 See Bandiera et al. (2017) for a thorough discussion of the types of jobs available to the ultra poor in this context and the seasonality associated with them.

The program may also affect *who* the ultra-poor households receive transfers from or give transfers to. Previous work has shown that the program leads to long-term improvements in the socio-economic status of ultra-poor households (Bandiera et al. 2017). This may affect who ultra-poor households are matched with in transfer networks (Genicot 2006; Munshi and Rosenzweig 2016). Moreover, an important aspect of the program is to create community networks and community support for ultra-poor households through (a) increasing the interactions among ultra-poor households and (b) the establishment of GDBCs that bring together the village elite with representatives of the ultra poor. Both of these features of the program may lead to the creation of new transfer networks and/or weakening of existing ties.

An alternative source of transfers for the rural poor may be their employers. An eminent literature in development economics studies interlinked labor and patronage relationships in village economies (Bardhan 1983; Mukherjee and Ray 1995; Platteau 1995a,b). When faced with a negative income shock, poor workers may need a transfer or a loan from their employers. This demand for informal insurance may affect the terms of the labor contract, generating an interlinkage between the labor and insurance markets.[12] The program, by improving the outside option(s) of the poorest workers may enable them to transition from interlinked labor to casual labor and/or self-employment. Thus, the program may reduce the likelihood of their being in receipt of transfers from their employers.

4. Data Description

The dataset I use was collected in order to evaluate BRAC's TUP program in Bangladesh. The data covers 1,409 communities, with the average community consisting of 90 households (387 individuals).[13] At baseline, an initial census of all households was carried out in every community, covering 126,810 households. This census collected information on the identity as well as the wealth, occupation, education, and demographic characteristics of all the households living in these communities at baseline.

Following the census, a detailed household questionnaire was carried out on a representative sample of households. For the sampling, census data was combined with information on households' socio-economic statuses from the community wealth appraisal BRAC conducted as part of their procedure of identifying the ultra-poor households (see the section on the program "Targeting the Ultra Poor"). The sample for the household survey included all (ultra and other) poor households and a random sample of the rest of the community. This corresponds to 7,953 ultra-poor households and an additional 19,012 non-ultra-poor households. Households in this sample were surveyed at baseline (2007), at midline (2009), and endline (2011). As detailed in the section on the program "Targeting the Ultra Poor," the implementation of the program lasts at most two years—less for some of its components such as the subsistence allowance, which lasts for a maximum of 40 weeks. This implies that by the time the midline survey was conducted, the program implementation was nearly completed in many communities, and by the endline survey it had been completed in all communities for at least one year. An additional follow-up survey was conducted in 2014 and I will use this data to examine the long-run effects of the program for early versus late-treated ultra-poor households.

The household survey measured a number of individual outcomes, including occupational choices, labor supply, income, social and economic networks of the household. The main survey modules were directed towards the main female in the household, who is the intended beneficiary in ultra-poor households. In cases where the main female was different from the household head, the head of the household was also surveyed for the business activities and land modules. To capture the social and economic networks of the household, respondents were asked to list households they interacted with for each of the

12 More generally, the idea that a risk-neutral employer may provide a risk-averse worker with insurance against income fluctuations is not limited to the rural labor market only (Knight 1921; Baily 1974; Azariadis 1975).

13 The districts and communities in which the data were collected were determined by BRAC as part of their aim to identify the poorest parts of the country. See Bandiera et al. (2017) for details.

surveyed activities. For example, in the business activities module, the respondent reported her main employer(s) for all income-generating activities she was involved in. If they reported a household within the same community as their own, the identity of this household was recorded (using the census listing).

To capture transfers exchanged between households, respondents were asked to report any transfers their household received and any transfers their household gave to others during the 12 months preceding each survey wave. For each transaction, they were asked the value of the transfer, whether it was in cash or in kind, the location of the sender/recipient and the identity of the sender/recipient. If the transfer source/recipient was a household residing in the same village as the respondent, their identity was recorded using the census listing. During the piloting of the baseline survey, it was observed that many respondents reported major transfer transactions, but left out smaller, in-kind transfers (which would only come up upon prompting by enumerators) that typically consisted of food items. In order to capture such transactions, respondents were asked if their household ever receives food from other households and if so who the main sources/recipients of such transfers were. For every respondent, up to three sources and three recipients of food transfers were recorded.[14]

Attrition. Over the four years from baseline to endline, 13 percent of ultra-poor households and 15 percent of non-ultra-poor households attrited from the original sample. Table S1.1 in supplementary online appendix S1 tests for differential attrition among the ultra poor in treatment and control communities. Two findings are of note. First, attrition rates are the same in treatment and control communities. Second, attrition is not differentially correlated with the interaction of treatment status and the baseline levels of key outcomes of interest: transfers received/given (neither on the extensive nor the intensive margin) or the likelihood to have received transfers from employers. Therefore, to ease comparability across different specifications, I will restrict the sample to households that appear in all three waves throughout.

4.1. Poverty and Informal Transfers at Baseline

Table 1 presents baseline descriptive statistics on characteristics of households in our sample. I use the wealth classes determined by the community appraisal at baseline to divide the sample into three groups: poor, middle, and upper class households. I further divide the poor households into those who were deemed (by BRAC) to be eligible for the program (the ultra poor) and those who were not found to be eligible (the other poor). The first panel of the table shows that this classification (based on the community-determined wealth classes and BRAC's selection criteria) is associated with significant differences in terms of the household's economic status and the main (female) respondent's human capital endowment. As indicators of economic status, I use the value of physical assets (land, livestock, other productive assets, and household durables) and per capita household consumption. As proxies for human capital, I use respondents' (the primary female in the household) literacy and heights. Table 1 shows that ultra-poor households have lower assets and consumption compared to other wealth classes—including the other poor households. They also have lower literacy rates and the respondents from ultra-poor households are shorter than women from other wealth classes. The differences between the different wealth classes are statistically significant at conventional levels.[15] This shows that the program's selection criteria were successful in identifying the poorest households in these communities.

14 One distinction between the food transfer questions and the larger transfer transactions is that the former captures not only the realization of transfers in a given period but also the most relevant potential transfer links. Therefore, by construction, the information on food transfers only captures the identity while the latter includes information on both the value of the transfers and the identity of the transfer sender/recipient. Whenever I estimate the effects on realized transfer transactions and their values, I use only the information from the latter. On the other hand, whenever I estimate the effects on network characteristics (such as the wealth status of transfer sources/recipients, or the reciprocity of connections), I use both types of transfer questions.

15 Table S1.2 in supplementary online appendix S1 reports p-values of pairwise comparisons across the wealth classes.

Table 1. Comparison of Socio-Economic Classes at Baseline

	Ultra poor (1)	Other poor (2)	Middle class (3)	Upper class (4)
A. Socio-economic status				
Wealth (BDT)	5,850.98	14,544.02	152,293.69	841,129.07
	(30,655.60)	(72,425.83)	(315,650.26)	(964,643.22)
Consumption (BDT)	9,829.35	10,127.50	12,205.72	20,664.38
	(4,518.74)	(4,771.70)	(7,112.22)	(36,441.19)
Literacy	0.07	0.17	0.27	0.51
	(0.26)	(0.37)	(0.44)	(0.50)
Height (cm)	148.71	149.13	149.82	150.13
	(5.37)	(5.32)	(5.14)	(5.07)
B. Informal transfers received				
Received any transfers during last year	0.22	0.22	0.16	0.22
	(0.41)	(0.41)	(0.37)	(0.41)
Value of transfers received (BDT)	270.08	328.30	513.29	3,097.03
	(1,798.85)	(2,708.87)	(4,081.52)	(29,555.99)
Ever receives food	0.92	0.92	0.83	0.42
	(0.27)	(0.26)	(0.38)	(0.49)
Number of food transfer sources	2.17	2.24	1.94	0.90
	(0.95)	(0.95)	(1.11)	(1.17)
Fraction of transfer sources within village	0.88	0.88	0.88	0.70
	(0.24)	(0.24)	(0.26)	(0.41)
C. Informal transfers given				
Gave any transfers during last year	0.02	0.03	0.09	0.21
	(0.14)	(0.18)	(0.28)	(0.41)
Value of transfers given (BDT)	45.20	62.69	251.40	691.21
	(1,044.62)	(1,323.43)	(2,993.95)	(4,273.22)
Ever gives food	0.44	0.54	0.73	0.81
	(0.50)	(0.50)	(0.45)	(0.39)
Number of food transfer recipients	1.00	1.27	1.70	1.91
	(1.24)	(1.30)	(1.21)	(1.13)
Fraction of transfer recipients within village	0.95	0.94	0.92	0.87
	(0.18)	(0.19)	(0.21)	(0.26)
D. Reciprocity				
Reciprocity of transfers within village	0.42	0.50	0.67	0.80
	(0.44)	(0.44)	(0.42)	(0.35)
Received transfers from an employer	0.10	0.08	0.03	0.00
	(0.30)	(0.28)	(0.16)	(0.00)
Observations	6,732	7,340	6,742	2,215

Source: Author's analysis based on original survey data.
Note: (a) The sample includes observations from the baseline survey. The sample is restricted to ultra-poor households in column 1, other poor households in column 2, middle class households in column 3, and upper class households in column 4. All monetary values are in Bangladeshi takas (BDT). In 2007, USD 1=BDT 69 nominal and USD 1=BDT 18.5 at PPP. (b) For variable definitions, see Appendix A at the end of the paper. Table S1.2 in the supplementary online appendix reports *p*-values for tests of equality of means across the four wealth classes.

Panel B of table 1 shows summary statistics related to informal transfers *received*. On average 22 percent of ultra-poor households reported receiving any transfers in the year preceding the baseline survey. The corresponding rates were similar in other wealth classes—except for the middle class where 16 percent of households had received any transfers during the past year. However, on the intensive margin (i.e., in terms of the value of transfers received), ultra-poor households had received significantly less compared to other wealth classes. Relative to the ultra-poor households, the value of informal transfers received by middle class households was 90 percent higher and the upper classes had received nearly

12 times more. When asked whether they ever received food from other households, 92 percent of ultra-poor households responded positively and they reported, on average, 2 households as their most common sources of food transfers. The corresponding figures are similar for the other-poor households but lower for wealthier households. Only 43 percent of the wealthiest households in the community reported ever receiving food transfers and on average they reported 1 household as the source of food transfers. The final row in Panel B shows the fraction of transfer sources (food or otherwise) who were living within the same community as the respondent's household. For the poor and middle classes, 88 percent of transfer sources were from the community, while the corresponding figure was lower (70 percent) for the upper class households. This suggests that the richest households in these communities are likely to have social networks that expand beyond their locality and send remittances.[16]

Panel C of table 1 summarizes the pattern of informal transfers *given* by the sampled households. Only 2 percent of ultra-poor households made any transfers in the past year, and only 44 percent reported ever giving food to other households. The corresponding rates are higher for wealthier households. Among the richest households in the community, 21 percent had given some transfers to others in the past year and 81 percent reported ever giving food transfers. The majority of these transfers were given to households within the same community, albeit with some variation across the wealth classes.

Comparison of rates and levels of transfers received versus transfers given (Panel B versus C) suggest that, especially for poor households in these communities, transfers received are not always reciprocated with transfers given. In Panel D I report the fraction of transfer sources who are also reported as transfer recipients. Note that I can only do this for within-community transfer sources, since the network-mapping allows us to identify the identity of network members within (but not outside) the community. Nevertheless, since the majority of the transfers happen within the community, especially for the poorer households, this should capture a significant share of the transfer network. On average, only 42 percent of transfer sources of the ultra-poor households were reciprocated (i.e., reported also as recipients of transfers), while the corresponding rate was 50 percent for other poor, 67 percent for middle class, and 80 percent for the richest households in the community.[17]

The final row in table 1 shows that 10 percent of the ultra-poor respondents reported an employer (of the primary female or the male head of the household) as a source of transfers. The corresponding rate was 8 percent among the other poor and 3 percent for middle class households. This suggests that one potential source, especially for the poorest households, may be their members' employers. The literature on interlinked labor contracts suggests that there may be a trade-off between earnings versus risk-sharing motives. Bardhan (1983) argues that workers with limited outside options may end up selecting jobs that provide them with a smoother income across the year but a lower wage rate. Consistent with this, I find that receiving transfers from an employer is correlated with having a lower hourly wage and less volatile annual wage earnings at baseline.[18]

Table 2 presents an overview of how households from different wealth classes were matched in transfer networks at baseline. Panel A shows percentages of households receiving transfers from different

16 Munshi and Rosenzweig (2016) find a similar pattern in India. Their model attributes this pattern to greater demand to share resources making wealthier households more likely to send migrants outside the network (their focus is on the subcaste network).

17 This pattern is in line with Schechter and Yuskavage (2011b) who find that in Paraguay, unreciprocated transfer links are more likely when one household is wealthier or more educated than the other.

18 To estimate the correlation between the terms of the labor contract and receiving informal transfers from the employer, I regress the worker's wage rate on an indicator variable for having reported an employer's household as a source of transfers at baseline, controlling for observable characteristics of the worker (wealth, literacy, height, age, age-squared). Table S1.3 in the supplementary online appendix shows the results. Workers who reported their employers as a source of transfers for their household received significantly lower hourly wages. Transfers from employers were associated with a 5 percent lower wage for female and 7 percent lower wage for male workers relative to the sample average. Moreover, workers receiving transfers also had less volatile earnings from wage-labor but this correlation is imprecisely estimated at conventional levels.

Table 2. Matching in Transfer Networks at Baseline

	Ultra poor (1)	Other poor (2)	Middle class (3)	Upper class (4)
Panel A:	*Percentage of households receiving transfers from…*			
Ultra poor	17.60	9.70	4.41	1.26
Other poor	32.58	39.51	17.49	6.00
Middle class	65.57	66.08	68.78	23.52
Upper class	21.33	23.50	21.51	26.14
Panel B:	*Percentage of households giving transfers to…*			
Ultra poor	14.05	8.83	7.25	11.51
Other poor	21.60	31.43	21.76	28.89
Middle class	28.30	36.09	60.25	51.74
Upper class	3.12	5.26	11.23	28.76
Observations	6,732	7,340	6,742	2,215

Source: Author's analysis based on original survey data.
Note: The sample includes observations from the baseline survey. In column 1 the sample is restricted to ultra-poor households, in column 2 to other poor households, in column 3 to middle class households, and in column 4 to upper class households. Panel A gives the percentage of households receiving transfers from the relevant group; Panel B gives the percentage of households giving transfers to the relevant group. The four rows within Panel A (B) show the percentage of respondents whose household received (gave) any transfers to an ultra-poor (other poor, middle class, or upper class) household in their village.

socio-economic classes at baseline. Of the ultra poor, 18 percent reported another ultra-poor household as a transfer source, while the corresponding rates are lower for other wealth classes (10 percent for other poor, 5 percent for middle, and 1 percent for the upper class households). As we move up along the wealth classes, the likelihood of receiving transfers from a given class rises. Interestingly, within every wealth class, the likelihood of receiving transfers from one's own wealth class is the highest. For example, 26 percent of respondents from the top wealth class reported other rich households in their village as sources of transfers, while the corresponding rate is 21 percent for the ultra poor. Similarly, 69 percent of the middle class respondents reported other middle class households in their village as sources of transfers, while the corresponding rate is 66 percent for ultra-poor households (the difference is significant at the 99 percent confidence level). The lower panel shows the likelihood that a household reported giving transfers to another, by wealth class of the respondent and the transfer recipient. The wealthier households are more likely to give transfers to middle or upper class households compared to the ultra poor: only 12 percent of the upper class respondents reported giving transfers to an ultra-poor household, while 52 percent and 29 percent reported middle or upper class households respectively. Similarly, among the middle class households, only 7 percent reported an ultra poor household as a transfer recipient, while 60 percent reported other middle class households. While these patterns suggest that households are more likely to exchange transfers with similar socio-economic classes to their own, they do not necessarily prove causality. Socio-economic classes are likely to be endogenous to the structure of social networks within a community. I will return to this in the section Matching in Transfer Networks, where I estimate the causal effect of poverty on the way households are matched in transfer connections.

4.2. Randomization and Balancing at Baseline

To evaluate the TUP program, the timing of the program's rollout was randomized at BRAC branch office level. BRAC determined 40 branch offices that would implement the program. Standard procedures to identify who would be the beneficiaries of the program were carried out (by BRAC program officers) in all of these branches in the same way. Following the identification of potential beneficiary households, 20 branch offices were randomly selected to receive the program in 2007, and the rest in 2011. The randomization was stratified at the subdistrict level—within each subdistrict, one branch was randomly allocated to treatment and one to the control group. All of the selected communities in treatment branches were treated in 2007 while the control communities were not treated until after the endline survey in 2011.

Table S1.4 in the supplementary online appendix reports baseline balance tests which compare the characteristics of ultra-poor households in treatment and control groups at baseline. For every variable, the table reports the mean and standard deviation in treatment and control communities, as well as the *p*-value on a test of equality of means (column 7) and the normalized difference of means (column 8). Overall, the samples are well balanced: only 2 out of 26 tests for the equality of the means are rejected, and all of the normalized differences are below the 0.25 threshold recommended by Imbens and Wooldridge (2009). Thus we can conclude that the randomization was successful and the control sample provides a valid counterfactual for the treatment group.

5. Empirical Analysis

5.1. Estimation

In order to estimate the effects of the TUP program, I pool observations from the two follow-up surveys and estimate an ANCOVA model. In particular, I estimate:

$$y_{idt} = \alpha + \lambda T_i + \beta y_{id0} + \gamma S_{t=2} + \delta_d + \epsilon_{idt}, \quad (1)$$

where y_{idt} is the outcome of interest for household i from subdistrict d at survey wave t with time periods referring to 2007 baseline ($t = 0$), 2009 midline ($t = 1$), and 2011 endline ($t = 2$); T_i is an indicator variable equal to 1 if household i lived in a treatment branch and 0 otherwise; y_{id0} is the baseline level of the outcome variable for household i; and δ_d are subdistrict (randomization strata) fixed effects. The parameter of interest is λ, the difference between treatment and control observations. The standard errors are clustered at the BRAC branch office level (the unit of randomization) in all the regressions. Under the identifying assumption that the control branches represent a valid counterfactual for the treated branches in the absence of the program, namely that trends in all outcomes of interest are the same in treatment and control branches, λ identifies the causal effect of the TUP program on y_{idt}. I will estimate (1) on the full sample of ultra-poor households, thus estimating the intention to treat (ITT) effect.[19]

In order to test for the differences in treatment effects at midline and endline surveys, I estimate the following specification:

$$y_{idt} = \alpha + \lambda T_i + \theta T_i \cdot S_{t=2} + \beta y_{id0} + \gamma S_{t=2} + \delta_d + \epsilon_{idt}, \quad (2)$$

where the main point of departure from specification (1) is the inclusion of the interaction term $T_i \cdot S_{t=2}$. As such, in specification (2), λ corresponds to the treatment effect at midline and θ estimates the difference between the treatment effect at endline compared to midline. The results are reported in Appendix S2. In general, I find that the estimates for θ are statistically and economically insignificant for our outcomes of interest. In the few cases where this is not the case, I draw the reader's attention to the difference between midline and endline treatment effects.[20]

5.2. Effects on the Level of Informal Transfers

Table 3 presents findings on the impact of the program on transfers received/provided by ultra-poor households. "Treatment" shows the estimate for λ in specification (1). The first column of the table shows that

19 Of the households identified as ultra poor in treatment branches, 86 percent eventually received an asset. The other 14 percent either ceased to meet the eligibility criteria when transfers were implemented, or chose not to take up the program. As such, the ITT estimates reported in the paper are close to the average treatment effect on the treated (ATT).

20 I also conducted a robustness check that controls for the interaction of the baseline level of the outcome y_{id0}, with the survey wave fixed effects $S_{t=2}$, in both specifications (1) and (2). The results are robust to this additional control—available upon request.

Table 3. Effects on Informal Transfers of Ultra-Poor Households

	Transfers received in past 12 months		Transfers given in past 12 months		Net transfers received in past 12 months	
	(Yes=1) (1)	(BDT) (2)	(Yes=1) (3)	(BDT) (4)	(Yes=1) (5)	(BDT) (6)
Treatment	−0.129***	−104.664	0.012	40.877	−0.131***	−151.721**
	(0.020)	(78.648)	(0.007)	(45.704)	(0.019)	(74.478)
Mean in control	0.497	783.055	0.059	143.978	0.484	639.076
Adjusted R-squared	0.126	0.027	0.030	0.001	0.121	0.019
Observations	13,464	13,464	13,464	13,464	13,464	13,464
Panel B:			*Within-village transfers*			
Treatment	−0.037***	41.246*	−0.000	−4.120	−0.036***	45.558*
	(0.013)	(23.040)	(0.002)	(4.078)	(0.013)	(23.656)
Mean in control	0.148	142.403	0.015	11.865	0.147	130.538
Adjusted R-squared	0.041	0.004	0.013	0.001	0.041	0.003
Observations	13,464	13,464	13,464	13,464	13,464	13,464
Panel C:			*Outside-village transfers*			
Treatment	−0.123***	−144.864*	0.011*	45.021	−0.123***	−197.782***
	(0.019)	(72.655)	(0.006)	(43.650)	(0.018)	(65.197)
Mean in control	0.430	640.652	0.048	132.113	0.420	508.539
Adjusted R-squared	0.117	0.027	0.023	0.001	0.112	0.018
Observations	13,464	13,464	13,464	13,464	13,464	13,464

Source: Author's analysis based on original survey data.
Note: The table reports ITT (intention to treat) estimates based on specification (1) estimated with OLS. The sample includes observations from ultra-poor households surveyed at the midline and endline surveys. All specifications control for the baseline level of the outcome, survey wave, and subdistrict (strata) fixed effects. Standard errors are clustered at the BRAC (Bangladesh Rural Advancement Committee) branch office level (unit of randomization). All monetary values are measured in Bangladeshi takas (BDT), deflated to 2007 prices using the annual CPI index published by the Bank of Bangladesh. In 2007, USD 1=BDT 69 nominal and USD 1=BDT 18.5 at PPP. The dependent variable in Panel A, column 1 is a dummy variable (DV) equal to 1 if the respondent's household received any transfers (in cash or in kind) from another household during the 12 months preceding the survey; in column 2 it is the monetary value of transfers received during the past 12 months; in column 3 it is a DV equal to 1 if the respondent's household gave any transfers to another household during the past 12 months; in column 4 it is the monetary value of all transfers given by the respondent's household during the last 12 months; in column 5 it is a DV equal to 1 if the respondent's household was a net transfer receiver in the past 12 months; in column 6 it is the monetary value of transfers received minus transfers made by the respondent's household in the past 12 months. All dependent variables in Panel B (C) are identical to those in Panel A, except that they refer to within (outside) village transfers. *** (**) (*) indicates significance at the 1 percent (5 percent) (10 percent) level.

the program reduced the likelihood of having received transfers (during the past year) by 13 percentage points (ppt), which corresponds to a 26 percent reduction relative to the control group. On the intensive margin (column 2), the value of transfers received by the ultra poor was lower by BDT 105 (13 percent relative to control) but this effect is imprecisely estimated. Columns 3 and 4 show that the program did not have a significant impact on the likelihood or level of transfers provided by ultra-poor households to others—the point estimates are positive, but imprecisely estimated. As a result, the effect on net transfers (columns 5 and 6) was a reduction. Ultra-poor households in treatment branches were 13 ppt less likely to be net transfer recipients and received, in net, BDT 151 less in transfers (24 percent relative to control).

The lower panels of table 3 break down the transfers by the location of the households giving or receiving transfers to/from the ultra-poor households. Panel B shows the treatment effects on transfers exchanged with households within the same community, and Panel C with those outside the village. I find that the ultra poor in treatment branches were 4 ppt less likely to receive transfers from within their communities. On the intensive margin, they received more transfers from their neighbors. In particular, the value of transfers they received from households in their community was higher by BDT 41, which corresponds to a 29 percent increase relative to the control group. There was no significant treatment effect on transfers the ultra-poor households made to other households within their communities (columns 3

Table 4. Effects on Food Transfers of Ultra-Poor Households

	Transfers received		Transfers given	
	(Yes=1)	(No.)	(Yes=1)	(No.)
	(1)	(2)	(3)	(4)
Treatment	0.013**	0.097*	0.069**	0.229***
	(0.005)	(0.050)	(0.026)	(0.065)
Mean in control	0.936	2.340	0.560	1.284
Adjusted R-squared	0.069	0.169	0.260	0.314
Observations	13,464	13,464	13,453	13,464
Panel B:		*Within-village transfers*		
Treatment	0.020***	0.136**	0.068**	0.227***
	(0.006)	(0.051)	(0.026)	(0.063)
Mean in control	0.907	2.199	0.547	1.236
Adjusted R-squared	0.078	0.193	0.259	0.318
Observations	13,464	13,464	13,464	13,464
Panel C:		*Outside-village transfers*		
Treatment	−0.027**	−0.034**	−0.003	−0.001
	(0.012)	(0.015)	(0.004)	(0.006)
Mean in control	0.107	0.141	0.037	0.047
Adjusted R-squared	0.065	0.074	0.037	0.036
Observations	13,464	13,464	13,464	13,464

Source: Author's analysis based on original survey data.

Note: The table reports ITT (intention to treat) estimates based on specification (1) estimated with OLS. The sample includes observations from ultra-poor households surveyed at the midline and endline surveys. All specifications control for the baseline level of the outcome, survey wave, and subdistrict (strata) fixed effects. Standard errors are clustered at the BRAC (Bangladesh Rural Advancement Committee) branch office level (unit of randomization). The dependent variable in column 1 is a dummy variable (DV) equal to 1 if the respondent's household ever receives any food transfers from other households. The dependent variable in Panel A, column 1 is a dummy variable (DV) equal to 1 if the respondent's household ever receives any food transfers; in column 2 it is the number of households (capped at 3) that the respondent's household receives food transfers from; in column 3 it is a DV equal to 1 if the respondent's household ever gives food transfers to other households; in column 4 it is the number of households (capped at 3) that the respondent's household gives food transfers to. All dependent variables in Panel B (C) are identical to those in Panel A, except that they refer to within (outside) village food transfers. *** (**) (*) indicates significance at the 1 percent (5 percent) (10 percent) level.

and 4), and as such the net effect (columns 5 and 6) was similar to the effect on transfers received. In contrast, Panel C shows that transfers received from outside the village were significantly lower, both on the extensive (by 12 ppt) and on the intensive margin (by BDT 144, or 23 percent relative to control).[21]

Next I analyze the effects on ultra-poor households' participation in food transfers. Table 4 presents the findings. On average, 94 percent of the control group reported that their household received food transfers. The program led to a small (1.3 ppt) increase in this rate. Moreover, ultra poor in treatment communities

21 In Online Appendix S2, I compare the treatment effects measured at midline and endline surveys. Table S2.1 in the supplementary online appendix shows that the treatment effects at midline are not statistically different from those estimated at endline. However, on the intensive margin, the large point estimates for θ suggest the crowding out effect (on the intensive margin) is not diminishing, and possibly getting larger, over time. It is possible to conduct the same analysis on informal loans exchanged with other households. Table S1.8 in the supplementary online appendix presents the treatment effects on interhousehold loans received/given by ultra-poor households. The program lowers the value of informal loans received by ultra-poor households, while it increases both the likelihood of giving loans and the value of loans given by ultra poor to other households. As a result, in net, the value of loans received by ultra-poor households goes down, by nearly 100 percent relative to the control group. This is true for both loans within and loans outside the village. These findings complement the results reported in Bandiera et al. (2017) where we analyzed the treatment effects on likelihood of receiving and giving loans. Table IV in Bandiera et al. (2017) shows that the program increased the likelihood of both receiving and giving loans by the ultra poor. The key difference between the results in Bandiera et al. (2017) and those reported here is the fact that loans received in Bandiera et al. (2017) include formal loans, whereas the results in table S1.8 are limited to interhousehold loans.

reported 0.10 more households as sources of food transfers, a 4 percent increase relative to the control group. Looking at the corresponding effect on food transfers given by ultra-poor households, columns 3 and 4 of the table show that ultra-poor households were 7 ppt more likely to give food transfers to others and on average they transferred food to 0.23 more households (18 percent more relative to control).[22] The rest of the table shows that these changes are driven by changes in food transfers *within* the village (Panel B), while food transfer sources from outside the village are slightly lower (Panel C).[23]

To summarize, the findings in this section suggest that the program crowded out informal transfers received by ultra-poor households, but this effect is less pronounced for within-village transfers. If anything, it led to a modest increase in the value of transfers received and the number of food transfer sources within the community.

5.3. Matching in Transfer Networks

In this section, I examine whether the program led to any changes in terms of *who* the ultra-poor households exchanged transfers with. In particular, I estimate the effects of the program on the likelihood that the ultra-poor exchange transfers with households from different wealth classes in their communities. The first panel of table 5 shows that the program increased ultra-poor households' likelihood to receive transfers from other ultra-poor households by 6 ppt (44 percent relative to control) and from other poor households by 4 ppt (10 percent). There was no significant effect on the likelihood of receiving transfers from middle or upper class households. Panel B shows that the program led to an increase on the likelihood of giving transfers to all wealth classes. In particular, ultra poor in treated communities were 5 ppt (48 percent) more likely to give transfers to households within their own wealth class, 3 ppt (13 percent) more to other poor, 5 ppt (14 percent) to middle, and 2 ppt (21 percent) more likely to provide transfers to upper class households in their communities.[24]

As an alternative test of effects on matching and to control for any self-reporting bias, I limit the sample to non-ultra-poor households and estimate the treatment effects on their likelihood to exchange transfers with ultra poor and other wealth classes within the community. For this, I first estimate specification (1) on the sample of non-ultra households. The estimate for λ corresponds to the average treatment effect on

22 For food transfers, data on the intensive margin (value of transfers) was not collected. However, at midline and endline surveys, respondents were asked to report whether their households' frequency of borrowing (lending) from (to) households reported as sources (recipients) of food transfers at baseline had increased, decreased, or remained the same. In table S1.5 in the supplementary online appendix I use this information to test whether the program affected the frequency of borrowing or lending food to baseline network partners. Overall, I fail to find a consistent effect. Ultra poor in treated communities were (relative to control) less likely to say their borrowing from baseline sources of food transfers had increased, but they were no more likely to say it had decreased; and there was no significant change in their frequency of giving food to baseline recipients either. Based on this, I conclude that the program did not crowd out food transfers that the ultra poor exchanged with their baseline network members. On the other hand, it led to a modest increase (on the extensive margin) in food transfers that the ultra poor exchanged with other households within their communities.

23 Table S2.2 in supplementary online appendix S2 tests whether the treatment effects at midline differ from those at endline. The effects typically get larger in magnitude by the endline; the differences are never precisely estimated.

24 Note that at baseline, the ultra-poor households in the treatment group were less likely to receive/give transfers to upper class households compared to the ultra poor in the control group and this difference was statistically significant (see table S1.4). As such, the treatment effect on transfers received/given to upper class households should be interpreted with caution. Having said that, the direction of the baseline difference suggests that the treatment effects I estimate are likely to be lower bound estimates of the true treatment effect. In table S2.3 in the supplementary online appendix I assess the difference between midline and endline treatment effects on non-ultra-poors' likelihood to receive transfers from different wealth classes. The results show no significant differences between the two surveys for most outcomes, except for the likelihood to give transfers to the upper class. The treatment effect at endline is significantly higher than at the midline. This suggests that it takes time for the ultra-poor households to start providing transfers to the highest wealth classes.

Table 5. Effects on Matching in Transfer Networks.

	(1)	(2)	(3)	(4)
Panel A:	*Transfers received by ultra-poor households from...*			
	Ultra poor	Other poor	Middle class	Upper class
Treatment	0.056***	0.036***	0.018	−0.008
	(0.007)	(0.012)	(0.012)	(0.008)
Mean in control	0.128	0.345	0.701	0.260
Adj. R-squared	0.439	0.415	0.327	0.407
Observations	13,464	13,464	13,464	13,464
Panel B:	*Transfers given by ultra-poor households to...*			
	Ultra poor	Other poor	Middle class	Upper class
Treatment	0.052***	0.032**	0.054***	0.016***
	(0.008)	(0.013)	(0.019)	(0.005)
Mean in control	0.108	0.252	0.394	0.078
Adj. R-squared	0.368	0.306	0.254	0.145
Observations	13,464	13,464	13,464	13,464

Source: Author's analysis based on original survey data.
Note: The table reports ITT (intention to treat) estimates based on specification (1) estimated with OLS. The sample includes observations from ultra-poor households. All specifications control for the baseline level of the outcome, survey wave, and subdistrict (strata) fixed effects. Standard errors are clustered at the BRAC (Bangladesh Rural Advancement Committee) branch office level (unit of randomization). In Panel A, the dependent variables in columns 1, 2, 3, and 4 are dummy variables equal to 1 if the respondent reported that her household received any transfers from any ultra poor, other poor, middle class, or upper class households (within her community) respectively. In Panel B, the dependent variables in columns 1, 2, 3, and 4 are dummy variables equal to 1 if the respondent reported that her household gave any transfers to any ultra poor, other poor, middle class, or upper class households (within her community) respectively. *** (**) (*) indicates significance at the 1 percent (5 percent) (10 percent) level.

all non-ultra-poor households. Second, I estimate

$$y_{idt} = \sum_{c=1}^{3} \alpha_c C_i^c + \sum_{c=1}^{3} \lambda_c T_i \times C_i^c + \beta y_{id0} + \gamma S_{t=2} + \delta_d + \epsilon_{idt}, \tag{3}$$

where C_i is baseline wealth class of household i (other poor, middle, or upper class). In this specification, λ_c gives the treatment effect on wealth class c. Table 6 presents the results. Panel A1 shows that non-ultra-poor households in treated communities were on average 2 ppt more likely to report receiving transfers from ultra-poor households within their communities. This corresponds to a large (37 percent) increase relative to the control group. On the other hand, there was no significant impact on transfers received from other wealth classes (columns 2–4). Panel A2 breaks down the effects by the respondent's wealth class, i.e., presents the estimates for λ_c in specification (3). Column 1 shows that other poor households were 4 ppt more likely to report receiving transfers from ultra poor in treated communities, corresponding to a 46 percent increase relative to the control group. The middle classes were 2.5 ppt (52 percent) more likely to receive transfers from ultra poor, while the corresponding effect was 1 ppt (71 percent) for the wealthiest class. The remaining columns in Panel A2 show no significant effects for other wealth classes. This implies that the increase in likelihood of transfers from ultra-poor households observed in column 1 did not crowd out non-ultra-poor households' likelihood of receiving transfers from other wealth classes.[25]

25 Note that the point estimate of the "Treatment effect for upper class" in column 4 is large (−0.04) and border-line significant (p-value = 0.109), suggesting that the program may have crowded out transfers received by upper class households from their own wealth class. In table S2.4 in the supplementary online appendix I assess the differences between midline and endline treatment effects on non-ultra-poors' likelihood of receiving transfers from different wealth classes. Two findings are of note: Based on the pooled specification (Panel A1), the midline treatment effect on non-ultra-poor's likelihood to receive transfers from ultra poor was 2 ppt and significant, while this effect had increased to 3 ppt

Table 6. Effects on Matching in Transfer Networks, Reported by Non-Ultra-Poor Households

	(1)	(2)	(3)	(4)
	Transfers received by non-ultra-poor households from…			
	Ultra poor	Other poor	Middle class	Upper class
Panel A1: Pooled				
Treatment	0.023***	0.012	0.001	−0.008
	(0.004)	(0.008)	(0.013)	(0.007)
Panel A2: By wealth class				
Treatment effect for other poor	0.039***	0.008	0.003	0.007
	(0.007)	(0.012)	(0.012)	(0.009)
Treatment effect for middle class	0.025***	0.013	0.005	−0.001
	(0.005)	(0.008)	(0.015)	(0.007)
Treatment effect for upper class	0.012**	0.003	−0.026	−0.040
	(0.006)	(0.012)	(0.030)	(0.025)
Control mean	0.062	0.319	0.691	0.279
… for other poor	0.085	0.448	0.711	0.272
… for middle class	0.048	0.224	0.770	0.261
… for upper class	0.017	0.128	0.385	0.360
Observations	32,594	32,594	32,594	32,594
	Transfers given by non-ultra-poor households to…			
	Ultra poor	Other poor	Middle class	Upper class
Panel B1: Pooled				
Treatment	0.025***	−0.011	−0.011	−0.002
	(0.005)	(0.010)	(0.020)	(0.007)
Panel B2: By wealth class				
Treatment effect for other poor	0.031***	−0.002	−0.012	−0.006
	(0.007)	(0.016)	(0.019)	(0.009)
Treatment effect for middle class	0.027***	−0.013	−0.019	−0.001
	(0.006)	(0.011)	(0.022)	(0.008)
Treatment effect for upper class	0.017	−0.027	−0.000	0.006
	(0.012)	(0.018)	(0.022)	(0.016)
Control mean	0.078	0.338	0.600	0.177
… for other poor	0.084	0.397	0.507	0.119
… for middle class	0.065	0.268	0.721	0.182
… for upper class	0.091	0.329	0.584	0.373
Observations	32,594	32,594	32,594	32,594

Source: Author's analysis based on original survey data.

Note: The table reports ITT (intention to treat) estimates based on specification (3) estimated with OLS. The sample includes observations from non-ultra-poor households. All specifications control for the baseline level of the outcome, survey wave, and subdistrict (strata) fixed effects. Standard errors are clustered at the BRAC (Bangladesh Rural Advancement Committee) branch office level (unit of randomization). In Panel A, the dependent variables in columns 1, 2, 3, and 4 are dummy variables equal to 1 if the respondent reported that her household received any transfers from any ultra-poor, other poor, middle class, or upper class households (within her community) respectively. In Panel B, the dependent variables in columns 1, 2, 3, and 4 are dummy variables equal to 1 if the respondent reported that her household gave any transfers to any ultra-poor, other poor, middle class, or upper class households (within her community) respectively. In the pooled regressions (Panels A1 and B1) each observation is weighted using sampling weights calculated as the fraction of households surveyed from each wealth class (lower, middle, and upper) relative to the number of households from the relevant wealth class in the community census. *** (**) (*) indicates significance at the 1 percent (5 percent) (10 percent) level.

The lower panel of table 6 shows the effects on transfers given by non-ultra-poor households to different wealth classes. On average, the non-ultra-poor households were 3 ppt more likely to report

by the endline (the difference is marginally significant at the 10 percent level). This suggests that the effects are increasing over time as the ultra poor are becoming more likely to provide transfers to the non-ultra-poor in their communities. Second, at midline, I find that upper class households were 6 ppt less likely to receive transfers from other upper class giving transfers to ultra poor in their communities (a 35 percent increase relative to the control group). Columns 2 to 4 show that the treatment effects on likelihood of transfers to other wealth classes are negative but small in magnitude and imprecisely estimated. Panel B2 breaks down the effects by wealth class of the respondent's household. Respondents from other poor and middle class households were 3 ppt more likely to report giving transfers to ultra-poor households. While the corresponding effect for upper class households is also positive, it is imprecisely estimated.[26] The rest of the table shows that there were no significant spillover effects of the program on non-ultra-poor households' likelihood to give transfers to households from other non-ultra-poor households.[27]

In order to assess the long-run effects of the program, I use data from the third follow-up survey that was conducted in 2014, that is, seven years after the baseline. At that point, 49 percent of control communities had also been treated by the program. Moreover, only the female respondents were surveyed and detailed network information was not collected. However, respondents from all wealth classes were asked whether they provided informal transfers to any of the ultra-poor households within their communities, and if so to which ones. They were also asked whether they used to provide transfers to any of the ultra-poor households seven years ago, and if so to which ones they used to provide transfers. I use these two pieces of information to test whether households in the early-treatment communities (i.e., treated in 2007) are differentially more likely to provide transfers to ultra-poor households in their communities, and how the effects vary by the wealth class of the respondent's household. For this, I estimate specification (3), using as y_{id0} the retrospective question on pre-treatment transfers. Table 7 presents the results. The first column shows that non-ultra-poor households in early-treatment communities were significantly more likely to report giving transfers to ultra-poor households in their communities. In particular, the long-run treatment effect for other poor households is 1.3 ppt, for middle classes 2 ppt, and for the upper classes it is 3 ppt. These effects are both statistically and economically significant (very few non-ultra-poor households in the control group reported giving transfers to the ultra poor, as demonstrated by the means reported in the lower panel). The second column shows that the percentages of ultra-poor households reported as receiving transfers from middle and upper class households were higher by 0.5 and 1.2 ppt respectively—corresponding to about a 100 percent increase relative to the respective control group means. This implies that the program led to a long-run increase in the likelihood that ultra-poor households receive informal transfers from higher wealth classes within their communities.

To sum up, I find that the program increased ultra-poor households' likelihood to give transfers, not only to other ultra-poor households but to households in wealthier classes within their communities. One possible explanation for this could be that the program, by improving ultra-poor's economic conditions, may have enabled them to reciprocate past transfers they used to receive from others. In line with this, I show in table S1.6 in the supplementary online appendix that the increase in ultra-poor's transfers is largely driven by an increase in their likelihood to provide transfers to households who at baseline used to be their transfer sources. Households who used to provide transfers to ultra poor at baseline experience a larger increase in their likelihood to receive transfers from ultra poor in their communities. Nevertheless, there are also some significant, although weaker, effects for other poor and middle class households who were not providing transfers to the ultra poor at baseline. Moreover, I showed above that the ultra poor also become more likely to *receive* transfers from wealthier classes within their communities. This suggests that the effects are not only driven by ultra poor reciprocating for past transfers. Instead, some of the impact must be driven by a change in terms of who the ultra-poor households are exchanging transfers

households, but this effect had dissipated by the endline survey. This implies that in the short run, the program crowded out likelihood that upper class households receive transfers from their own wealth class.

26 I cannot reject the null hypotheses that the treatment effects on middle and upper classes, or on other poor and upper classes are equal (p-values are 0.448 and 0.287 respectively).

27 Table S2.4 in the supplementary online appendix presents the results of comparing midline and endline treatment effects. Overall, I do not find significant differences between the treatment effects measured at midline and endline.

Table 7. Long-Run Effects on Matching in Transfers

	Whether gave transfers to any ultra-poor household (1)	Fraction of ultra-poor households who were given transfers (2)
Treatment effect for ultra poor	−0.004	−0.057
	(0.005)	(0.130)
Treatment effect for other poor	0.013***	0.265
	(0.006)	(0.163)
Treatment effect for middle class	0.015***	0.509***
	(0.004)	(0.176)
Treatment effect for upper class	0.030***	1.149**
	(0.009)	(0.494)
Control mean	0.012	0.441
... for ultra poor	0.008	0.280
... for other poor	0.007	0.313
... for middle class	0.012	0.510
... for upper class	0.034	1.066
Observations	21,547	21,547

Source: Author's analysis based on original survey data.
Note: The table reports ITT (intention to treat) estimates based on specification (1) estimated with OLS. The sample includes observations from the long-run follow-up survey (2014). All specifications control for subdistrict (strata) fixed effects, and the pre-treatment level of the outcome as reported (retrospectively) in the 2014 survey. Standard errors are clustered at the BRAC (Bangladesh Rural Advancement Committee) branch office level (unit of randomization). The dependent variable in column 1 is a dummy variable equal to 1 if the respondent's household gave any transfers or loans to any ultra-poor household in her village. The dependent variable in column 2 is the fraction of ultra-poor households living in the respondent's village who received transfers or loans from the respondent's household, multiplied by 100 (so that the effects correspond to percentage points). *** (**) (*) indicates significance at the 1 percent (5 percent) (10 percent) level.

Table 8. Effects on Reciprocity of Ultra Poor's Transfers

	Reciprocity of transfer links	Reciprocity of transfers with...			
		Ultra poor	Other poor	Middle class	Upper class
	(1)	(2)	(3)	(4)	(5)
Treatment	0.076***	0.044*	0.094***	0.086***	0.117***
	(0.018)	(0.024)	(0.020)	(0.020)	(0.026)
Mean in control	0.510	0.627	0.560	0.449	0.267
Adjusted R-squared	0.217	0.189	0.208	0.235	0.192
Observations	12,774	2,792	4,823	9,474	3,135

Source: Author's analysis based on original survey data.
Note: The table reports ITT (intention to treat) estimates based on specification (1) estimated with OLS. The sample includes observations from ultra-poor households surveyed at the midline and endline surveys. All specifications control for the baseline level of the outcome, survey wave, and subdistrict (strata) fixed effects. Standard errors are clustered at the BRAC (Bangladesh Rural Advancement Committee) branch office level (unit of randomization). The dependent variable in column 1 is the fraction of transfer sources within the village who are also reported as recipients of transfers (of last 12 months' or food transfers ever) from the respondent's household. The dependent variables in columns 2–5 are the fraction of ultra-poor, other poor, middle class, or upper class households respectively who are reported as sources of transfers (food, cash, or other) and also reported as recipients of transfers given by the respondent's household. *** (**) (*) indicates significance at the 1 percent (5 percent) (10 percent) level.

with. In the following section, I will test directly whether the program affected the reciprocity of informal transfer links of the ultra poor.

5.4. Effects on Reciprocity of Informal Transfers

In order to estimate the treatment effect on reciprocity of transfers, I estimate specification (1) using as the outcome variable the fraction of ultra poor's transfer sources who are also reported as receivers of transfers. Table 8 presents the results. In the control group, 51 percent of ultra poor's transfer sources were

Table 9. Effects on Transfers from Employers

	Household received transfers from…		
	Any employer(s) (1)	Female respondent's employer(s) (2)	Male respondent's employer(s) (3)
Treatment	−0.039***	−0.035***	−0.013***
	(0.008)	(0.008)	(0.004)
Mean in control	0.091	0.081	0.032
Adjusted R-squared	0.068	0.059	0.039
Observations	13,464	13,464	7,778
Panel B:		*Sample of wage-workers in all 3 surveys*	
Treatment	−0.040***	−0.026	−0.019
	(0.013)	(0.016)	(0.015)
Mean in control	0.133	0.138	0.073
Adjusted R-squared	0.079	0.071	0.077
Observations	6,336	4,488	1,332

Source: Author's analysis based on original survey data.
Note: The table reports ITT (intention to treat) estimates based on specification (1) estimated with OLS. The sample includes observations from ultra-poor households surveyed at the midline and endline surveys. All specifications control for the baseline level of the outcome, survey wave, and subdistrict (strata) fixed effects. Standard errors are clustered at the BRAC (Bangladesh Rural Advancement Committee) branch office level (unit of randomization). The dependent variable in column 1 is a dummy variable (DV) equal to 1 if either the main female or the male head of the household had worked (in the last 12 months) for an employer who lives in the same village and is reported as a source of transfers for the household. The dependent variable in column 2 (3) is a DV equal to 1 if the main female respondent (male head of the household) had worked (in the last 12 months) for an employer who lives in the same village and is reported as a source of transfers for the household. In Panel B, the sample is restricted to ultra-poor households where either the main female respondent or the male head of the household had worked for an employer in the 12 months preceding the survey at all 3 surveys (baseline, midline, and endline). *** (**) (*) indicates significance at the 1 percent (5 percent) (10 percent) level.

also reported as recipients of transfers by the ultra poor. The program led to an 8 ppt increase (15 percent) in the reciprocity of ultra poor's transfer connections. The following columns of the table break down this measure according to the baseline wealth class of the connection. The results show that the reciprocity of ultra poor's transfers increased with all wealth classes, and especially with households who were ranked in higher wealth classes at baseline. In particular, reciprocity of ultra poor's transfers increased by 9 ppt (17 percent) with other poor, by 9 ppt (19 percent) with middle class, and by 12 ppt (44 percent) with upper class households. In contrast, the increase in reciprocity of transfers with other ultra poor was 4.4 ppt (7 percent) and marginally significant.[28]

An alternative source of transfers for ultra-poor households at baseline may be their employer(s). Baseline correlations presented in Poverty and Informal Transfers at Baseline suggested that such transfers may be reciprocated by working for a lower wage rate, lowering the costs of labor for the employers.[29] In table 9, I test whether the program affected ultra-poor households' likelihood to receive transfers from their members' employers. The dependent variable in column 1 is an indicator for having received any transfers from an employer of either the main female respondent or the male head of the household. On average, 9 percent of ultra-poor households in the control group reported an employer as a source of transfers for their household. The program led to a fall in the likelihood of receiving transfers from employers by 4 ppt. This effect is both statistically and economically significant (43 percent relative to the control group). The next two columns show the effect for the female and the male respondent(s) separately. Ultra-poor households were 4 ppt less likely to receive transfers from an employer of the main

28 Table S2.5 in the supplementary online appendix tests whether the treatments effects on reciprocity of ultra poor's transfer links differ between the midline and endline surveys. The results show that the treatment effects were significantly higher at endline relative to midline. This implies that the increase in reciprocity happened gradually over time, and was only fully realized by the endline survey. In other words, it took time for the ultra-poor households to establish reciprocal transfer links.

29 See table S1.3 discussed in footnote 18 above.

female respondent, while the corresponding effect for the male respondents was 1.3 ppt. While these effects suggest that the improvement in socio-economic conditions of ultra-poor households may have reduced their engagement in interlinked labor contracts, they may also be driven by the fall in wage-labor caused by the program. In fact, table S1.7 in supplementary online appendix S1 shows that the program significantly reduced labor supply of both female and male respondents into wage-labor (although the effect on the extensive margin is insignificant for the latter). As such, it is possible that the fall in likelihood of receiving transfers from employers may simply capture a fall in having an employer. In order to assess whether the effect is driven by this, in Panel B of table 9 I restrict the sample to ultra-poor households who are engaged in wage-employment at baseline as well as at the two follow-up surveys. Similarly, I find that households within this subsample are less likely to receive transfers from their employers. Note that this analysis has a caveat (since it is based on a selected sample), and as such it should be interpreted as suggestive evidence.[30]

Overall, the findings in this section imply that the program led to a change in the mechanisms through which ultra-poor households reciprocate transfers from others. The reduction in their poverty level enabled the ultra poor to have reciprocal transfer connections with other households in their communities, and reduced their likelihood to receive transfers from their employers. The latter suggests that there was a reduction in the interlinkage of labor and insurance arrangements of the ultra poor.

6. Discussion

6.1. Outside-Village Transfers

One of the emerging findings is that the program crowded out outside-village transfers. Unfortunately, I have limited information on the identity of outside-village transfer sources/recipients. There is one important piece of information about outside-village transfers: I know whether the transfers are given/received to/from the respondent's first-degree family members.[31] Moreover, at baseline, respondents were asked to classify the wealth status of their first-degree family members' households relative to their own (whether the family members are better off, the same, or worse off in terms of their wealth). I exploit this information to shed some light on the changes in transfers outside the village. First, note that a large share of the outside-village transfers consists of transfers from first-degree family members. In particular, 63 percent (81 percent) of the outside-village transfers received (given) by ultra poor in the control group at midline/endline were from (to) their first-degree family members living outside the village. Second, I analyze the effects on outside-village transfers separately for family versus non-family transfers; table S1.9 in the supplementary online appendix displays the results. On the extensive margin, ultra-poor households were significantly less likely to be net receivers of transfers from outside the village and this seems to be driven mainly by non-family transfers. On the intensive margin, I find that the effects on within-family transfers display a similar pattern to outside-family transfers. Treated ultra-poor households received BDT 74 less in net transfers from their first-degree family members who live outside the village, and they received BDT 124 less from others. While the former is imprecisely estimated, the two effects are statistically not different from one another.

Next I distinguish the intra-family transfers by the relative wealth of the family members at baseline. Table S1.10 in the supplementary online appendix displays the effects on transfers from/to family members who were reported to be wealthier than the respondent's household at baseline in Panel A, and the transfers from/to family members whose wealth conditions were the same or worse off than the respondent's household at baseline. I find that treated households give significantly more transfers (in value) to

30 In table S2.6 in the supplementary online appendix I assess the differences in treatment effects on probability of having transfers from employers at endline relative to midline. None of the differences are precisely estimated and the treatment effects were similar at the two follow-up surveys.

31 Parents, children, siblings, and siblings-in-law.

their wealthier family members—the value of transfers given to wealthier family members is higher by BDT 6.3, which is a nearly two-fold increase relative to the mean in the control group (BDT 2.6). This is in line with the effects on within-village transfer networks, where I showed that the ultra poor were more likely to give transfers to wealthier households in the village. So, even though I do not have more detailed data to shed light on the full network outside the village, the effects within the first-degree family network are largely in line with what I find in terms of matching in within-village transfers.

6.2. Mechanisms

As described in the section on the program "Targeting the Ultra Poor," the program entails a number of components that may affect ultra poors' informal transfers and who they interact with in risk-sharing networks within the community. This paper evaluates the effects of the entire program package because the research design does not allow for isolating and contrasting the impacts of specific program components. Disentangling these mechanisms is beyond the scope of the current paper. Nevertheless, I can exploit the variation in participation rates in various program components in order to shed some light on the role of various program components in crowding out of interhousehold transfers. Following Gelbach (2016), I decompose the overall ITT impacts into components explained by different potential mediators. The analysis provides useful suggestive evidence on which channels might contribute more significantly to the overall effects on transfers.

During the first two years of the program 86 percent of the ultra-poor households participated in the enterprise support (which includes asset transfer, training, and cash transfer) and all of them also received the encouragement to save. Hence, there is no independent variation I can explore to disentangle the mediating effects of these four components. On the other hand, 54 percent of the ultra poor received a sanitary latrine and/or tube well, 34 percent received health support, 7 percent received help in paying schools fees, and 30 percent received some other type of support (e.g., assistance with paying costs to transport livestock to/from the nearby market). After the first two years of the program were over and the treated ultra-poor households "graduated" from the program, they were invited to participate in BRAC's mainstream microfinance program. Of the ultra-poor households in the treatment group, 33 percent ended up participating in BRAC microfinance. In addition to these components, which are directly implemented by BRAC, 15 percent of the ultra-poor households received some sort of support from the GDBCs that were established in treatment villages.

Table S1.11 in the supplementary online appendix displays the results of the mediation analysis. The first row in each panel replicates the baseline ITT estimates (specification (1)). The second row estimates the same ITT specification but also controls for the mediators. The difference between these estimates corresponds to the total mediated effect, shown in the third row. The remaining rows then show how much each mediator contributes to explaining this mediated effect (assuming no complementarity between mediators). Three findings emerge: First, having received enterprise support from BRAC (which combines asset transfer, training, cash transfers, and encouragement to save—since there is no independent variation in participation in these components) is a significant mediator for crowding out of informal transfers, on both the extensive and the intensive margins. Second, having received health support from BRAC seems to crowd in informal transfers, at least on the intensive margin. Third, having microfinance loans from BRAC is another significant mediator that is associated with crowding out of informal transfers. The findings that enterprise support and microcredit access crowd out informal transfers is in line with the hypothesized effects of these components in the Conceptual Framework. While the positive association between health support of the program and the crowding in of transfers may seem surprising, one explanation for this could be that households who need health support from BRAC possibly need and are more likely to receive support from their social networks as well. Healthcare costs can be rather high in this context and health expenses are likely to be prioritized over other types of needs by network members. While these results are intriguing, they are merely suggestive due to endogeneity of participation in the program components. Further research that opens up the black box of the program is needed to fully understand which components are driving the effects of this multi-faceted program on interhousehold transfers and to pin down the mechanisms behind these effects.[32]

6.3. Comparison of Magnitudes

Previous studies evaluating the effects of public transfer programs on informal transfers have found relatively large crowding-out effects. Studying the effects of an increase in old-age pensions in South Africa, Jensen (2003) finds that a unit increase in the pension amount leads to a 0.25 to 0.30 reduction in private transfers. Juarez (2009) finds that an exogenous increase in the income of the elderly in Mexico City (through a transfer program) led to a 0.33 reduction (per unit of public transfer) in private transfers.[33] Albarran and Attanasio (2002) evaluate the effects of a conditional cash transfer (CCT) program that was implemented in rural communities in Mexico ("Progresa"). They find that the grant, which had an average value of 250 pesos per household[34] reduced net transfers by 140 pesos per beneficiary household.[35] Angelucci and de Giorgi (2012) show that a CCT program implemented in urban Mexico ("Oppurtinades") had a very limited crowding-out effect on monetary transfers received, but reduced the likelihood of in-kind transfers significantly (they don't have data on the value of in-kind transfers). Relative to these estimates, the crowding-out effect I find is smaller. I find a fall of 13 ppt in the likelihood of receiving transfers and BDT 152 decrease in the value of net informal transfers received by the average household that was eligible for the TUP program. This is low, relative to the value of assets transferred by the program (BDT 9,500). It is perhaps more relevant to benchmark the crowding-out effect on informal transfers relative to the effect of the program on annual household consumption. When I estimate the effect of the program on consumption, using specification (1), I find that the program led to an increase of BDT 1,136 in annual per capita consumption (p-value = 0.000). The reduction in informal transfers is therefore small relative to the improvement in economic conditions of ultra-poor households caused by the program.

There are a number of possible explanations for the relatively small crowding-out effect I find on informal transfers. A key distinction between the TUP program and the programs in the aforementioned studies is the multi-faceted nature of the TUP program. As discussed in the Conceptual Framework, many of these components may have opposing effects on interhousehold transfers, limiting the extent of crowding out. Another key feature of the TUP program is that it is a one-off, big push intervention, while the transfer programs studied in previous work entail small but regular cash transfers. It is possible that access to continuing transfers is perceived differently by social networks of the beneficiaries, compared to a one-shot transfer, and therefore their crowding out effects are different. Moreover, the program I study targeted some of the poorest households in rural Bangladesh where out-migration is limited, especially for the poor (Bryan, Chowdhury, and Mobarak 2014). In comparison, the households studied in previous work from South Africa or Mexico had greater access to migrant networks and remittances. Furthermore, the crowding out effect of CCT programs may be different due to the fact that these programs are conditional on investments in child education and health, whereas the TUP program did not entail such conditionalities. In light of this, a more relevant comparison may be with unconditional cash transfer

32 See, for example, Banerjee et al. (2018) for evidence from Ghana on how disentangling some of the components that are bundled together in BRAC's approach may affect the program's impacts on ultra poor's socio-economic status.

33 Juarez (2009) finds a larger, almost one-to-one, crowding-out effect at the individual level. The data does not allow me to distinguish transfers received by different members of the same household, thus I cannot estimate the effects at the individual level.

34 The amount of the transfer varied across beneficiary households, depending on the number of children in the household.

35 Albarran and Attanasio (2003) show that the crowding-out effect was larger in villages where households' earnings were less volatile (i.e., average variance of household income is lower), which is in line with theoretical models of risk-sharing under imperfect enforcement.

(UCT) programs, but there is limited evidence on the long-term impacts of cash transfer programs in general (Haushofer and Shapiro 2018), and on their effects on private transfers in particular. Evaluations of TUP and similar programs find improvements in the socio-economic statuses of targeted households that are sustained in the long run (Banerjee et al. 2016; Bandiera et al. 2017). To the extent that the long-term effects of UCT or CCT programs on the socio-economic statuses of targeted households differ from those of TUP-type programs, their impacts on private transfers and social networks of the beneficiaries are also likely to differ. Future research contrasting the effects of alternative transfer programs on private transfers is needed to shed light on this issue.

7. Conclusion

Faced with highly volatile earnings and limited access to credit and insurance markets, the poorest households in rural economies often rely on transfers from their social networks. It is important to understand how large-scale antipoverty programs targeting poor households within a village affect their access to informal transfers and the structure of transfer networks within targeted communities.[36]

In this paper, I showed that a program that targeted ultra-poor households in rural communities in Bangladesh led to a small reduction in the level of private transfers they received. This crowding-out effect was driven mainly by a reduction in transfers they received from outside the village, while there was a small increase in transfers they received from their neighbors. The program enabled the ultra-poor households to exchange transfers with households from other, wealthier socio-economic classes within their communities. They were more likely to receive and give transfers from/to households who, at baseline, were in higher wealth classes relative to the ultra poor. Finally, the program improved the reciprocity of ultra-poor households' transfer connections while reducing the likelihood that they receive transfers from their members' employers.

The findings show that poverty has a causal effect on households' participation in transfer networks and who they are matched with in reciprocal transfer arrangements. Moreover, the finding that the program reduces the likelihood of transfers from employers demonstrates an interlinkage between insurance and labor contracts, which is particularly relevant for poor workers with limited outside options. This implies that policies affecting either the labor or the insurance market are likely to have impact(s) on the other one. Evaluations of labor and insurance policies in village economies should take such interlinkages into account.

8. List of variables

- "Wealth"—sum of the monetary values of land, livestock, and durable assets owned by the respondent's household.
- "Consumption"—total household consumption expenditure over the previous year divided by adult equivalent household size. The adult equivalence scale gives weight 0.5 to each child younger than 10. The expenditure items covered are food, fuel, cosmetics, entertainment, transportation, utilities, clothing, footwear, utensils, textiles, dowries, education, charity, and legal expenses.
- "Literacy"—a dummy variable equal to 1 if the female respondent reported that she can read and write a letter.
- "Height"—gives the female respondent's height in centimeters.
- "Received any transfers during last year"—a dummy variable equal to 1 if the respondent's household received any transfers in cash or in kind during the 12 months preceding the survey.

36 See also Comola and Prina (2017) and Banerjee et al. (2020) for evidence on the effects of development programs on social networks within rural communities.

- “Value of transfers received”—monetary value of transfers received (in cash or in kind) by the respondent’s household during the 12 months preceding the survey.
- “Gave any transfers during last year”—a dummy variable equal to 1 if the respondent’s household gave any transfers in cash or in kind during the 12 months preceding the survey.
- “Value of transfers given”—monetary value of transfers made (in cash or in kind) by the respondent’s household during the 12 months preceding the survey.
- “Ever receives food”—a dummy variable equal to 1 if the respondent’s household ever receives food transfers from other households.
- “Number of food transfer sources”—number of households (up to a maximum of 3) who are reported as the main sources of food transfers received by the respondent’s household.
- “Ever gives food”—a dummy variable equal to 1 if the respondent’s household ever gives food transfers to other households.
- “Number of food transfer recipients”—number of households (up to a maximum of 3) who are reported as the main recipients of food transfers from the respondent’s household.
- “Fraction of transfer sources within village”—fraction of households who are reported as sources of transfers (food or other) received by the respondent’s household, who live within the same community as the respondent’s household.
- “Fraction of transfer recipients within village”—fraction of households who are reported as recipients of transfers (food or other) given by the respondent’s household, who live within the same community as the respondent’s household.
- “Reciprocity of transfers within village”—fraction of transfer sources within the village who are also reported as recipients of transfers (food or other).
- “Reciprocity of transfers with…”—reciprocity of transfers with different wealth classes gives the fraction of transfer sources of a given wealth class within the village (conditional on having any) who are also reported as recipients of transfers.
- “Received transfers from any employer”—a dummy variable equal to 1 if either the primary female respondent or the male head of the household is working for an employer who lives in the same community and is also reported as a source of transfers for the respondent’s household.

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