Poverty, Occupational Choice and Social Networks:

Essays in Development Economics

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Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others. The second chapter draws on work that was carried out jointly with equal share by Oriana Bandiera, Robin Burgess, Imran Rasul, Munshi Sulaiman and me.

I declare that my thesis consists of 58,623 words.

Selim Gulesci

Abstract

This thesis contains three independent chapters that are aimed towards contributing to our understanding of three questions in the literature on poverty, occupational choice and social networks. The first chapter asks whether labor contracts in a rural economy play a significant role in insuring workers against risks and if the outside options of workers determine the extent to which their labor contracts are interlinked with their insurance arrangements. As such, it provides evidence on a well-established idea in the study of rural labor markets – that of labor-tying – by showing that it is an important channel through which the poor workers smooth their income and that an exogenous improvement in their outside options induces them to exit labor-tying and switch to alternative channels of informal insurance. The second chapter provides evidence on whether transfer of capital and skills enable the poor to permanently exit poverty by entering into higher return occupations. It shows that such a transfer not only transforms the occupational choices of the targeted poor, but has significant general equilibrium effects on the local markets, and corresponding spillover effects on non-targeted households. The third chapter provides evidence on the question "do formal transfers crowd out informal transfers", exploiting the randomized roll-out of a large scale asset transfer and training program to test for its effects on the informal transfer arrangements of the poor. It shows that the informal transfers to the poor are crowded out by the program, but this effect is highly heterogenous depending on the location of the sender and the vulnerability of the targeted poor.

For my parents, Mukaddes and Huseyin Gulesci

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Preface

This thesis contains three independent chapters that are aimed towards contributing to our understanding of three key questions in the literature on poverty, occupational choice and social networks. All three chapters exploit the randomized roll-out of a large scale poverty alleviation program in Bangladesh to provide evidence on these questions.

The first chapter aims to contribute to our understanding of the role that contracts in a rural labor market may play in insuring workers against fluctuations in their income. As such, it provides evidence on a well-established idea in the study of rural labor markets – that of labor-tying. I show that labor-tying is an important channel through which the poor in rural Bangladesh insure themselves against risks. Using a theoretical framework adapted from Bardhan (1983), I analyze the effects of an exogenous increase in the outside options of poor women (through an improvement in their self-employment opportunities) on their and their spouses' participation in tied labor, as well as the general equilibrium effects of the treatment on the terms of the labor contracts in the village. I find that treated women and their spouses are less likely to be in tied-labor contracts. Their wages increase through two channels: (a) due to the switch from tied to casual labor contracts (b) through the general equilibrium effects in the village labor market. Furthermore, I find that the treated households form reciprocal transfer links with wealthier households in the village. These findings imply that poor households may be involved in second-best labor contracts to insure themselves against risks. When their self-employment opportunities improve, they break these ties and move to greater reliance on reciprocal transfer arrangements.

The second chapter is concerned with the question of whether entrepreneurships programs can transform the economic lives of the poor. The world's poorest people lack both capital and skills and are trapped in low return occupations. Whether their economic lives can be transformed by programs which provide both assets and training to enable them to run small businesses is however unknown. To shed light on this issue we conduct a randomized evaluation of an entrepreneurship program that provides assets and training to the poorest women in rural Bangladesh. A simple theoretical model of occupational choice under capital constraints makes clear that the effect of the program on occupational choice is ambiguous because the asset transfer creates a wealth effect that reduces labor supply and time spent running small businesses, while training generally increases both. We derive testable predictions on heterogeneity of the effects on treated households, the general equilibrium effects and the spillover effects on non-treated households. We find that the program transforms the occupational choices of the treated poor women by inducing them to spend more time in self-employment,

less in wage labor and increases their labor market participation, leading to a 36% increase in annual income. Moreover, the program leads to an increase in wages at the village level and its effects spillover to other poor women who experience an increase in labor supply and income.

In the third chapter, I provide evidence on whether unconditional formal transfers lead to a crowding out of informal transfers, and if so which households in the community are most likely to be affected by this. I exploit the randomized roll-out of the ultra-poor program in Bangladesh to test whether informal transfers received by the targeted households are crowded out by the program. I find that treated households experience a crowding out in the informal transfers that they receive. In particular the transfers they receive from within the community are crowded out, and this effect is heterogenous by the degree of vulnerability of the household at baseline – those who had greater food insecurity at baseline are less likely to experience a crowding out in their informal transfers. I provide evidence that this heterogeneity in the crowding-out effect is likely to be caused by an innovative component of the program studied, which entails the establishment of village elite committees to make the local elites target their transfers to the targeted poor that are in greater need.

1 Labor-Tying and Poverty in a Rural Economy: Evidence from Bangladesh

1.1 Introduction

In rural labor markets, a tied-labor contract involves a long-term relationship between an employer and a worker where the employer provides a steady but low wage to the worker (relative to a casual labor contract that offers a high wage rate during the harvest season). The role of labor-tying on terms of labor contracts has been studied extensively in theoretical studies (Bardhan (1983), Eswaran and Kotwal (1985), Mukherjee and Ray (1995)) and the empirical relevance of tied-labor has been shown, particularly in South Asia[1] (Bardhan and Rudra (1978)). In developing countries where poor households face substantial amounts of risk and limited insurance opportunities, labor-tying is likely to be an important channel through which they smooth their income, hence their consumption[2] (Morduch (1995)). Yet recent empirical studies have mainly focused on other mechanisms of consumption-smoothing such as informal insurance and pre-cautionary savings[3]. Using original survey data from Bangladesh, I show that labor-tying is an important mechanism through which poor workers smooth their consumption. Furthermore, I test the effects of an experiment that increases the expected income of the poor women living in rural Bangladesh on their involvement in tied labor. In particular, I show that an exogenous improvement in the outside option of poor workers decreases their participation in tied-labor, and allows them to enter labor contracts with higher return but higher income volatility. This change in the level and composition of labor supply within the village has different general equi-

[1]The existence of tied-labor arrangements have been documented in a variety of settings such as Germany, Egypt, Brazil and Japan in economic history and modern-day empirical studies (Anderson(1990), Lewis and Barnouw (1958), Bhalla (1976), Richards (1979), Smith (1959)). These types of labor arrangements are often characterized by dependency of the worker on the employer in terms of credit, housing and labor opportunities, in turn receiving a lower wage.

[2]The role of implicit insurance in labor contracts is not limited to rural labor markets in developing countries. The idea that a risk-neutral employer may provide a risk-averse worker with insurance against income fluctuations dates back to Knight (1921). Baily (1974) and Azariadis (1975) model the contractual relationship of the employer-worker as an implicit contract model where the entrepreneurs provide insurance to risk-averse workers; and more recently Guiso et al. (2005 and 2010) show that risk-sharing plays an important role on the wage-profile of workers, depending on the degree of financial development in the economy. I contribute to this literature by showing that a similar relationship between an employer and a worker exists in tied labor contracts in rural labor markets and analyzing how an exogenous improvement in the outside option of the worker changes the terms of her labor contracts.

[3]Key mechanisms highlighted in the literature include reciprocal exchange of loans and gifts (e.g. Udry (1994), Fafchamps and Lund (2003)) and pre-cautionary savings (e.g. Paxson (1992), Rosenzweig and Wolpin (1993)).

librium effects on the returns to tied and casual labor in the male and female labor markets within the village. Finally, I provide evidence that suggests that the treated poor households are changing the mechanisms through which they smooth their consumption. In particular, the households that are exogenously made wealthier are less likely to engage in tied-labor arrangements, but more likely to form reciprocal transfer links with other villagers. Taken all together, the findings show that as poor households (exogenously) get richer, they move from second-best labor contracts (that yield a low return but insure them against risks) to more profitable yet riskier income generating activities, accompanied with reciprocal transfer arrangements that help smooth their consumption.

In order to formalize the incentives of workers and employers in entering tied-labor arrangements, I adopt the risk-sharing model of labor-tying developed by Bardhan (1983) where a risk-averse worker enters into a tied labor arrangement with a risk-neutral employer in order to smooth her income during the lean and peak seasons. Alternatively, the worker can choose to settle down for her expected outside option, which will be a function of her wealth and vulnerability (proneness to risks). The model assumes that tied workers and casual laborers are perfect substitutes in the farm production function during the peak season. Hence, the employer's only incentive in offering tied-labor contracts is to ensure supply of cheap labor during the peak season. In equilibrium, it will be the poorest and most vulnerable workers that enter into tied-labor contracts, while better-off workers will choose to remain self-employed and work for the employer as a casual worker whenever the realized village market wage rate exceeds their expected outside option. This automatically implies that casual workers will receive a higher wage rate on average.

I use this theoretical framework to test the effects of an exogenous increase in the outside option of poorest workers on their participation in tied labor and on the terms of labor contracts in the village economy. The exogenous variation I exploit is the randomized roll-out of the "ultra poor" program in Bangladesh. The "ultra poor" program was pioneered by BRAC[4] and targets the poorest women living in villages. It involves a combination of a large asset transfer (livestock or trees), enterprize training and weekly visits by program officers to ensure that the treated females are able to generate income from the assets that they receive. In short, the program improves the self-employment opportunities of treated women. The data used in this paper comes from the randomized evaluation of BRAC's ultra poor program in Bangladesh. The

[4]BRAC, formerly known as "Bangladesh Rural Advancement Committee", is originally a Bangladesh-based NGO. Today, it has operations in a number of countries in South Asia and Africa and in terms of the number of people it employs it is the largest NGO in the world.

program identifies the poorest females living in rural villages, who are often landless laborers. They rely primarily on finding work as agricultural day-laborers or maids, and on the transfers they receive from the rest of the community. This is a setting where seasonal fluctuations in wage earnings are very significant (see Figure 1) and a large proportion of the targeted poor households enter into tied-labor contracts that provide a smoother income profile but lower average wage.

The theoretical model gives the following predictions with respect to an exogenous shock to the outside options of the poorest workers in the economy:

1. In partial equilibrium (assuming there is no effect on the returns to tied or casual labor)

   (a) Treated workers will be less likely to be working for a wage. This depends on two factors: (i) whether the amount of increase in the outside option of the treated worker is large enough (ii) the initial level of the outside option of the worker.

   (b) Conditional on remaining in wage-employment, treated workers will be less likely to be in tied-labor contracts and more likely to be in casual labor contracts.

2. In general equilibrium, depending on how the program affects the aggregate distribution of workers' outside options, wages for both tied and casual laborers may increase. In that case, the threshold level of outside option below which workers enter into tied contracts also increases.

3. A corollary of prediction (2) is that the effect of the program on whether treated workers remain in wage-work and the type of contracts they enter will be ambiguous in general equilibrium. The direct effect on their outside options and the GE effects through the labor market have opposing effects on their labor market participation.

4. Finally, if workers are matched assortatively by their outside options in reciprocal transfer arrangements, then treated workers will be more likely to enter reciprocal arrangements with wealthier workers to smooth their consumption. This will increase their likelihood to switch from tied to casual labor contracts.

In order to test the predictions of the model empirically, I make use of two key characteristics of the evaluation strategy: First, in order to identify tied and casual workers empirically, I use data on the identity of workers' employers and their food

transfer links. The data is unique in the sense that for every business activity that the respondents were engaged in, they were asked to report the identity of their employer and as long as the employer was within the same village (as the respondent), their household ID number was recorded. Similarly, respondents were asked to identify the most important 3 households they would borrow food from at times of need. Using these two pieces of information, I can identify which employers were also a borrowing source for the worker: 25% of the poor workers report their employer as a source of food transfers in times of need. I show that this definition of tied labor contracts also correlates with having lower average wage rate and lower wage volatility, in line with the definition of tied labor contracts in the theoretical framework[5].

Second, in order to identify the direct effects of the program on the treated households and the indirect spillover effects on non-treated households via the labor market, I make use of the fact that the program was randomized at the village level and the sample includes both treated and non-treated workers in treatment and control villages. Comparison of treated workers in treatment villages to those workers that were selected for treatment but were not treated in control villages (henceforth "selected workers") allows me to identify the direct effect of the program combined with any indirect general equilibrium effects. By comparing the non-treated workers in treatment villages to the relevant group of workers in control villages, I identify the general equilibrium effects of the program on the rest of the community.

I start by analyzing the effects of the program on the treated women. I find that the program has a negative impact on the participation of treated females in the female labor market in the village. They are 10% less likely to be working for another household in the village at followup relative to eligible women in control villages. This suggests that there is an overall fall in the labor supply in the female labor market in the village. In line with prediction 2, conditional on being in wage employment, treated females are 20% less likely to be in tied-labor contracts. Hence there is a greater fall in the supply of tied female workers relative to casual workers. Furthermore, this suggests that the direct effect of the program on the outside options of treated women dominates any indirect GE effects through the labor markets.

[5]Classification of agricultural laborers into categories of "attached" and "casual" is common practice in India. This classification was adopted in the First (1950-51) and the Second (1956-57) Agricultural Labor Enquiries. Yet the distinction between attached and casual laborers was often not clear (see Thorner (1956) and Raj (1962) for criticism of the ambiguous distinctions between attached and casual workers in the Agricultural Labor Enquiries of India). Bardhan and Rudra (1978) use village survey data from different parts of India to show that consumption loans play a big role in labor tying. In 61 to 92 per cent of the cases from different parts of India, tied workers (whom Bardhan and Rudra (1978) refer to as "farm servants") report taking consumption loans from their employers. This is in line with the definition of tied labor I use in my empirical strategy where I identify tied labor contracts as the ones in which the employer is a source of transfers for the worker at times of need.

Next, I test predictions 1 and 2 on the spouses of treated females. Although the program is targeted to females, male members of the treated households are also likely to be affected by the increase in the self-employment opportunities of their spouse. At the same time, labor markets are highly segmented by gender in this setting, thus the GE effects of the program are likely to be different on the male and female labor markets[6]. In fact, I find no effect on the participation of male workers living in treated households in wage employment. On the other hand, conditional on being in wage-employment, males in treated households are 8.5 percentage points (43%) less likely to be in tied-labor contracts.

In order to test prediction 3 on the GE effects of the program, I use the sample of non-treated female workers. Women in non-treated households do not experience the direct increase their outside options through the program, but will be affected by any GE effects through the female labor market. I find that the hourly wage rate of non-treated female workers who live in treatment villages increases by 16% relative to its baseline level and relative to control villages. Further examination of this effect shows that the increase is coming from casual workers' wages, while the workers that are in tied labor contracts do not experience any increase in their wage rate. The latter finding is not in line with prediction 3, which predicts an increase in tied-labor wage rate as well as an increase in casual labor wage rate. Moreover, there is no effect on the proportion of non-treated female workers who are in tied contracts, which suggests that the program had no impact on the threshold level for tied contracts.

In order to test GE effects on the male labor market, I restrict the sample to male workers living in non-treated households. There is a small positive effect on the wage rate of non-treated male workers: their hourly wage increases by 4%. Further examination shows that this effect is coming mainly from an increase in the wage rate of males under tied-labor contracts. Their average hourly wage increases by 12%, while the effect on wages of casual laborers is only 2% and imprecisely estimated. This is in line with previous findings on the type of labor contracts of treated men: the program lowers the aggregate supply of tied male workers in the village, putting a pressure of the male tied wage rate. There is no similar effect on the wage rate for male casual labor.

Finally, in order to test prediction 4 on the involvement of treated workers in recip-

[6]The male and female labor markets in this setting are rather distinct. In conjunction with findings of Foster and Rosenzweig (1996) in rural India, males are more likely to be involved in physically-demanding jobs, while females often work in jobs that require less physical strength, such as sowing seeds, taking care of livestock, working as a maid etc. Furthermore, hourly wage rate for males is much higher compared to that of females (average hourly earning of a male worker is 59% higher at baseline relative to that of a female worker). Due to these reasons, I analyze the effects on male and female labor markets separately.

rocal transfer arrangements, I construct measures of wealth and reciprocity of their food exchange links. I find that treated households form food exchange links with households that are on average 6% wealthier (wealth measured at baseline so that any effect of the transfers of the program are not included). This implies that an exogenous increase in the income of an agent allows her to form food exchange links with richer households[7]. Furthermore, I find that the treated households are more likely to engage in reciprocal transfers with other households where they reciprocate food transfers in times of need with transfers out to their neighbors (a greater proportion of their food borrowing links are reciprocated by food lending). This suggests that as poor agents get wealthier (in this case by having better opportunities in self-employment and thus higher income in a given period) they change the mechanism through which they smooth their consumption: they switch from entering low-return occupations that yield a low but steady income to informal insurance mechanisms where they exchange transfers with other households.

A striking finding is that although the supply of both male and female tied workers is decreasing as a result of the treatment, only the returns to male tied labor is increasing significantly while the returns to female tied labor is unchanged. I provide evidence on two alternative mechanisms that may explain while the demand elasticity for female tied labor may be lower than that for the male tied labor: First, I consider the availability of substitutes for male and female tied labor in employer households. I find that female members of employer households that hired tied female workers at baseline spend significantly more time doing household chores at followup, which suggests that the employers are substituting hired female tied labor with female family labor. Descriptive statistics show that female household members in employer households work fewer hours relative to the male household members (less than 50% on average). This implies that the opportunity cost of time for female family labor is lower in employer households relative to that of male family labor. As a result, employer households are likely to substitute female tied workers with household labor, but may choose not to do so for male tied workers. This would explain why the elasticity of demand for male

[7]Genicot (2006) proves that in informal insurance arrangements under heterogeneity in permanent income and limited commitment, positive assortative matching can be stable. Fafchamps and Gubert (2007) find that the wealth difference between two households (as well as age and geographic distance) is negatively correlated with whether two households have a risk-sharing link with each other. De Weerdt (2004) carries out a similar analysis using data from a village in Tanzania, but finds that difference in wealth (particularly livestock value) increases the probability that either of two households report one another as someone they can rely on in times of need. Attanasio et al. (2009) show in an experimental set-up that individuals with similar risk attitudes and those that are connected via family links are more likely to form risk-sharing groups with each other. I contribute to this literature by showing that an exogenous increase in the income of an agent makes him/her more likely to enter into reciprocal exchange links with wealthier agents.

tied workers is lower compared to that for female tied workers.

Second, it is likely that there is a differential importance of non-wage benefits for tied female workers relative to the males. Women in rural Bangladesh often face difficulty accessing services that require them to interact with others in social spaces (there is a large sociological literature on the institution of "purdah" in South Asia that tries to prevent women from being seen by men). As such, women are more likely to need their employer's assistance to guarantee access to services in institutions such as the health centers, markets, courts etc. I find that conditional on visiting a list of such institutions, female tied workers were significantly more likely to receive assistance from others at followup. This potentially implies that the female tied workers receive better assistance from their employers in accessing services. If such assistance is part of the compensation from a tied-labor contract, then instead of receiving a higher wage rate (like the male tied workers) they may be receiving a higher level of assistance from their employers.

The rest of the paper is organized as follows: Section 1.2 presents the theoretical framework and the implied predictions for the study at hand. Section 1.3 describes the setting of the study, the intervention and the characteristics of the data. Section 1.4 presents the empirical findings. Section 1.5 concludes.

1.2 Theoretical Framework

In this section, I use the simple risk-sharing model[8] of tied-labor based on Bardhan (1983) to derive predictions on the effects of the ultra poor program[9]. I extend the model to an infinite-horizon setting, in order to allow for comparability with reciprocal transfer arrangements a la Coate and Ravallion (1993). The theoretical framework provides predictions on both the direct and the general equilibrium effects of the program through the village labor market. These predictions will guide the empirical analysis in section 1.4.

1.2.1 Set-up

Preliminaries: There are two types of agents in the economy: a continuum of size $N > 1$ of landless workers and a unit measure of landowners who employ labor. Time is

[8]See Ghatak (2010) for a simple expository version of Bardhan's (1983) model.

[9]In this study, I adopt the Bardhan (1983) framework, however the results would be similar under an efficiency wage approach similar to Eswaran and Kotwal (1995). The key distinction between the two models is that in Eswaran and Kotwal (1995) a tied labor contract provides greater total utility through giving the worker a long-run steady income and employers use tied labor contracts to give incentives to workers in tasks where effort is harder to monitor. As I do not observe the productivity of individual workers, I can not test directly whether the assumptions of the Eswaran and Kotwal (1995) model hold in the data. I test whether tied workers receive higher total utility in terms of income, pce and calories consumed and find no significant difference between tied and casual workers.

infinite with periods alternating between two stylized "seasons". Every even numbered period, $t = 0, 2, 4...$, is a fallow season in which there is no cultivation and hence no employment opportunities for workers. Every odd numbered period, $t = 1, 3, 5...$, is a peak season with demand for labor on the employer's farm. Workers and landowners discount the future at common rate $\beta \in (0, 1)$.

Landowners: Production is stochastic with the labor requirement for each landowner in a peak season in period t being $L_t = A_t x$ where x is the land owned by each employer. All employers are assumed to be identical in their land holdings. The realization of A_t is stochastic and has finite support on $[0; \bar{A}]$ with (right continuous) distribution function $F(A)$ and $E(A) = 1$. The shock is perfectly correlated across all landowners within a season but *iid* over time.

Workers: There are N workers in the village economy. Worker i's lifetime utility is given by

$$E \sum_{t=0}^{\infty} \beta^t u(c_t^i), \quad \forall i \in \{1, \ldots, N\} \tag{1}$$

where $u()$ is increasing, twice continuously differentiable and strictly concave.

Workers differ in their outside options, which I will interpret as their self-employment opportunities in this context[10]. The outside option of each worker is stochastic and depends on the state of the world that is realized in period t. If the state of the world in period t is "good", the worker receives a payoff y_i: However, with probability p_k the state of the world is "bad" and the worker receives 0. Hence each agent is index by (i, k). p_k is indexed such that higher k means higher p_k so that $0 < p_1 < p_2 < \cdots < p_N < 1$. p_k can be interpreted as the vulnerability of the worker where a higher p_k implies that the worker is more prone to risks. This implies that the expected utility of worker (i, k) in autarky (self-employment) will be: $(1 - p_k) \cdot u(y_i) + p_k \cdot u(0)$. Without loss of generality, I normalize $u(0) = 0$ so that the expected outside option of agent (i, k) is $(1 - p_k) \cdot u(y_i)$. Let $\tilde{y}_k^i$ denote the expected outside option of agent (i, k) so that $\tilde{y}_k^i = (1 - p_k) \cdot u(y_i)$. Furthermore, I assume that the cumulative distribution function of $\tilde{y}_k^i$ amongst the agents is given by $G(\tilde{y}_k^i)$.

Equilibrium concept: In each productive season, the wage is competitively determined by the forces of supply and demand. A stationary competitive labor market equilibrium is a wage function $W(A)$ such that labor demand and labor supply are

[10]More generally, any source of income that is alternative to working for the employer is part of the outside option of the worker. For example, within the theoretical framework, opening up of a factory that employs the workers at a steady wage in both seasons would yield to a similar increase in the outside options of the workers as an increase in their self-employment opportunities.

equated for each realization of $A \in [0; \bar{A}]$. Each worker and landowner takes the function $W(A)$ as given and optimizes accordingly. In equilibrium, workers' and landowners' beliefs about $W(A)$ are fulfilled, i.e. there are rational expectations.

1.2.2 Labor Demand

A landowner can offer two types of contracts to his workers: tied-labor contracts and casual-labor contracts.

In a tied-labor contract the employer pays a fixed amount z every period to the worker, while the worker in exchange commits her labor to the employer in both peak and lean seasons (i.e. she cannot pick up any alternative employment opportunities while she's in a tied labor contract)[11].

In a casual labor contract the employer will have to pay the competitively determined wage rate $W(A)$ which depends on the realized productivity shock.

Let ℓ_t be the number of tied laborers hired by the landowner. The net profit of the employer in each peak-season period will be given by[12]:

$$\pi_t = \left\{ \begin{array}{ccc} A_t x - z\ell_t & if & A_t x \leq \ell_t \\ A_t x - z\ell_t - (A_t x - \ell_t) \cdot W(A_t) & if & A_t x > \ell_t \end{array} \right\} \tag{2}$$

The decision to hire tied laborers is made ex ante, before the realization of A. Since the landowner's problem is stationary, this will be fixed over time.

Thus

$$\ell^* \in \arg\max_{\ell \geq 0} \left\{ \frac{\beta}{1-\beta^2} x - \frac{z\ell}{1-\beta} - \frac{\beta}{1-\beta^2} \int_{\frac{\ell}{x}}^{\overline{A}} (Ax - \ell) W(A) \cdot dF(A) \right\} \tag{3}$$

Note that the landowner is taking the spot wage function $W(A)$ as given. The first

[11] The assumption that the employer pays z to the worker in the lean season, although the worker doesn't do any farm work during this season is one that simplifies the analysis. One can easily modify the model so that during the lean season tied workers do some non-farm work (e.g. household work) for the employer that doesn't contribute directly to the farm production in the peak season, and the results would be unchanged

[12] Note that in the case when $Ax \leq L_t$, (2) implies that the employer may make a loss in a given peak-season period. This is because his decision on how many tied workers he will hire is based on his lifetime profits. The number of tied workers he hires/pays for need not yield non-negative profits in a given period, if the realization of A is too low.

order condition for 3 yields:

$$\frac{z}{1-\beta} = \frac{\beta}{1-\beta^2} \int_{\frac{\ell^*}{x}}^{\overline{A}} W(A) \cdot dF(A) \tag{4}$$

This gives the time invariant demand for tied labor, ℓ^*, as a function of the wages z and $W(A)$. The latter will be determined in market equilibrium.

1.2.3 Labor Supply

Workers decide whether to enter into a tied-labor contract or to remain self-employed at date 0.

If worker (i,k) enters into a tied labor contract with a landowner, she receives z in every period from the landlord, in return to committing her labor to the employer in both peak and lean seasons (i.e. she is bound not to undertake any alternative employment opportunities while she's in a tied labor contract).

If she chooses to remain self-employed, then she can choose to work for the employer (under a casual contract) in any peak period where the realized spot wage rate $W(A)$ is such that the utility from becoming a casual worker, $u(W(A))$ exceeds her expected utility from remaining self-employed, $\widetilde{y}_k^i$. I assume that the productivity shock A_t is realized before the worker makes her decision between being a casual worker or remaining self-employed[13].

In order to simplify the analysis, I make the following assumption:

Assumption 1: There are no alternative insurance mechanisms available for the workers so that any worker (i,k) that doesn't enter tied-labor is forced to self-insure.

I will relax Assumption 1 further on by considering reciprocal transfer arrangements among workers.

Workers whose expected outside options satisfy the following inequality will choose to enter into tied-labor contracts:

$$\frac{u(z)}{1-\beta} \geq \frac{\widetilde{y}_k^i}{1-\beta^2} + \frac{\beta}{1-\beta^2} \cdot E[\max\{\widetilde{y}_k^i, u(W(A))\}] \tag{5}$$

[13]Note that workers are heterogenous *ex ante* in terms of their expected outside options, but once they decide to enter into a labor contract (either tied or casual) with the employer, they are homogenous as the marginal product of each worker is the same. This *ex post* (conditional on entering into the labor market) homogeneity of workers is the economic intuition behind imposing equal z and $W(A)$ levels for any tied and casual worker respectively.

where expectations on the right hand side are taken with respect to A. The left-hand side of (5) is the life-time utility from entering tied-labor. Alternatively, during every even numbered period (lean season) she receives her expected outside option, $\tilde{y}_k^i$, and in every odd numbered period she may choose to work as a casual laborer if her utility from the realized spot market wage rate (W) is higher than her expected outside option. As long as the expected utility from self-employment ($\tilde{y}_k^i$) satisfies (5) in period 0, it will be optimal for the worker to enter a tied-labor contract and to remain a tied worker thereafter.

The level of $\tilde{y}_k^i$ that satisfies (5) with equality will be denoted as $\widehat{y}$. This will depend on labor market conditions as expressed by the payment for tied labor, z, and the wage function for casual labor $W(A)$.

The supply of workers who want to be in tied-labor contracts is then given by all those whose outside option is below this critical threshold. This defines labor supply into tied labor as:

$$S = NG(\widehat{y}) \tag{6}$$

As with the demand for tied labor, this is time invariant.

1.2.4 Equilibrium in the Labor Market

The equilibrium wage function can now be determined using a fixed point argument based on equating labor demand and labor supply for tied labor along with the decision of non-tied laborers to be self-employed or casual laborers. Given any wage function and value of z, we must have that:

$$NG(\widehat{y}) = \ell^* \tag{7}$$

However, both sides of this depend on the shape of the wage function $W(A)$, which is determined ex-post. We now turn to this.

Consider any peak season ($t = 1, 3, 5 \dots$). There are two cases to consider.

If $A_t x \leq \ell^*$, there is no demand for casual workers in the spot market and the casual wage falls to zero. Thus $W(A) = 0$ for all

$$A \leq \frac{NG(\widehat{y})}{x} \tag{8}$$

In this case, spot workers earn their outside options.

Now consider what happens when $A_t x > \ell^*$. In this case, there is positive demand for spot labor. However, the market wage needs to clear the labor market. Suppose that $W(A) > u^{-1}(\widehat{y})$. Then the wage must solve:

$$A_t x - \ell^* = N\left[G\left(u\left(W\left(A\right)\right)\right) - G\left(\widehat{y}\right)\right] \tag{9}$$

or:

$$W\left(A\right) = u^{-1}\left(G^{-1}\left(\frac{A_t x - \ell^*}{N} + G\left(\widehat{y}\right)\right)\right) \tag{10}$$

Thus:

$$W(A) = \begin{cases} u^{-1}\left(G^{-1}\left(\frac{A_t x - \ell^*}{N} + G\left(\widehat{y}\right)\right)\right) & \text{if } Ax > \ell^* \\ 0 & \text{otherwise} \end{cases} \tag{11}$$

(Observe that $W(A) > u^{-1}(\widehat{y})$ as hypothesized.)

Now we can solve for the equilibrium. Using (4), (5), (9) and plugging in $\ell^* = NG(\widehat{y})$, we have that

$$\frac{z^*}{1-\beta} = \frac{\beta}{1-\beta^2}\int_{\frac{NG(\widehat{y})}{x}}^{\bar{A}} W^*(A)dF\left(A\right) \tag{12}$$

$$\max\{0,\ A_t x - NG\left(\widehat{y}\right)\} = N\left[G\left(u\left(W^*\left(A\right)\right)\right) - G\left(\widehat{y}\right)\right] \tag{13}$$

$$\frac{u\left(z^*\right)}{1-\beta} = \frac{\widehat{y}}{1-\beta^2} + \frac{\beta}{1-\beta^2}\left[F\left(\frac{NG(\widehat{y})}{x}\right)\widehat{y} + \int_{\frac{NG(\widehat{y})}{x}}^{\bar{A}} u(W^*(A))dF\left(A\right)\right] \tag{14}$$

This gives three equations in three unknowns: $\widehat{y}$, z^* and $W^*(A)$. It is the properties of these equations which are of interest.

1.2.5 Comparative Statics

In this section, I consider the effects of an exogenous increase in the outside options of a group of workers at the bottom of the distribution (of outside options) in the village (such as the ultra poor program). At the individual level, the program shifts the outside option of a treated worker upwards. At the aggregate level, it potentially changes the shape of the distribution function $G()$.

First, in partial equilibrium (assuming that z; $W(A)$ and $\widehat{y}$ remain unaffected), the rise in the outside option of worker (i,k) implies that her labor supply into wage work may be affected in two different ways: (i) If the program moves her expected outside option above the utility from casual wage-work, $u(W(A))$, she will choose to remain self-employed and not enter into any wage-work. (ii) If she was employed in a tied

contract, she may switch to a casual contract instead, if the program moves her outside option above $\widehat{y}$ but below $u(W(A))$. Both of these effects will be more likely for workers that had higher expected outside options (were closer to the threshold $\widehat{y}$) to start with.

Of course in general equilibrium, the shift in the distribution of outside options of workers in the economy may lead to a change in the wage level(s) and the threshold level to enter into tied contracts. To analyze this, I consider the effect of a second order stochastic shift in the distribution of outside options. Thus, I index the distribution function by λ where:

$$\left\{ \begin{array}{ll} G_{\lambda}(y;\lambda) \leq 0 & if\ y \leq \widetilde{y} \\ G_{\lambda}(y;\lambda) \geq 0 & if\ y \geq \widetilde{y} \end{array} \right\} \tag{15}$$

for some $\widetilde{y} \in (0, \widehat{y})$. Figure 3 demonstrates the effect of λ on the distribution of outside options graphically. The line AB corresponds to the distribution of outside options before the shift, and $A'B$ to the distribution after the shift.

We are interested in the effect of a shift λ of the form (15) in the distribution of outside options, $G(.)$, on the equilibrium levels of $W^*(A)$, c^* and $\widehat{y}$. For simplicity, I assume that A_t is always high enough so that the spot labor market is active. This implies that the first term in (13) will always be non-zero. In practice, casual contracts are abundant in the harvest season, hence focusing on this case is not a farfetched assumption.

Proposition 1 *If* $u(W(A)) \leq \widetilde{y}$ *then* $\frac{dW}{d\lambda} \geq 0$, $\frac{dz}{d\lambda} \geq 0$ *and* $\frac{d\widehat{y}}{d\lambda} \geq 0$.

Proposition (1) implies that as long as the highest outside option among treated workers before the shift was at least as large as the utility from casual employment, the shift in distribution of outside options will weakly increase wage rates for both tied and casual contracts. If $u(W(A)) \leq \widetilde{y}$, then the aggregate impact of the program lowers the supply of both treated and casual workers, which leads to a rise in wages of both types of workers. On the other hand, if $u(W(A)) > \widetilde{y}$, this is not necessarily the case. The increase in tied and casual wage rates have opposing effects on the threshold level $\widehat{y}$. Proposition (1) implies that the final effect is a rise in the threshold level.

Corollary 2 *The total effect of the program on participation of treated workers in both tied and casual labor is ambiguous. If any treated workers switch from tied to casual contracts, they are likely to be those that had higher outside options to start with.*

Corollary (2) follows immediately from Proposition (1) and the previous discussion on partial equilibrium effects of the program. The increase in the outside option of treated workers induced by the program implies that (in partial equilibrium) they will

reduce their labor supply into wage-work, and will be likely to quit tied contracts for casual ones. On the other hand, the GE effects of the program imply that the rise in casual wage rate will increase the attractiveness of wage-work for treated workers. Moreover, the resulting increase in $\widehat{y}$ implies that it is ambiguous whether in general equilibrium, any treated workers will make the transition from tied to casual contracts. However, if any treated workers make this transition, it will be the ones that had higher outside options and hence were closer to $\widehat{y}$ to start with.

To summarize, the predictions of the model on the effects of an exogenous improvement in the outside options of a group of workers are as follows:

Prediction 1: In partial equilibrium

- **(i)** treated workers will be less likely to be working for a wage;
- **(ii)** treated workers will be less likely to be in tied-labor contracts and more likely to be in casual labor contracts. Treated workers that had higher outside options and thus were closer to $\widehat{y}$ to start with will be more likely to make the transition from tied to casual employment.

Prediction 2: In general equilibrium, wages for both tied and casual laborers may increase. In that case, the threshold level of outside option below which workers enter into tied contracts will also increase.

Prediction 3: A corollary of prediction (2) is that the effect of the program on whether treated workers remain in wage-work and the type of contracts they enter will be ambiguous in general equilibrium.

Until now, the outside option of worker (i, k) was assumed to be self-insurance. In other words, if worker (i, k) does not enter a tied-labor arrangement, then her consumption is determined by her individual income alone. In practice, there may be alternative mechanisms that the workers can engage in to smooth their consumption (e.g. formal insurance, pre-cautionary savings, reciprocal transfer contracts with other villagers etc.). As long as the access of worker (i, k) to such alternative mechanisms is increasing in her outside option (i.e. self-employment opportunities), the predictions of the model will be reinforced. For example, if workers with higher outside options can insure themselves better by purchasing formal insurance from an NGO, then they will be less likely to enter into tied-labor contracts relative to workers with low outside options. An increase in outside option of poor workers will enable them to switch from tied-labor to casual labor more readily as they will now be able to insure themselves against the risks associated with a casual labor contract by purchasing formal insurance.

An important alternative mechanism that has received a lot of attention in the literature is informal insurance (Coate and Ravallion (1993)) where agents may enter into reciprocal transfer arrangements with one another. Kocherlakota (1996) and Ligon et al (2002) derive the terms of informal insurance contracts under limited commitment where any contract has to be ex post preferred by the agent to autarky. Genicot (2006) extends the analysis to allow for heterogeneity in permanent income (or wealth) of agents and shows that depending on the type of correlation between the income shocks received by the agents, positive assortative matching may be stable in equilibrium.

In terms of the model outlined above, allowing workers to enter reciprocal transfer arrangements will increase their outside options and make tied-labor less attractive. However, if workers are matched assortatively so that workers with higher expected incomes choose to enter into reciprocal transfer arrangements with richer workers, the effect of reciprocal transfer arrangements on the outside options of workers will be decreasing in the outside option of the workers (i.e. poorer workers will not be able to insure themselves as good as the rich workers via informal insurance mechanisms). In this case, the predictions of the model will be the same as before.

Prediction 4: *Effect on Insurance Arrangements* - If workers are matched assortatively by their outside options in reciprocal transfer arrangements, an increase in the outside option of worker (i, k) will enable her to enter into reciprocal transfer arrangements with wealthier workers and increase her expected utility from such contracts. As a result, treated workers will switch from tied-labor to reciprocal transfer arrangements.

If workers are matched assortatively by their outside options in reciprocal transfer arrangements, then treated workers will be more likely to enter reciprocal arrangements with wealthier workers. Having richer partners implies the expected outside option of the worker during the lean season will be even higher - both because their individual outside option is improved by the program and they can insure themselves more effectively via reciprocal transfer arrangements with wealthier partners. Hence their likelihood to switch from tied to casual labor contracts will be even higher than in the case where outside option is limited to self-insurance.

1.3 Data Description

1.3.1 Setting of the Study and Survey Design

The data used in this study comes from a data collection exercise implemented in order to evaluate the effects of BRAC's ultra poor program in Bangladesh. BRAC's ultra

poor program is a multi-faceted package that aims to lift the poorest women in rural Bangladesh out of poverty. It combines asset transfer, skills training, weekly support visits, a savings scheme, health support, and training on legal, social and political rights[14]. The most important aspect of the program for this study is that it improves the self-employment opportunities of treated households. In order to do so, the program combines a large asset transfer, with enterprize training and weekly visits by BRAC officers to ensure that the beneficiaries are able to generate income from the assets that they receive.

The evaluation strategy was designed to exploit the roll-out of the program across the country. The timing of the roll-out was randomly chosen at the branch office level. A branch office covers a large area with a radius of approximately 4km. The ultra poor program determined 40 branch offices that would implement the program. Standard procedures to identify who would be the beneficiaries of the program were carried out in all 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, the rest in 2011. All villages in treatment branches were treated in 2007.

In order to identify the poorest females in rural Bangladesh, the program carries out a detailed procedure: First, prior to asset transfer, the program identifies a village, or cluster of households that form a natural geographical unit. These villages consist of 387 individuals that live in 90 households on average[15]. There are 1409 such villages in the sample.

The program carries out a participatory wealth ranking exercise in every village during which the community allocates every household in the village into 5-6 wealth ranks. For the purpose of the current study, I aggregate these wealth ranks into 3 groups: the bottom rank (henceforth "the poor"), the middle classes and the top wealth rank. After further assessment of their demographic and economic characteristics, the program selects roughly half of the households in the bottom wealth rank to be treated while the rest of the poor remain untreated based on certain pre-determined criteria[16].

[14]Further details on the components of the program are provided in Section 2.2.

[15]Due to the high population density in rural Bangladesh, "village"s in administrative terms are often contiguous. The villages as defined by the program and used in this study are smaller than an administrative village, but they form a natural social and economic unit. For example, when respondents were asked to report up to 3 households they would borrow food from if they ever faced food shortage in their household, on average 93% of the links they reported were within the same cluster that was defined by the program

[16]There are three exclusion criteria, all of which are binding. Households who are already borrowing from an NGO providing microfinance, who are recipients of government anti-poverty programs, who have no adult women in their members, are excluded from the program.To be selected a household has to satisfy three of the following five inclusion criteria: (i) total land owned including homestead is not more than 10 decimals; (ii) there is no adult male income earner in the household; (iii) adult women in the household work outside the homestead; (iv) school-going-aged children have to work; and (v)

This selection procedure and all the steps of identification outlined above were carried out in the same way in treatment and control villages.

In every village that was part of the study, an initial census of all households was carried out between April and December 2007. This census allows me to identify the identity as well as wealth, occupation, education and demographic characteristics of all the households that live in any village that is part of the study. This is an essential component of my identification strategy, as it allows me to identify characteristics of every household that the respondent interacts with within the village.

Following the census of all households in the village, a detailed questionnaire was carried out on a smaller sample that included all poor households and a random sample of the rest of the village. Households in this sample were surveyed at baseline (between April and December 2007) and two years after (April-December 2009). The poor households eligible for treatment were selected at the same time in both treatment and control branches, using the same method outlined above. The only difference between them is that the poor in treated branches receive the assets immediately whereas the poor in control branches will receive them in 2011. Selected households and non-selected households in treatment and control villages were not significantly different in terms of observable characteristics at baseline. Table 14 in the Data Appendix provides the normalized differences of key characteristics of households in treatment and control villages by their wealth class and selection status. Normalized differences along the key observable baseline characteristics of treatment and control households from all wealth ranks are less than the benchmark level of 0.25.

The survey questionnaire measures a rich set of individual outcomes, including occupational choices, income and expenditure, business and household assets, health, business skills, and empowerment. It also contains questions on social and economic networks of the household, related to each outcome. The main survey modules were directed towards the main female in the household, as the program is targeted towards women. In cases where the main female was different from the household head, the household head was also surveyed for the business activities and land modules.

Respondents are asked to list all the households they interact with in each of the surveyed activities, thus for instance in the business activities module, the respondent lists all the households he/she works for. For respondents that reported employing other households, only one worker was reported per business activity. This implies that for employment links I can identify all employers of worker households, but I can not identify all workers of employer households. That is why for the majority of the results that follow, I will be considering the effects of the program from the workers'

the household has no productive assets.

perspective[17].

Food exchange is a very important form of interaction in this setting. At baseline, more than 90% of the poor households report receiving food transfers from other households at times of need. The question used to identify the food transfer links is the following: "Does your household ever borrow/lend rice or other food items from/to other households?" If the answer to this question is "yes", the respondent is asked "If you had to borrow food from another household, who would be the main 3 households your household would normally ask for rice or other food items"[18]. Furthermore, the respondents were asked whether they were expected to pay back the amount of food borrowed (or whether they expected to receive to be repayed for the food they have lent to others). 78% reported that returning the food was state-contingent (i.e. depended on whether they could), 9% said they would return the food borrowed whenever they could and the rest said they did not have to return it. This shows that these types of relationships are mainly state-contingent, similar to informal insurance links reported for loans by Udry (1994). Moreover, 99% of the respondents said they never had to pay interest for these food borrowing transactions.

1.3.2 Characteristics of Ultra Poor at Baseline

Table 1 gives summary statistics on the key characteristics of the ultra poor households and the rest of the village at baseline. Column (1) provides the descriptive statistics for the poor households that were selected to be beneficiaries of the program (henceforth "the ultra poor"), column (2) for the other poor households that were ranked at the lowest wealth rank by the community but not selected by the program (henceforth "the other poor"), column (3) for the households that were ranked in the middle wealth ranks (between the lowest and the top rank) and column (4) for the wealthy households chosen to be in the top wealth rank by the community. The first row in Table 1 provides the wealth (defined as total value of household assets including land, livestock, other productive assets and household durables) of households in each wealth rank. As a reality check, one can see that the community's grouping of the villagers into wealth groups matches with the relative wealth of households in each rank. Strikingly, the poor that were selected by the program have on average 40% as much wealth as the households chosen as poor by the community but not selected by the program

[17]The respondents also reported any links they have in terms of family, land, credit, asset sales and transfers. Only 15% of ultra poor households reported having loans in cash from other households and 5% reported renting land from others. The two types of interactions that were most important for the ultra poor households at baseline were employment and food exchange links.

[18]This method of identifying informal insurance partners is commonly adopted in the literature. For examples see Fafchamps and Lund (2003), De Weerdt and Dercon (2006), Barr and Genicot (2007)

(henceforth the "other poor"). Their total per capita expenditure is also lower than the other poor.

As mentioned above, one of the selection criteria that increases the likelihood of being a beneficiary of the program is to not have a working male member in the household. Corresponding to this, row 3 in Table 1 shows that only 58% of the ultra poor households had male household heads. This increases to 79% for the other poor and nearly 100% for the wealthier classes. Further examination shows that 75% of female-headed households were widowed and 19% were divorced or separated from their husband. Of the remaining, only 5% were actually married and living with their spouse. It's not easy for a once-married woman to re-marry in this setting, so this shows that being a female-headed household is not a choice but more likely to be a consequence of some event that happened in the past and is likely to have important consequences for the livelihood of these households. Descriptive statistics provided in Table 13 in the Data Appendix show that female-headed poor households have significantly lower wealth, fewer working age household members and lower human capital (education and health) measures compared to the male-headed poor households[19].

Table 1 also shows that the ultra poor households are more likely to be working for another household at baseline relative to higher wealth ranks (74% of the ultra poor do so, as opposed to 67% for other poor and 38% for middle classes). Conditional on working for another household, 49% of the ultra poor have an employer that lives in the same village as them. The rest either migrate temporarily to different parts of the country or work for an employer nearby but outside the village. As I will be analyzing employment relationships within a village only, this implies that the results presented will be based on half of the employment links that the poor have. As such, they should be interpreted as an analysis of employment contracts within a village, and not as an analysis of the universe of employment contracts available. The types of jobs that the poor are involved in are limited. Women either work as a maid or as an agricultural laborer and men work mainly as agricultural laborers, at times working as day-laborers in non-agricultural tasks (such as construction work).

Finally, Table 1 also shows that 93% of the ultra poor households report receiving food transfers at times of need from other households. The proportion of respondents that report having to borrow food from others diminishes by wealth class of the respondent to 82% for middle class and only 42% for the upper class. This implies that

[19]Dréze and Srinivasan (1997) show that the correlation between being female-headed and the poverty level of the household is very sensitive to controlling for economies of scale (i.e. small household size). They show that per capita expenditure measures of poverty are not significantly different between male and female headed households, but for a given household size and child-adult ratio female-headed households have lower per capita expenditure than male-headed households.

informal support networks are very important for the poor's livelihood and the rich are more likely to have alternative mechanisms for smoothing their consumption (such as pre-cautionary savings). The next row shows the proportion of respondents that report ever giving out food transfers. Only 44% of the poor report giving out food transfers, implying that more than half of their food borrowing links are not reciprocated by food lending. In fact, only 37% of their food borrowing links are reported as also food lending links at baseline.

The final row in Table 1 shows that the poor are not nearly as able as the rich to smooth their consumption. When asked "Is your household able to afford at least 2 meals a day", only 42% of the ultra poor households responded "Yes" while the corresponding figure is 55% for the other poor, 82% for the middle class and 97% for the upper class households.

1.3.3 Labor Contracts of The Poor

In order to identify tied labor contracts I look at the overlap between employment and food borrowing links. Conditional on working for someone in the same village, 25% of the ultra poor report one or more of their employers as a food borrowing source in times of crisis. This decreases to 23% for the other poor and to 15% for the middle class households. For the rest of the analysis I classify all employment contracts (within a village) where the employer is reported as a food borrowing source at times of need as a "tied-labor" contract, and all other employment relationships (within a village) as a "casual-labor" contract. This definition corresponds to the tied-labor contracts in the theoretical framework in section 2 where the employer acts as an insurance provider. Furthermore, to empirically test whether the fact that the worker reports the employer as a source of food transfers is correlated with relevant characteristics of the contract, I look at the following correlations: (1) the correlation between reporting an employer as a borrowing source and the average wage received from employers (2) the correlation between reporting an employer as a borrowing source and the volatility of the wage earnings.

In order to test the first correlation at baseline, I regress the average wage rate received by any worker in the household on a dummy variable for whether any worker in the household was in a tied-labor contract, controlling for the wealth and human capital measures of the household, the wealth rank of the employer, and demographic characteristics of the worker's household. Table 2 provides the results of this regression. Households in tied labor contracts have on average 4.5% lower earnings per hour (calculated by dividing total earnings by total hours spent working). In column (2)

of Table 2, the dependent variable is the daily wage[20] that the worker receives, which shows a very similar correlation. The measure of earnings used in columns (1) and (2) are the total of earnings in cash and in kind. Next, I examine the correlation of being in a tied contract with earnings in cash and in kind separately. As tied workers are likely to receive more food transfers from their employers, I expect being in a tied contract to be positively correlated with earnings in kind and negatively with earnings in cash. The results provided in columns (3) and (4) of Table 2 confirm this: the coefficient of "tied worker" in column (3) of Table 2 shows that being in a tied labor contract is correlated positively with earnings in kind. On average, a tied worker has 28% higher daily wage in kind compared to a casual worker. On the other hand, column (4) shows that being a tied worker is correlated negatively with earnings in cash. On average, tied workers earn 27% lower daily wages in cash. In total their daily wage is 4.2% lower than that of casual workers.

In order to test the correlation between being in a tied-labor contract and the volatility of wages, I use the following information: the respondents were asked to report for each business activity whether their earnings varied across the year and if so which months were the months of minimum and maximum income. For the months of minimum/maximum earnings, they were asked to report the level of earnings. Using this information, I construct monthly total earnings from wage employment[21]. Figure 1 plots the monthly earnings from wage employment from male and female workers respectively. One can easily see that months 2 and 8 are the two harvest seasons, while months 5, 6 and 7 correspond to the lean season. To test for correlations between contract type and fluctuations in earnings form wage employment, I estimate:

$$y_{it} = \alpha + \beta tied_i + \delta season_t + \lambda tied_i \times season_t + \gamma' X_{it} + \epsilon_{it} \tag{16}$$

where the dependent variable is log average earnings of respondent i from wage em-

[20] As part of the survey questionnaire, the respondents were asked to report the total income and total hours spent on any activity during the past year. In addition to that they were asked to report the daily wage they received on a typical day. The dependent variable in column (1) of Table 2 is the ratio of total income from wage employment to total hours spent in wage employment, while the dependent variable in the second column is the daily wage received on a typical working day. Not all workers are paid on a daily basis (although that is the most common practice for wage payment in this setting) and that is why the number of observations are slightly different.

[21] Monthly earnings from each activity of the household head was calculated as follows: The respondents reported the total annual income from each activity; if their income from the activity varied across the year they were asked to report the months of minimum and the maximum earnings and the amount of monthly earnings in these months. For all other months (neither maximum nor minimum income months, i.e. "normal" income months), monthly income was imputed by subtracting total amount of earning in maximum and minimum months from the total annual earnings and diving this residual by the number of "normal" income months.

ployment in season t, $tied_i$ is a dummy variable for whether the respondent was in a tied-labor contract and $season_t$ is a dummy variable for whether the season was a peak season, lean season or neither. I define months 2 and 8 as peak season, months 5, 6 and 7 as lean season and the rest as "normal" season. The coefficient δ gives the difference between wage earnings of the respondent in each season relative to the normal season and the coefficient λ gives the differential change in wages of tied workers in that season. Table 3 provides the results of this estimation. As expected, monthly wage earnings are higher and lower during the peak and lean seasons respectively, relative to the normal season. This is so for both female and male wages. Female workers in tied contracts earn 51% more during the lean season relative to casual workers (or to put it more precisely, their earnings from wage employment falls by 51% less relative to casual workers). This correlation is precisely estimated at conventional levels. Similarly male workers in tied contracts earn 28% more during the lean season, but this is imprecisely estimated. These correlations show that, in line with the characteristics of a tied-labor contract in the theoretical framework, workers that have tied labor contracts according to my empirical definition have lower wages on average, yet they have smoother wage income profiles.

1.4 Empirical Analysis

To test for the effects of the program on the labor contracts of the targeted poor workers, I use the following identification strategy:

$$y_{it} = \alpha + \beta T_i + \delta R_t + \lambda T_i \times R_t + \gamma' X_{it} + \epsilon_{it} \tag{17}$$

where y_{it} is outcome of interest for worker i in period t; $T_i = 1$ if worker i lives in a treated village and $= 0$ if she live in a control village, $R_t = 1$ after the program and 0 otherwise. The parameter of interest is λ, the difference in difference between treatment and control before and after the program. The standard errors are clustered at the village level in all the regressions[22]. Under the identifying assumption that the control villages represent a valid counterfactual for the treated villages in the absence of the program, namely that trends in all outcomes of interests are the same in treatment and control, λ identifies the causal effect of the treatment.

[22]I cluster the standard errors at the village level due to two reasons: First, the employment links I analyze are formed within the village hence the terms of the labor contracts of workers from the same village are likely to be correlated. Second, the descriptive statistics imply that food exchange links are formed almost entirely (more than 90% of the time) within the same village. Therefore not adjusting the standard errors for correlations at the village level may lead to a large bias in the standard errors.

1.4.1 Direct Effects on Treated Women

I start by testing the effects of the program on the beneficiaries that are directly affected by the treatment, that is the main female respondent in treated households. I start by testing **prediction 1(i)**, on labor supply of treated women into wage employment. In order to do so, I restrict the sample to the treated women in treatment villages, and women that were selected as beneficiaries by the program in control villages (who will be treated in 2011). Hence the estimate for λ gives me the difference-in-difference estimate for the direct effect of the program on treated women. Table 4 provides the results where the estimates for the coefficient of interest λ are given in the row "treat $\times$ post". Column (1) shows that females in treated households are 5% less likely to be working for another household in the village after the treatment, relative to control households. This effect is significantly estimated at conventional levels. Column (2) of Table 4 shows that females in treated households work on average 72 hours less per year compared to women in control households. This effect is precisely estimated at conventional levels and corresponds to a 22% decrease in the hours spent in wage employment by ultra poor females. These findings imply that the labor supplied for wage employment by treated females is decreasing, both on the extensive and the intensive margins.

Next, I test **prediction 1(ii)** on the participation of treated women in tied versus casual labor. Column (1) in Table 5 shows that conditional on being in wage employment (i.e. sample restricted to females that report working for another household within the village in either survey wave), treated females are 5% less likely to be in a tied-labor contract. This implies that supply of labor by treated females into tied labor contracts is falling[23].

In order to test whether the change in the terms of treated women's labor contracts is in line with them switching from tied to casual labor contracts, I analyze the change in the wage rate and the volatility of the earnings of treated women. In order to identify the effect of the program, I estimate difference-in-difference specifications similar to (17) above, and in order to identify the differential change in contract terms of tied vs casual workers I estimate heterogenous diff-in-diff specifications where I use "whether a worker was in a tied contract at baseline" as the interaction term. More specifically,

[23]The result is the same when I run the same regression on the full sample of ultra poor women, coding the dependent variable to 0 if the respondent is not working for any household in the village. The difference-in-difference estimate in this case is -0.02 and significant at 10% level, which implies that there is a significant fall in the incidence of tied-labor among treated women.

the model I estimate for the heterogenous effects is:

$$y_{it} = \alpha + \beta_1 T_i + \beta_2 R_t + \beta_3 Z_{i0} + \beta_4 T_i Z_{i0} + \beta_5 R_t Z_{i0} + \lambda_1 T_i R_t + \lambda_2 T_i R_t Z_{i0} + \gamma' X_{it} + \epsilon_{it} \quad (18)$$

where y_{it}, T_i, R_t and X_{it} will be the same as in (17) and Z_{i0} will be whether the female respondent in household i was in a tied-labor contract at baseline. The parameters of interest are now λ_1 and λ_2: λ_1 will be the difference in difference between treatment and control households with $Z_{i0} = 0$ (i.e. those who were not in a tied contract at baseline) before and after the program; λ_2 will be the additional effect of the program on those households with $Z_{i0} = 1$ (i.e. those who were in a tied contract at baseline). The estimates for $\lambda_1 + \lambda_2$, the total impact of the program on households with $Z_{i0} = 1$, are also reported[24].

Column (2) of Table 5 shows that the wages of female workers after the program are higher. The difference in difference estimate for treated female workers is 0.72 and significant, which corresponds to a 16% increase in the wages of treated women who work for other households in the village. Column (3) shows that this effect is similar for both casual and tied workers. The wage rate for women in casual labor contracts increases by 0.57 Takas per hour, while the wage rate for tied workers increased by 0.95 Takas per hour. The difference between the two is not significantly estimated. Since the wage rate is observed only for those women who report working for a wage, these effects on the wage of treated women could be the result of two different effects: (i) The fall in the supply of female workers in the village could lead to an increase in the wage level, as predicted by the model (ii) It could be that the women who decide to stay in wage employment are the ones who had higher wages to start with and therefore the difference-in-difference estimate is positive and significant. In order to determine which of these two channels the effect is coming from I will focus on the effects on non-treated poor females[25] in Section 4.3.

Next, I analyze the effect on the "volatility" of wage earnings. The measure of volatility that I use is the ratio of minimum to maximum monthly earnings of the respondent from wage employment, so a smaller value means higher volatility. Results in column (4) of Table 5 show that the ratio of minimum to maximum monthly earnings decreases by 0.03, which implies that the volatility of earnings is increasing for treated females. However this effect is imprecisely estimated. Column (5) of Table 5 shows that the effect of volatility of wage earnings is higher for women in tied contracts. The differential effect on women who were in tied contracts at baseline is -0.03, which is

[24]The estimates for $\beta_3, \beta_4, \beta_5$ and γ are not reported for brevity.

[25]If it is the case that the estimated effect on wages of treated women is due to the second channel alone, then there should be no effect on the wages of non-treated women

consistent with them switching to casual contracts. These effects on the labor contracts of female workers is in line with them switching from tied to casual labor contracts and with the program having GE effects on the female labor market. Following discussion in section 4.3 on the effects on contracts of non-treated females (who experience any GE effects through the labor market, but are not affected by the direct effect of the program on their outside options) will shed light on the relative magnitude of these two channels.

In the following discussion, I will analyze the heterogeneity of these effects on treated women. The second part of **Prediction 1(ii)** predicts that the direct effect of the program should be higher for women who face lower risks in their outside options, i.e. for women with lower p_k. A good proxy for this is the gender of the household head. Female-headed households are likely to have higher vulnerability (higher p_k)) as they have fewer income-earning members in the household and any negative shocks to their health or to their business activity will have a greater cost. For example, if p_k is a function of q where q is the probability that any one of the working-age members will receive a negative health shock and will not be able to work in period t, then the probability that there will be no working age member available in period t will be q^n where n is the number of working age members in the household. If the asset transferred is associated with a production function that requires at least one person to take care of the asset (which is likely to be the case with typical assets transferred such as livestock or trees) then female-headed households that have fewer working age members will be more prone to risks to their outside option. Hence they may choose to stay in tied employment even though they receive the same asset transfer as male-headed households.

Table 6 presents the results for treated females who live in female-headed households (Panel A) and male-headed households (Panel B) separately. Column (1) shows that the effect on being in tied-labor as opposed to casual labor is very different for the two types of households. Females in male-headed households are 9% less likely to be in tied labor after the treatment. This effect is precisely estimated at 5% significance level. On the other hand, the effect for female-headed households is insignificant and in the opposite direction. This implies that females in male-headed households switch from tied to casual employment more readily than females where they are the head of the household. This is in line with the intuition explained previously, where female-headed households face greater risks and therefore choose to stay in tied employment. The increase in the expected outside option of treated women in female-headed households is not enough to move them above the threshold level to terminate tied-labor contracts.

Looking at the wages of females in female vs male headed households (column (2)

of Table 6)), both types of households experience a significant increase in their wages. However column (3) shows that the effects on the wages of treated women in male and female-headed households come through different channels. Column (3) in Panel A shows that females in female-headed households have higher wages in both casual and tied labor contracts (the increase is smaller for those in tied labor but the difference between the increase in tied and causal labor is insignificant). On the other hand, Panel B shows that the increase in wages of females in male-headed households is coming mainly from those that were in tied labor at baseline. This implies that the effect for the latter group is due to them switching from tied to casual labor contracts. The results on the volatility measure confirm this: The volatility of wage income for females in female-headed households does not change (Panel A, Column (5)), while the volatility of wage income for females in male-headed households that were in tied labor contracts increases significantly (Panel B, Column (5)). This is consistent with them switching from tied to casual labor contracts and as a result experiencing a higher wage rate at expense of higher wage volatility.

To sum up, the analysis of the effects of the program on treated females implies that: (i) Total supply of female wage workers goes down. (ii) The composition of female wage workers changes so that there are fewer tied workers and more casual workers. (iii) Treated women who live in male-headed households and are less vulnerable to negative shocks are much more likely to switch from tied to casual labor contracts. Next, I analyze the indirect effects of the program on men who live in treated households.

1.4.2 Spillover Effects on Men in Treated Households

Even though the program targets women, men who are part of the same household as treated women are also likely to experience an increase in their outside option through the increase in the self-employment income of the household. Therefore, in this section I will test **Prediction 1(i)** and **Prediction 1(ii)** on males who live in treated households, using the same identification strategy as above. Table 7 tests **Prediction 1(i)** on males in treated households. I find that the amount of labor supplied in the village labor market by males in treated households is not affected by the program. Column (1) of Table 7 shows that the difference-in-difference estimate for the likelihood of being in wage employment for treated men is 0.009 and imprecisely estimated. Column (2) shows that the effect of the program on the hours spent in wage employment by men who live in treated households is 15, but imprecisely estimated. These findings show that the program has practically no effect on participation in wage employment in the village by men in treated households. As discussed above, given that the average male wage rate is more than 50% higher relative to that of females, it is not surprising that

the men's outside options do not change enough to move them over $Eu(W)$.

Table 8 provides results on the labor contracts of men who live in treated poor households. In column (1) of Table 8, I test **Prediction 1(ii)** on the composition of treated men's labor contracts. In line with the prediction of the model, men who live in treated households and experience an increase in their outside options are 8.5% less likely to be in tied labor contracts[26] This implies that the improvement in their spouse's self-employment opportunities allows them to terminate tied labor contracts and enter employment opportunities with higher returns but more risk. Column (2) of Table 8 shows that the wages of men in treated households increases by 0.44 Takas per hour on average. However, column (3) shows that this effect is coming mainly from men that were in tied contracts at baseline. Column (3) shows that the wages of men in treated households that were in tied contracts at baseline increases by 1.35 Takas per hour. This corresponds to a 14% increase relative to their wage at baseline. The effect on men that were in casual contracts at baseline is only 0.29 and imprecisely estimated. This implies that the average effect on men's wages estimated in column (2) is driven by an increase in wages of workers who were in tied contracts at baseline. This could be coming from the fact that these workers are likely to switch from tied to casual labor contracts (which provide higher wages) or potentially a positive GE effect on the wages of men in tied contracts (or a combination of the two channels). I will test for the latter in the following section. Finally, column (5) of Table 8 shows that the volatility of wage earnings has increased for men who were in tied contracts at baseline has increased (although this effect is imprecisely estimated). This is in line with men in treated households switching from tied to casual labor contracts.

1.4.3 Spillover Effects Through the Labor Markets

In order to identify the spillover effects of the program through the labor markets, I test for its effects on non-treated households who do not experience a direct increase in their self-employment opportunities. Therefore, any effect on their labor contracts will be an indirect effect of the program through the village labor market. To identify the indirect spillover effects of the program, I restrict the sample to the other poor (households that were ranked in the bottom wealth groups by the community but not selected as beneficiaries by the program) in treatment and control villages. As the descriptive statistics presented in Table 1 demonstrated, this group of households is

[26]When I estimate the same model on the full sample of males in treated households, unconditional on being in wage employment (i.e. recoding the dependent variable so that it takes the value 0 for any male who is not working for a wage) the difference-in-difference estimate is -0.015 and significant at 10% level.

second most likely to be working for a wage for other households in the village (after the ultra poor), hence they are most likely to be affected by any GE effects through the labor market.

I will be testing the spillover effects of the program on females and males separately as the labor markets for the two are likely to be different in this setting. Foster and Rosenzweig (1996) show that male and female workers in rural India work in different types of jobs, depending on comparative advantage. This is similar in rural Bangladesh where men often work in physically-demanding jobs, while women work in jobs that require less physical strength, such as sowing seeds, taking care of livestock, working as a maid etc. Furthermore, hourly wage rate for male workers is much higher compared to that of females (average wage rate for a male worker is 59% higher at baseline relative to a female worker). Due to these reasons, I analyze the effects on male and female labor markets separately. The identification strategy is same as the difference-in-difference methodology in (17) and (18).

I start by testing the effects on labor contracts of non-treated females. **Prediction 2** implies that under certain conditions, the program is likely to lead to an increase in the incidence of tied-labor among non-treated workers (due to the increase in $\widehat{y}$) and the wage rates for both tied and casual workers. Table 9 presents the results on effects of the program on labor contracts of female workers in non-treated households. First, there is an insignificant fall in incidence of tied-labor among the untreated poor females. They are 1.8% less likely to be in tied-labor contracts, but this effect is imprecisely estimated at conventional levels.

Second, results in columns (2) and (3) show that the wage rate is significantly higher for females who were in casual contracts at baseline, but not for females who were in tied-labor contracts. Column (2) shows that the average wage rate for non-treated women who live in treatment villages increased by 0.87 Taka per hour, corresponding to a 15% increase. Column (3) shows that this effect is coming mainly from women that were in casual contracts at baseline. The effect on the wages of women that were in tied contracts at baseline is only 0.27 Taka per hour and imprecisely estimated, while the wages of women in casual contracts increases by 0.89 Taka per hour. Although the difference between tied and casual workers is not precisely estimated, the coefficient is negative and the total effect on wages of workers that were in tied contracts at baseline is nearly 0. This implies that although the supply of both casual and tied female workers in the village falls, only the wages for casual female workers increases and the wages for tied female workers remains practically unchanged. Finally, columns (4) and (5) of Table 9 show that there is no effect on the volatility of the wage earnings of non-treated women. This is consistent with the findings that the non-treated women

are not significantly more likely to change the type of their labor contracts.

In order to test whether the program has any GE effects on the male labor market, I limit the sample to male workers from other poor households – who would be affected by such effects – and estimate (17). Table 10 presents the results. Column (1) in Table 10 shows that there is no effect on the incidence of tied labor among male workers that are not treated. The difference-in-difference estimate is practically 0. On the other hand, column (2) in Table 10 shows that there is a small positive effect on the wage rate of male workers who work within the village. The difference-in-difference estimate for the average effect on wage rate of non-treated males is 0.41 Takas per hour and significant at 5% level. Column (3) shows that this effect is coming almost entirely from men who were in tied labor contracts at baseline. The wages of non-treated male workers that were in tied contracts at baseline is increased by 1.21 Takas per hour, corresponding to a 13% increase relative to the baseline wage level for tied male workers. Finally, columns (4) and (5) show that, consistent with them staying in tied labor contracts, there is no effect on the volatility of their wage earnings. These findings are in line with the effects on the labor contracts of treated male workers: due to the fall in the supply of tied male workers, the wages for tied male workers increases, but there is not effect on the wages for casual male workers.

1.4.4 Changing Pattern of Insurance

In this section, I test **Prediction 4** on whether the treated households are more likely to enter reciprocal transfer arrangements to insure themselves against risks. As reported earlier, at baseline, only 37% of the ultra poor's food borrowing links were reciprocated by food lending, much lower than that of middle class (62%) or rich (73%) households (where middle and rich is determined according to the community wealth ranking).

Table 11 presents results on the wealth and reciprocity of treated poor households' food exchange network. In column (1), I estimate the effect on average wealth of households that are part of the respondent's food exchange network (I use wealth as defined at baseline in order to control for any increase in wealth of network members through the treatment). The results show that on average the treated households exchange food with households that were 4.5% wealthier at baseline. This implies that treated households who experience on exogenous increase in their self-employment opportunities, hence their income, are likely to form food exchange links with wealthier households. Column (2) of Table 11 shows that the proportion of their borrowing links that are reciprocated by lending increases by 6.8 percentage points. This corresponds to an 18% rise in the degree of reciprocity of their food borrowing links relative to baseline. The next four columns of Table 11 breaks down this aggregate reciprocity

into reciprocity of their borrowing links with households from the top wealth rank, middle and bottom wealth ranks respectively. One may think that the effect is partly the effect of the program bringing treated households together and enabling them to build tighter social networks with one another. However results in columns (3)-(6) show that the reciprocity of food exchange with upper, middle and non-treated poor households increases while the reciprocity with ultra poor households is insignificantly increased.

Taken together, these findings and the findings on incidence of tied-labor among the treated households suggest that the treated poor are likely to engage less in tied-labor and more in reciprocal transfer arrangements in order to smooth their consumption. This is consistent with the discussion in section 1.2 – the fact that treated poor households that are wealthier and have better earnings are matching with wealthier households in reciprocal exchange links is consistent with them being able to enter into better informal insurance arrangements. The increase in outside options of the treated households enables them to switch from tied labor contracts to a combination of casual labor contracts (with higher yield but more risk) and reciprocal transfer arrangements with other, wealthier households.

1.4.5 Interpretation of Results

The findings on male and female labor contracts showed that the fall in the labor supply of female workers has led to an increase in wage rate for casual female workers, but the wage rate for tied female workers has been affected much less. On the other hand, in the male labor market there hasn't been an impact in total supply of male workers but the supply of tied workers has diminished, leading to an increase in the returns to supply of casual laborers. The finding that although the supply of tied workers in both the male and female labor markets is being reduced, the returns to tied labor for males is increasing while the returns to tied female labor is not changing suggests that there may be different demand structures for male versus female tied laborers. A key assumption behind the predictions of the theoretical model in Section 2 was that tied labor and casual labor are perfect substitutes during the peak season, and the tied workers are not put to any use by the employer during the lean season. In practice, this is not likely to be the case and tied workers are likely to be (occasionally) employed in various agricultural (such as preparing the land for cultivation) or non-agricultural (such as house work) activities during the lean season, while both tied and casual workers are likely to be employed on farm work during the peak season. The findings suggest that the elasticity of demand for female tied workers is much higher than that for tied male workers.

One possible mechanism behind this could be the availability of female family labor in employer households, while the availability of male family labor is much more limited. Descriptive statistics at baseline show that the average female in a top rank household reports working for 812 hours per year while the average male reports working for 1771 hours. Hence, the opportunity cost of time for females in wealthy households is likely to be much lower relative to the opportunity cost of time for men in these households. In order to examine whether this mechanism could explain the difference in demand elasticities of demand for male vs female tied labor, I limit the sample to employer households and estimate a regression of the form (18) where the dependent variable is hours worked by female members of employer households and Z_{i0} is a dummy variable equal to 1 if the household employed any female from a ultra poor household at baseline. Table 12, column (1) shows the result. I find that there is a differential positive impact on the hours spent doing chores by female members of households that employed tied female workers from treated poor households at baseline. When I estimate the same specification for hours spent working by males in employer households, I find a positive but insignificant differential effect on those that hired tied female workers from treated households at baseline. This suggests that instead of replacing the tied female workers with new ones, the employers may be replacing them with family labor which is likely to have lower cost. On the other hand, it is not likely to be cheap to replace tied male workers with male family labor (given that the males in employer households are already working longer hours relative to females) and that may be the reason why the elasticity of demand for male tied laborers is low relative to the elasticity of demand for female tied workers.

An alternative mechanism that may explain why the returns to male tied labor is increasing while the returns to female tied labor remains relatively unchanged could be the differential importance of non-wage benefits for tied male and female workers. In particular, women in rural Bangladesh are much more constrained in terms of accessing services that require them to interact with others in social spaces. The institution of "purdah" that limits women's participation in social and economic systems in South Asia is well documented in sociological studies (e.g. Papanek (1973)). As a result, it is likely that female tied workers value non-wage benefits, such as receiving assistance from their employers to access services (e.g. healthcare, courts, markets) more than the male tied workers. In order to examine whether this mechanism may explain the findings, I use information on whether non-treated female respondents report visiting such spaces and conditional on visiting these places, whether they report receiving assistance from others in order to access services[27]. Column (2) of Table 12 shows the result of this

[27]More specifically, respondents were asked whether during the past 1 year they visited any of the

exercise. I find that non-selected women who were in tied-labor contracts at baseline are 22% more likely to report receiving assistance from others during a visit to any of the reported social spaces. This suggests that the differential role of non-wage benefits for female tied workers could explain why the wage rate of female tied workers does not increase as much as the wages of male tied workers. However, this interpretation should be taken with some caution as I cannot identify whether this assistance was from their employers (as I do not have information on the identity of who the assistance was given by).

1.5 Conclusion

There is a large literature on the theory of labor tying in rural economies. The existence of tied-labor in rural labor markets has been well documented, particularly in the context of South Asia. A tied-labor contract involves a long term relationship between an employer and a worker, where the employer provides a low but steady wage to the worker (as opposed to a casual labor contract that yields a high return during the harvest season). As such, tied-labor is likely to be an important mechanism through which poor households in developing countries insure themselves against risks, yet recent empirical literature on informal insurance in developing countries has been dormant on the role of tied-labor. In this paper, I show that tied labor is an important channel through which the poor in rural Bangladesh insure themselves against fluctuations in their income and hence in their consumption. Furthermore, I exploit exogenous variation in the outside options of poor women provided by the randomized roll-out of BRAC's "ultra poor" program to provide evidence on how the outside option of the worker determines the type of labor contract (tied or casual) he/she enters.

In order to evaluate the effects of the program on the labor contracts of treated women, their spouses, and the rest of the community, I use a theoretical framework adapted form Bardhan (1983). The theoretical framework has 4 main predictions on the effects of the treatment: (i) In partial equilibrium, treated workers may be less likely to be working for a wage and less likely to be in tied labor contracts (ii) If the supply of tied and casual labor in the village falls, the returns to both will increase. This will yield to an increase in the threshold level of outside option below which workers enter into tied-labor. (iii) In general equilibrium, the program has ambiguous effects on treated workers' participation in tied and casual employment. (iv) Treated workers

following: health center, court, NGO office, livestock office, agricultural office, local markets. If they reported having visited any of these places, they were asked to report whether they received any assistance from others to access services (could be from other household members or individuals from outside the household, but I cannot differentiate between the two).

will be more likely to enter reciprocal transfer arrangements.

In line with prediction (i), I find that treated women are less likely to be working for a wage and they are less likely to be in tied labor contracts. This implies that the supply of both tied and casual female labor in the village is lower as a result of the treatment. Corresponding to prediction (ii), I find that the return to casual female labor is increased, yet the return to tied female labor is unchanged. I provide evidence on two alternative channels that may explain this: First, the employers are substituting tied female labor with female family labor. Second, female workers who remain in tied labor are more likely to receive assistance in order to access public services (which is likely to be part of their compensation from tied labor).

The effects on the spouses of treated women show that they are equally likely to be working for a wage as before, but they are less likely to be in tied labor. In line with this, the wages of tied male workers in the village increases but the wages of casual male workers is not affected.

Finally, I show that, in line with prediction (iv), the treated households form food exchange links with wealthier households in the village and increase the reciprocity of their transactions. This suggests that poor households may not be able to smooth their consumption via reciprocal transfer arrangements as efficiently as the wealthier households in the village. As a result, an exogenous improvement in their self-employment opportunities that increases their expected income level allows them to switch from tied labor to reciprocal transfers as a consumption smoothing mechanism.

Taken all together, the findings imply that poor households in rural markets may be involved in second-best labor contracts to insure themselves against risks. An exogenous improvement in their wealth enables them to move to riskier but more profitable labor opportunities while making them more likely to insure themselves via reciprocal transfer arrangements with other households in the village.

These findings have important policy implications. First, they imply that poor households in rural markets may be involved in tied labor contracts to insure themselves against risks. This creates a link between insurance and labor markets, which implies that policies that affect one of these markets are likely to have impact(s) on the other one. For example in the current study, an exogenous improvement in the outside option of the worker causes the link between her labor and insurance arrangements to get weaker, as she moves to riskier but more profitable labor opportunities while increasing her participation in reciprocal transfer arrangements with other households in the village.

Second, my findings show that labor markets for wage-employment are highly segmented by gender in rural Bangladesh. As a result, the general equilibrium effects

of the treatment on male and female workers are very different. An increase in self-employment opportunities benefits both males and females in treated households, as both sexes become involved in family-run businesses. The effects on the village labor market differ greatly by gender. Females both in treated and non-treated households benefit from a rise in wages which results from treated females reducing their labor supply. In contrast, males in non-treated households benefit mainly from the program enabling men to break tied contracts with employers. My results suggest that in evaluating the impacts of entrepreneurship programs (and other interventions aimed to reduce poverty), it is essential to carry out the analysis separately by gender as segmentation of labor markets in poor economies imply the effects will be very different.

Finally, the fact that female-headed households choose to remain in tied-labor arrangements, although they receive the same amount of exogenous wealth transfer as male-headed households draws attention to the fact that household composition plays a very important role in the choice of insurance mechanisms of the household. Households that are more vulnerable to shocks may prefer to remain in less profitable labor arrangements to insure themselves against risks, even when they are made wealthier. This suggests that a more intensive treatment (perhaps one that explicitly addresses insurance) may be required for such households, in order for them to take advantage of similar benefits as less vulnerable households.

Table 1: Descriptives at Baseline

	Ultra Poor	Other Poor	Middle Class	Upper Class
	(1)	(2)	(3)	(4)
Wealth	5635.3	14049.6	153087.2	853759.9
	(30046.0)	(70166.4)	(324018.7)	(973480.1)
Pce	3958.7	4251.4	5556.3	12002.0
	(2272.3)	(3004.7)	(5278.4)	(34611.4)
Male hh head	0.58	0.79	0.94	0.95
	(0.49)	(0.41)	(0.23)	(0.22)
Household size	3.26	3.70	4.43	5.02
	(1.69)	(1.65)	(1.66)	(2.01)
Work for another hh	0.74	0.67	0.38	0.04
	(0.44)	(0.47)	(0.49)	(0.20)
Work for another	0.49	0.47	0.41	0.28
hh in same village	(0.50)	(0.50)	(0.49)	(0.45)
Receives food transfer	0.93	0.92	0.82	0.42
	(0.26)	(0.27)	(0.38)	(0.49)
Gives food transfer	0.44	0.52	0.72	0.81
	(0.50)	(0.50)	(0.45)	(0.39)
Can have at least	0.42	0.55	0.82	0.97
2 meals a day	(0.51)	(0.59)	(0.54)	(0.25)
N	6746	8470	7190	2407

Notes: Columns 1,2,3,4 give summary statistics at baseline for ultra poor, other poor (households that were ranked in the bottom wealth rank by the community but not chosen for treatment by the program), middle rank households and top rank households respectively. "Wealth" is total measure (in TAKAs) of household assets, including land, livestock, other productive assets and household durables. "Pce" is total annual per capita expenditure of the household, in Bangladeshi Takas (1 Taka=0.014 US Dollar as of 10/22/2010). "Male hh head" is the proportion of households that have a male household head. "Household size" is the number of household members. "Works for another hh" is the proportion of households where the main female respondent and/or the male household head works for another household. "Work for another hh in same village" is the proportion of households in which, conditional on working for another household, either the main female and/or the male household head works for another household in the same village. "Receives food transfer" is the proportion of households that reports ever having to receive rice or other food items from other households. "Gives food transfer" is the proportion of households that reports ever giving rice or other food items to other households. "Can have at least 2 meals a day" is the proportion of households that responded "Yes" to the question "Could your household afford two meals per day most of the time during last year?"

Table 2: Correlates of Contract Type at Baseline

	Log earnings per hour	Log total daily wage	Log daily wage in cash	Log daily wage in kind
	(1)	(2)	(3)	(4)
tied worker	-0.045**	-0.042**	-0.323***	0.235***
	(0.019)	(0.021)	(0.078)	(0.073)
log wealth	0.020***	0.014***	0.008	-0.004
	(0.004)	(0.004)	(0.017)	(0.014)
cons	1.696***	3.615***	2.795***	2.484***
	(0.058)	(0.060)	(0.222)	(0.216)
N	2447	2396	2391	2391

Notes: *** stands for p-value<0.01, ** stands for p-value< 0.05, * stands for p-value< 0.10. Standard errors are clustered at spot level. Sample is restricted to ultra poor households at baseline. "Tied worker" is a dummy variable equal to 1 if any of the main female's or the male household head's employers is reported as a food borrowing source in times of need. All regressions control for the following variables: a dummy for whether the religion of the household head was Islam, whether the respondent was undernourished at baseline (BMI<18.5), log of baseline household wealth, whether the respondent reported being able to read and write at baseline, number of under 10 household members, average wealth class of the respondent's employer(s), whether the survey month was "kartik" (lean season)

Table 3: Correlates of Seasonality in Wage Earnings at Baseline

	Log Wage Earnings of ultra poor females	Log Wage Earnings of ultra poor males
	(1)	(2)
peak season	0.602***	0.427***
	(0.052)	(0.047)
lean season	-1.825***	-1.563***
	(0.091)	(0.121)
tied	0.108	-0.019
	(0.084)	(0.098)
peak × tied	0.003	-0.024
	(0.078)	(0.080)
lean × tied	0.412***	0.248
	(0.126)	(0.209)
total effect for tied workers at peak season	0.112*	-0.042
	(0.064)	(0.097)
total effect for tied workers at lean season	0.520***	0.229
	(0.143)	(0.232)
N	5499	2673

Notes: *** stands for p-value<0.01, ** stands for p-value< 0.05, * stands for p-value< 0.10. Standard errors are clustered at spot level. Sample is restricted to ultra poor households at baseline. Dependent variable in columns 1 and 2 are the log monthly earnings of the main female and household head respondents, respectively. "peak season" is a dummy =1 if the observation was recorded at months 2 or 8 according to the Bengali calender. "lean season" is a dummy =1 if the observation was recorded in months 5, 6, 7 according to the Bengali calender. "tied" is a dummy =1 if respondent's household borrows food from her/his employer(s) during times of need. All regressions control for the following variables: a dummy for whether the religion of the household head was Islam, whether the respondent was undernourished at baseline (BMI<18.5), log of baseline household wealth, whether the respondent reported being able to read and write at baseline, number of under 10 household members, average wealth class of the respondent's employer(s), whether the survey month was "kartik" (lean season)

Table 4: Direct Effects on Women's Labor Supply (Within Village)

	Extensive Margin: Whether respondent works for a wage	Intensive Margin: Hours spent in wage employment
	(1)	(2)
treatment	0.028*	-0.079
	(0.017)	(23.606)
post	-0.008	-73.902***
	(0.012)	(18.219)
treat × post	-0.047***	-72.195***
	(0.017)	(22.593)
cons	0.468***	646.531***
	(0.031)	(42.603)
N	13490	13490

Notes: *** stands for p-value<0.01, ** stands for p-value< 0.05, * stands for p-value< 0.10. Standard errors are clustered at spot level. Sample is restricted to ultra poor households. The dependent variable in column 1 is a dummy variable equal to 1 if the main female respondent in the household reports working for another household within the same village. The dependent variable in column 2 is total hours the main female respondent spent working for other households within the village. "treat" is a dummy variable =1 if the observation is from a treatment village. "post" is a dummy variable =1 if the observation if from the followup survey. All regressions control for the following variables: a dummy for whether the religion of the household head was Islam, whether the respondent was undernourished at baseline (BMI<18.5), log of baseline household wealth, whether the respondent reported being able to read and write at baseline, number of under 10 household members, average wealth class of the respondent's employer(s), whether the survey month was "kartik" (lean season)

Table 5: Direct Effects on Women's Labor Contracts

	Tied-Labor	Wage per Hour		Volatility	
	(1)	(2)	(3)	(4)	(5)
treatment	-0.024	-0.114	0.078	-0.022	-0.022
	(0.023)	(0.202)	(0.191)	(0.018)	(0.019)
post	-0.006	0.004	0.230	-0.052***	-0.040***
	(0.023)	(0.190)	(0.179)	(0.014)	(0.016)
treat × post	-0.053*	0.723***	0.571**	-0.030	-0.026
	(0.031)	(0.277)	(0.291)	(0.020)	(0.021)
treat × post × tied at base			0.378		-0.030
			(0.596)		(0.040)
total effect for			0.949*		-0.057
workers tied at base			(0.542)		(0.037)
N	3389	3389	3389	3367	3367

Notes: *** stands for p-value<0.01, ** stands for p-value< 0.05, * stands for p-value< 0.10. Standard errors are clustered at spot level. Sample is restricted to females in ultra poor households who work for another household in either survey wave. The dependent variable in column 1 is a dummy variable equal to 1 if the female respondent in the household reports an employer as a source of food transfers in times of need. The dependent variable in columns 2 and 3 is her wage earnings per hour. The dependent variable in columns 4 and 5 is the ratio of her monthly wage earnings during the month in which her wage earnings were minimum to her earnings during the month in which her wage earnings were maximum. "treat" is a dummy variable =1 if the observation is from a treatment village. "post" is a dummy variable =1 if the observation if from followup survey. "tied at base" is a dummy equal to 1 if the respondent was in a tied labor contract at baseline. All regressions control for the following variables: a dummy for whether the religion of the household head was Islam, whether the respondent was undernourished at baseline (BMI<18.5), log of baseline household wealth, whether the respondent reported being able to read and write at baseline, number of under 10 household members, average wealth class of the respondent's employer(s), whether the survey month was "kartik" (lean season)

Table 6: Heterogeneity of Direct Effects on Women's Labor Contracts

Panel A: Female-Headed Treated Poor					
	Tied-Labor	Wage per Hour		Volatility	
	(1)	(2)	(3)	(4)	(5)
treat	-0.042	-0.175	0.070	-0.020	-0.008
	(0.029)	(0.284)	(0.265)	(0.023)	(0.024)
post	-0.018	-0.043	0.255	-0.060***	-0.046**
	(0.027)	(0.275)	(0.252)	(0.018)	(0.019)
treat × post	0.042	1.030**	0.972**	-0.036	-0.044*
	(0.038)	(0.456)	(0.489)	(0.025)	(0.026)
treat × post × tied at base			-0.406		0.016
			(0.936)		(0.056)
total effect for			0.566		-0.028
workers tied at base			(0.812)		(0.053)
N	1847	1847	1847	1832	1832

Panel B: Male-Headed Treated Poor					
	Tied-Labor	Wage per Hour		Volatility	
	(1)	(2)	(3)	(4)	(5)
treatment	-0.006	-0.071	0.034	-0.022	-0.039
	(0.034)	(0.208)	(0.222)	(0.021)	(0.025)
post	0.037	0.031	0.138	-0.039**	-0.034
	(0.037)	(0.213)	(0.218)	(0.019)	(0.023)
treat × post	-0.090**	0.430*	0.241	-0.028	-0.011
	(0.044)	(0.261)	(0.277)	(0.025)	(0.030)
treat × post × tied at base			1.046*		-0.069
			(0.587)		(0.049)
total effect for			1.286**		-0.079**
workers tied at base			(0.552)		(0.039)
N	1542	1542	1542	1535	1535

Notes: *** stands for p-value<0.01, ** stands for p-value< 0.05, * stands for p-value< 0.10. Standard errors are clustered at spot level. Sample is restricted to females in ultra poor households who work for another household in either survey wave. Sample is divided into female vs male-headed households in Panels A and B respectively. Variable definitions are identical to those in Table 5)

Table 7: Effects on Labor Supply of Men in Treated Households

	Extensive Margin: Whether respondent works for a wage	Intensive Margin: Hours spent in wage employment
	(1)	(2)
treatment	0.008	-2.733
	(0.017)	(32.037)
post	-0.041**	-114.593***
	(0.018)	(30.265)
treat × post	0.009	15.023
	(0.022)	(36.538)
cons	0.250***	1540.729***
	(0.038)	(79.961)
N	7472	7472

Notes: *** stands for p-value<0.01, ** stands for p-value< 0.05, * stands for p-value< 0.10. Standard errors are clustered at spot level. Sample is restricted to ultra poor households. The dependent variable in column 1 is a dummy variable equal to 1 if the male household head respondent in the household reports working for another household within the same village. The dependent variable in column 2 is total hours the male household head respondent spent working for another household within the same village. "treat" is a dummy variable =1 if the observation is from a treatment village. "post" is a dummy variable =1 if the observation if from followup survey. All regressions control for the following variables: a dummy for whether the religion of the household head was Islam, whether the respondent was undernourished at baseline (BMI<18.5), log of baseline household wealth, whether the respondent reported being able to read and write at baseline, number of under 10 household members, average wealth class of the respondent's employer(s), whether the survey month was "kartik" (lean season)

Table 8: Effects on Labor Contracts of Men in Treated Households

	Tied-Labor	Wage per Hour		Volatility	
	(1)	(2)	(3)	(4)	(5)
treatment	0.005	-0.322	-0.219	-0.007	-0.011
	(0.028)	(0.202)	(0.214)	(0.018)	(0.019)
post	0.014	0.332*	0.357*	-0.050***	-0.049***
	(0.036)	(0.199)	(0.211)	(0.017)	(0.018)
treat × post	-0.085**	0.444*	0.294	-0.020	-0.013
	(0.043)	(0.266)	(0.274)	(0.021)	(0.022)
treat × post × tied at base			1.060*		-0.057
			(0.623)		(0.056)
total effect for			1.354**		-0.070
workers tied at base			(0.611)		(0.053)
N	1611	1528	1528	1510	1510

Notes: *** stands for p-value<0.01, ** stands for p-value< 0.05, * stands for p-value< 0.10. Standard errors are clustered at spot level. Sample is restricted to males in ultra poor households who work for another household in either survey wave. The dependent variable in column 1 is a dummy variable equal to 1 if an employer of the male respondent in the household is reported as a source of food transfers in times of need. The dependent variable in columns 2 and 3 is his wage earnings per hour. The dependent variable in columns 4 and 5 is the ratio of his monthly wage earnings during the month in which his wage earnings were minimum to his earnings during the month in which her wage earnings were maximum. "treat" is a dummy variable =1 if the observation is from a treatment village. "post" is a dummy variable =1 if the observation if from followup survey. "tied at base" is a dummy equal to 1 if the respondent was in a tied labor contract at baseline. All regressions control for the following variables: a dummy for whether the religion of the household head was Islam, whether the respondent was undernourished at baseline (BMI<18.5), log of baseline household wealth, whether the respondent reported being able to read and write at baseline, number of under 10 household members, average wealth class of the respondent's employer(s), whether the survey month was "kartik" (lean season)

Table 9: Spillover Effects on Non-Treated Women's Labor Contracts

	Tied-Labor	Wage per Hour		Volatility	
	(1)	(2)	(3)	(4)	(5)
treat	-0.031	-0.422	-0.363	-0.009	-0.010
	(0.026)	(0.277)	(0.227)	(0.017)	(0.019)
post	-0.016	-0.370*	-0.260	-0.052***	-0.044***
	(0.022)	(0.208)	(0.199)	(0.012)	(0.013)
treat × post	-0.018	0.864***	0.892***	-0.016	-0.018
	(0.031)	(0.306)	(0.280)	(0.019)	(0.021)
tied at base			0.410		0.022
			(0.448)		(0.021)
treat × post × tied at base			-0.628		0.022
			(0.754)		(0.043)
total effect for			0.265		0.004
workers tied at base			(0.747)		(0.039)
N	3132	3132	3132	3103	3103

Notes: *** stands for p-value<0.01, ** stands for p-value< 0.05, * stands for p-value< 0.10. Standard errors are clustered at spot level. Sample is restricted to females in other poor households who work for another household in either survey wave. The dependent variable in column 1 is a dummy variable equal to 1 if the female respondent in the household reports an employer as a source of food transfers in times of need. The dependent variable in columns 2 and 3 is her wage earnings per hour. The dependent variable in columns 4 and 5 is the ratio of her monthly wage earnings during the month in which her wage earnings were minimum to her earnings during the month in which her wage earnings were maximum. "treat" is a dummy variable =1 if the observation is from a treatment village. "post" is a dummy variable =1 if the observation if from followup survey. "tied at base" is a dummy equal to 1 if the respondent was in a tied labor contract at baseline. All regressions control for the following variables: a dummy for whether the religion of the household head was Islam, whether the respondent was undernourished at baseline (BMI<18.5), log of baseline household wealth, whether the respondent reported being able to read and write at baseline, number of under 10 household members, average wealth class of the respondent's employer(s), whether the survey month was "kartik" (lean season)

Table 10: Spillover Effects on Labor Contracts of Men in Non-Treated Households

	Tied-Labor	Wage per Hour		Volatility	
	(1)	(2)	(3)	(4)	(5)
treat	-0.038*	-0.478***	-0.428***	0.020	0.017
	(0.022)	(0.155)	(0.164)	(0.018)	(0.020)
post	-0.051***	0.469***	0.454***	-0.058***	-0.061***
	(0.019)	(0.147)	(0.152)	(0.014)	(0.015)
treat × post	0.004	0.412**	0.349	-0.031	-0.030
	(0.029)	(0.208)	(0.214)	(0.020)	(0.021)
tied at base			0.096		-0.009
			(0.177)		(0.019)
treat × post × tied at base			0.773*		0.018
			(0.457)		(0.060)
total effect for			1.121**		-0.012
workers tied at base			(0.465)		(0.059)
N	2686	2578	2578	2534	2534

Notes: *** stands for p-value<0.01, ** stands for p-value< 0.05, * stands for p-value< 0.10. Standard errors are clustered at spot level. Sample is restricted to males in other poor households who work for another household in either survey wave. The dependent variable in column 1 is a dummy variable equal to 1 if an employer of the male respondent in the household is reported as a source of food transfers in times of need. The dependent variable in columns 2 and 3 is his wage earnings per hour. The dependent variable in columns 4 and 5 is the ratio of his monthly wage earnings during the month in which his wage earnings were minimum to his earnings during the month in which her wage earnings were maximum. "treat" is a dummy variable =1 if the observation is from a treatment village. "post" is a dummy variable =1 if the observation if from followup survey. "tied at base" is a dummy equal to 1 if the respondent was in a tied labor contract at baseline. All regressions control for the following variables: a dummy for whether the religion of the household head was Islam, whether the respondent was undernourished at baseline (BMI<18.5), log of baseline household wealth, whether the respondent reported being able to read and write at baseline, number of under 10 household members, average wealth class of the respondent's employer(s), whether the survey month was "kartik" (lean season)

Table 11: Wealth and Reciprocity of Food Exchange Links

	Log Wealth	Reciprocity	Reciprocity with upper class	Reciprocity with middle class	Reciprocity with non-ultra poor	Reciprocity with ultra poor
	(1)	(2)	(3)	(4)	(5)	(6)
treat	-0.056***	0.014	-0.041	-0.005	-0.018	0.053
	(0.019)	(0.024)	(0.031)	(0.027)	(0.035)	(0.048)
post	0.072***	0.046***	0.057**	0.061***	-0.006	0.025
	(0.013)	(0.017)	(0.029)	(0.020)	(0.026)	(0.043)
treat × post	0.056***	0.084***	0.152***	0.067***	0.132***	0.041
	(0.017)	(0.022)	(0.039)	(0.025)	(0.034)	(0.049)
N	7307	7257	1586	5538	2557	1737
Sample	Selected poor	Selected poor	Selected poor	Selected poor	Selected poor	Selected poor

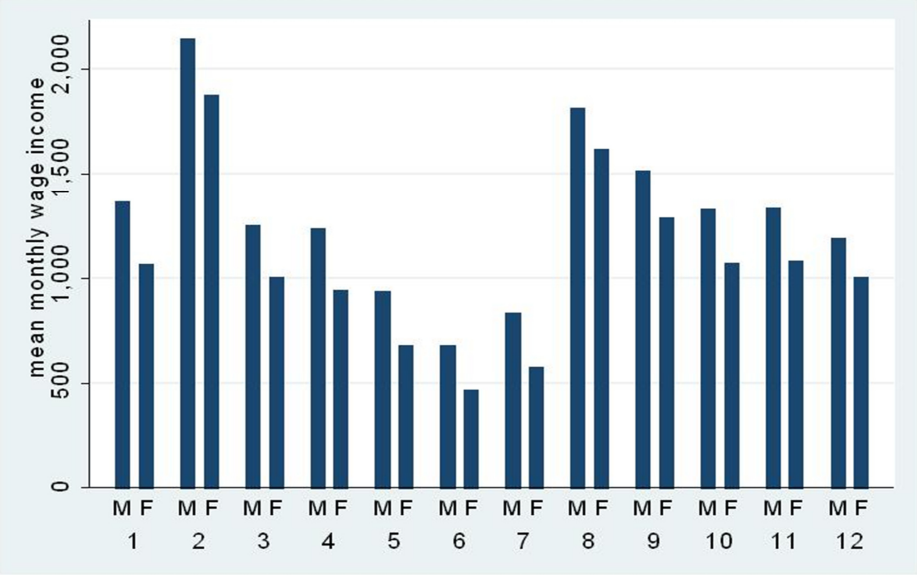
Notes: *** stands for p-value<0.01, ** stands for p-value< 0.05, * stands for p-value< 0.10. Standard errors are clustered at spot level. "treat" is a dummy variable =1 if the observation is from a treatment village. "post" is a dummy variable =1 if the observation if from followup survey. Sample is restricted to ultra poor households in all the regressions. The dependent variable in column 1 is the log average wealth (total value of household assets) of the households that the respondent's household exchanges (either transfers to or receives transfers from) food with, where wealth is as measured at baseline census. The dependent variable on column 2 is the proportion of food exchange links that the respondent's household receives food from (in times of need), that are also reported as links her household transfers food to. The dependent variable on columns 3, 4, 5 and 6 are the proportion of upper class, middle class, other poor and ultra poor (respectively) food exchange links that the respondent's household receives food from, that are also reported as links her household transfers food to.

Table 12: Mechanisms

	Mechanism 1: Hours spent in chores by employer females	Mechanism 2: Assistance received by non-selected women
	(1)	(2)
treatment	-0.721	0.057
	(27.942)	(0.045)
post	-3.639	0.078**
	(24.011)	(0.040)
treat × post	-82.948**	-0.041
	(33.288)	(0.057)
treat × post × "tied at base"	367.531**	0.262***
	(183.159)	(0.101)
total effect for "tied at base"	284.584	0.222**
	(183.193)	(0.096)
N	10086	2308
Sample	Employers	other poor

Notes: *** stands for p-value<0.01, ** stands for p-value< 0.05, * stands for p-value< 0.10. Standard errors are clustered at spot level. "treat" is a dummy variable =1 if the observation is from a treatment village. "post" is a dummy variable =1 if the observation if from followup survey. In Column 1, the sample is restricted to households that report *employing* another household within the same village in either survey wave. The dependent variable is total number of hours spent doing household chores by the main female respondent during the past year. "tied at base" is a dummy variable equal to 1 if the household employed any treated poor female as a tied worker at baseline. In Column 2, the sample is restricted to other poor females who report visiting any of the following places during the past year: local market, local health center, NGO office, court, livestock office, agricultural office. The dependent variable in column 2 is a dummy variable equal to 1 if the respondent reports receiving any assistance while visiting any of the mentioned places. "tied at base" is a dummy variable equal to 1 if the main female respondent in the household was working as a tied worker for another household at baseline.

FIGURE 1: SEASONALITY OF EARNINGS FROM WAGE EMPLOYMENT



Notes: This figure shows the monthly earnings from wage employment for male and female workers respectively. Every bar labeled "M" gives the mean total wage income of male household heads from wage employment in a given month. Every bar labeled "F" gives the mean total wage income of main female respondents from wage employment in a given month. Sample is restricted to baseline observations.

FIGURE 2: EQUILIBRIUM OCCUPATIONAL CHOICE

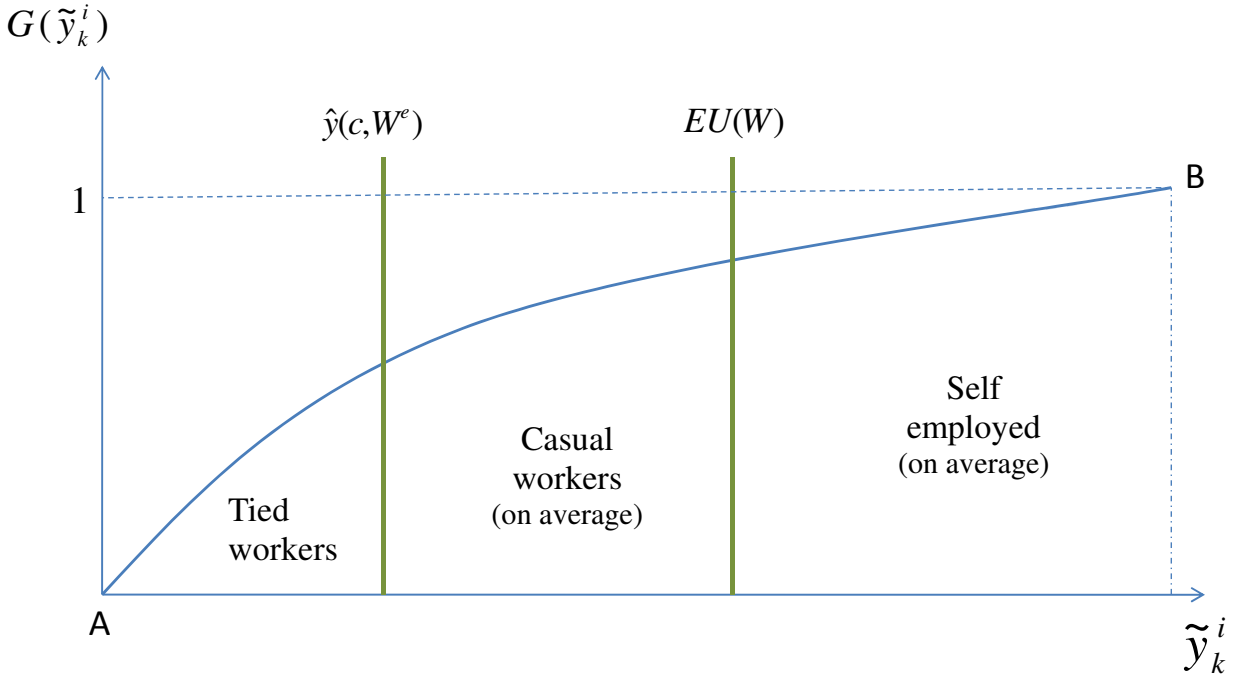
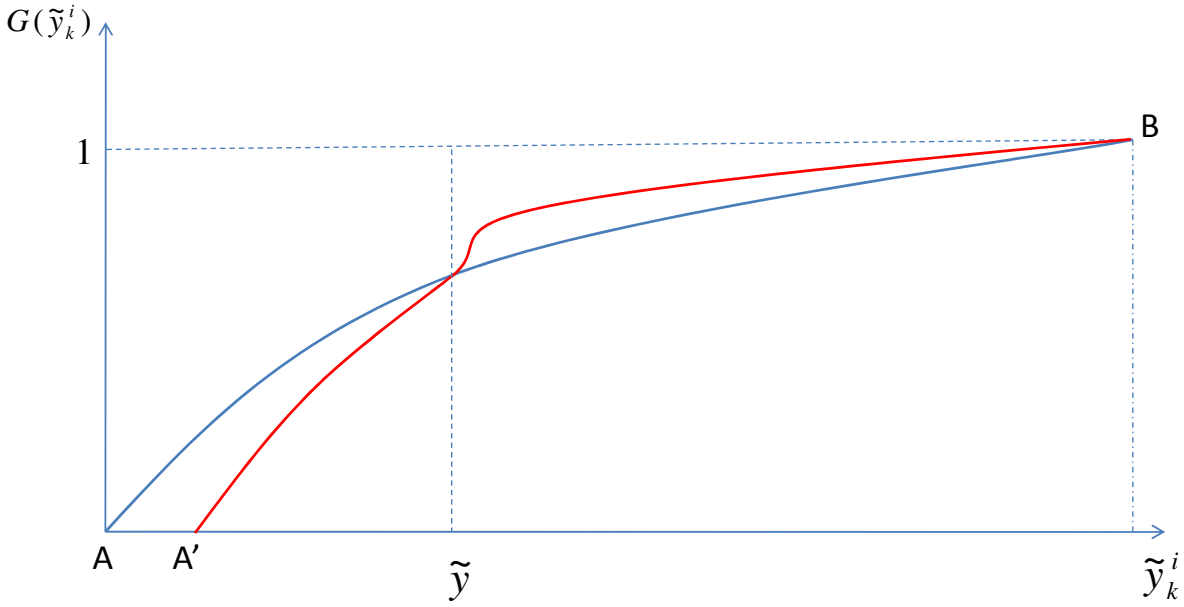


FIGURE 3: AGGREGATE EFFECT ON OUTSIDE OPTIONS



1.A Appendix

1.A.1 Proofs

Proposition 1 If $u(W(A)) \leq \widetilde{y}$ then $\frac{dW}{d\lambda} \geq 0$, $\frac{dz}{d\lambda} \geq 0$ and $\frac{d\widehat{y}}{d\lambda} \geq 0$.

Proof. Totally differentiating the system of equations given by (12), (13) and (14) gives:

$$\Omega_{3x3} \cdot \begin{bmatrix} dW \\ dz \\ d\widehat{y} \end{bmatrix} = \begin{bmatrix} 0 \\ NG_{\lambda}(u(W)) \\ \frac{\beta}{1-\beta^2}\left(F'\left(\left(\frac{NG(\widehat{y})}{x}\right)\frac{N}{x}G_{\lambda}\left(\widehat{y}\right)\widehat{y}\right)\right) \end{bmatrix} \cdot d\lambda \tag{19}$$

where

$$\Omega = \begin{bmatrix} -\frac{\beta}{1-\beta^2}\int\limits_{\frac{NG(\widehat{y})}{x}}^{\overline{A}} dF(A) & \frac{1}{1-\beta} & 0 \\ -NG'(u(W))u'(W) & 0 & 0 \\ \int\limits_{\frac{NG(\widehat{y})}{x}}^{\overline{A}} u'(W)dF(A) & \frac{u'(z)}{1-\beta} & -\left[\frac{1}{1-\beta^2}+\frac{\beta}{1-\beta^2}\left(F'\left(\frac{NG(\widehat{y})}{x}\right)\frac{NG'(\widehat{y})}{x}\widehat{y}+F\left(\frac{NG(\widehat{y})}{x}\right)\right)\right] \end{bmatrix} \tag{20}$$

The first row, second and third rows of Ω are derived by totally differentiating equations (12), (13) and (14) respectively. The inverse of Ω is given by:

$$\Omega^{-1} = \begin{bmatrix} 0 & -\frac{1}{NG'(u(W))u'(W)} & 0 \\ 1-\beta & \frac{\frac{\beta(1-\beta)}{1-\beta^2}\int\limits_{\frac{NG(\widehat{y})}{x}}^{\overline{A}} dF(A)}{-\frac{NG'(u(W))u'(W)}{}} & 0 \\ -\frac{u'(z)}{\theta} & \mu & -\frac{1}{\theta} \end{bmatrix} \tag{21}$$

where

$$\theta = \left(\frac{1}{1-\beta^2}+\frac{\beta}{1-\beta^2}\left(F'\left(\frac{NG(\widehat{y})}{x}\right)\frac{NG(\widehat{y})}{x}\widehat{y}+F\left(\frac{NG(\widehat{y})}{x}\right)\right)\right) \tag{22}$$

and

$$\mu = \frac{\left[-\frac{\beta}{1-\beta^2}u'(z)\int\limits_{\frac{NG(\widehat{y})}{x}}^{\overline{A}} dF(A) - \int\limits_{\frac{NG(\widehat{y})}{x}}^{\overline{A}} u'(W)dF(A)\right]}{NG'(u(W))u'(W)\left[\frac{1}{1-\beta^2}+\frac{\beta}{1-\beta^2}\left(F'\left(\frac{NG'(\widehat{y})}{x}\right)\frac{NG'(\widehat{y})}{x}\widehat{y}+F\left(\frac{NG'(\widehat{y})}{x}\right)\right)\right]} \tag{23}$$

This implies that:

$$\begin{pmatrix} dW \\ dz \\ d\widehat{y} \end{pmatrix} = \Omega^{-1} \cdot \begin{pmatrix} 0 \\ NG_\lambda(u(W)) \\ \frac{\beta}{1-\beta^2}\left(F'\left(\frac{NG(\widehat{y})}{x}\right)\frac{N}{x}G_\lambda(\widehat{y})\widehat{y}\right) \end{pmatrix} \cdot d\lambda \tag{24}$$

Hence

$$\frac{dW}{d\lambda} = -\frac{1}{NG'(u(W))u'(W)}NG_\lambda(u(W)) \tag{25}$$

$$\frac{dz}{d\lambda} = -\frac{\int_{\frac{NG(\widehat{y})}{x}}^{\overline{A}} dF(A)}{NG'(u(W))u'(W)}NG_\lambda(u(W)) \tag{26}$$

$$\frac{d\widehat{y}}{d\lambda} = \mu \cdot NG_\lambda(u(W)) + \frac{-\beta\left(F'\left(\frac{NG(\widehat{y})}{x}\right)\frac{N}{x}G_\lambda(\widehat{y})\widehat{y}\right)}{\left(1+\beta\left(F'\left(\frac{NG(\widehat{y})}{x}\right)\frac{NG(\widehat{y})}{x}\widehat{y}+F\left(\frac{NG(\widehat{y})}{x}\right)\right)\right)} \tag{27}$$

Note that by definition of the shift λ, $G_\lambda(u(W)) \leq 0$for $u(W) \leq 0$. This implies that $\frac{dW}{d\lambda} \geq 0$ and $\frac{dz}{d\lambda} \geq 0$.

To evaluate the sign of $\frac{d\widehat{y}}{d\lambda}$, note that $\mu \leq 0$. Hence, for $u(W) \leq \widetilde{y}$, the first term in (27) will be non-negative. To evaluate the sign of the second term in (27), note that $u(W(A)) \geq \widehat{y}$, hence for $u(W) \leq \widetilde{y}$ it is the case that $\widehat{y} \leq \widetilde{y}$ and $G_\lambda(\widehat{y}) \leq 0$. Therefore the second term in (27) is also non-negative. This implies that $\frac{d\widehat{y}}{d\lambda} \geq 0$. ■

1.A.2 Appendix Tables

Table 13: Characteristics of Ultra Poor, by Gender of Household Head

	Male-headed poor	Female-headed poor	Difference p-value
	(1)	(2)	(3)
Wealth	6367.6	4617.7	0.01
	(28994.3)	(31426.0)	
Pce	3784.7	4209.9	0.05
	(1998.1)	(2597.7)	
Household size	4.10	2.08	0.00
	(1.44)	(1.25)	
Working-age member	2.62	1.57	0.00
	(0.95)	(0.86)	
Main female literate	0.10	0.03	0.00
	(0.30)	(0.17)	
Main female undernourished	0.51	0.55	0.00
	(0.50)	(0.50)	
Can have at least	0.45	0.37	0.00
2 meals a day	(0.52)	(0.51)	
N	3923	2823	

Notes: Summary statistics for ultra poor households at baseline provided. In Column 1, the sample is restricted to male-headed ultra poor households and in Column 2 to female-headed ultra poor households. Column 3 provides the p-values associated for a test of mean differences for each characteristic for the two sub-samples.

Table 14: Normalized Differences

	Ultra Poor			Other Poor			Middle class			Upper class		
	Mean		Normalized	Mean		Normalized	Mean		Normalized	Mean		Normalized
	Control	Treat	Difference	Control	Treat	Difference	Control	Treat	Difference	Control	Treat	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Household (HH) Characteristics												
Wealth	6,573.97	4,936.53	0.04	14,457.20	13,587.31	0.01	161,835.10	144,640.60	0.04	876,366.30	830,096.20	0.03
Per capita total expenditure	3,910.39	3,993.53	-0.03	4,223.05	4,283.96	-0.01	5,682.54	5,434.19	0.03	13,085.06	10,880.77	0.05
Per capita food expenditure	2,877.29	2,967.99	-0.05	2,920.28	2,970.59	-0.03	3,311.52	3,314.85	0.00	4.372.49	4,509.10	-0.05
total HH income from business activities of all members	17,393.24	17,962.61	-0.03	23,569.54	21,258.94	0.11	39,328.74	34,530.44	0.09	90,490.07	76,899.74	0.11
Maled headed HH	0.52	0.63	-0.15	0.80	0.77	0.05	0.94	0.94	0.00	0.95	0.95	0.01
HH size	3.10	3.38	-0.12	3.73	3.67	0.03	4.40	4.46	-0.02	5.07	4.97	0.04
Main female respondent literate	0.07	0.08	-0.02	0.17	0.15	0.04	0.28	0.26	0.02	0.54	0.50	0.06
Main female respondent numerate	0.91	0.96	-0.14	0.93	0.96	-0.09	0.95	0.97	-0.05	0.97	0.98	-0.07
Business Activities of Main Female Respondent												
Proportion engaged in any business activity	0.87	0.83	0.09	0.83	0.79	0.07	0.87	0.85	0.05	0.93	0.87	0.13
Total hours spent on all business activities	1,234.15	1,068.93	0.13	978.11	901.07	0.07	800.45	822.90	-0.02	808.94	815.70	-0.01
Proportion who works for a wage	0.58	0.53	0.06	0.37	0.36	0.00	0.09	0.10	-0.03	0.03	0.02	0.03
Proportion who, conditional on working for a wage, works for someone in village	0.47	0.53	-0.09	0.51	0.53	-0.03	0.41	0.51	-0.14	0.03	0.07	-0.15
Total hours spent in wage employment within village	349.03	324.98	0.03	212.18	205.48	0.01	40.17	51.79	-0.03	0.78	1.35	-0.01
Business Activities of Male Household Head												
Proportion engaged in any business activity	0.78	0.78	-0.01	0.83	0.79	0.08	0.81	0.81	0.00	0.80	0.83	-0.05
Total hours spent on all business activities	1,677.54	1,642.21	0.02	1,877.52	1,741.36	0.09	1,897.37	1,894.02	0.00	1,774.45	1,861.52	-0.05
Proportion who works for a wage	0.59	0.63	-0.06	0.63	0.60	0.05	0.37	0.41	-0.05	0.15	0.13	0.05
Proportion who, conditional on working for a wage, works for someone in village	0.34	0.33	0.02	0.34	0.34	0.01	0.32	0.33	-0.02	0.07	0.08	-0.01
Total hours spent in wage employment within village	326.23	325.25	0.00	326.61	318.48	0.01	175.00	196.69	-0.03	12.30	13.48	-0.01
Food Exchange Network of the Household												
Proportion who reports anyone as a source of food transfers at times of need	0.91	0.93	-0.05	0.93	0.91	0.05	0.84	0.81	0.05	0.46	0.39	0.09
Number of households reported (0-3) as a source of food transfers at times of need	2.27	2.13	0.11	2.34	2.06	0.21	2.06	1.80	0.17	1.03	0.77	0.16
Proportion who reports giving out food transfers to others who may be in need	0.41	0.46	-0.07	0.54	0.50	0.05	0.75	0.70	0.08	0.83	0.79	0.07
Number of households reported (0-3) as a destination of food transfers out	0.97	1.02	-0.03	1.33	1.12	0.11	1.82	1.56	0.15	2.03	1.75	0.18
Log Average wealth of household reported as food transfer sources or destinations	2.66	2.63	0.06	2.76	2.71	0.10	2.91	2.86	0.09	2.76	2.69	0.10
Proportion of food sources that are also reported as food destinations (reciprocity)	0.36	0.38	-0.04	0.46	0.44	0.03	0.67	0.62	0.08	0.76	0.71	0.08

Notes: The normalized differences between the treatment and control observations are calculated based on Imbens and Wooldridge (2009) where the normalized difference is given by $\Delta_X = \frac{\overline{X_1} - \overline{X_0}}{\sqrt{S_0^2 + S_1^2}}$ where $\overline{X_1}$ is the sample mean of treatment observations, $\overline{X_0}$ is the sample mean of control observations, S_0^2 is the sample variance of control observations and S_1^2 is the sample variance of treatment observations.

2 Can Entrepreneurship Programs Transform the Economic Lives of the Poor?[28]

2.1 Introduction

The world's poor lack both capital and skills (Banerjee and Duflo (2007)). They also tend to be employed in low return and often insecure occupations. This is true for both developed and developing countries. These simple observations have informed how we think about poverty. One strand of work examines mechanisms via which expanded access to capital can enable individuals to alter their occupational and production choices and exit poverty (Banerjee and Newman (1993); Besley (1995)). Another strand focuses on human capital formation and on how limited education and skills constrain the occupational and production opportunities of the poor (Becker (1964); Schultz (1979, 1993)).

Guided by these theoretical foundations the worldwide poverty industry spends millions on government, NGO, multilateral, bilateral and private sector led antipoverty programs. Banking, microfinance and asset transfer programs figure prominently in efforts to tackle the capital constraint. Education, adult education, vocational training, conditional cash transfers and skill transfer programs spearhead attempts to tackle the skills constraint. The amounts of money spent in the name of poverty reduction are enormous in both developed and developing nations.

And yet whether these plethora of programs are actually enabling the poor to permanently exit poverty by allowing them to move into higher productivity occupations is often called into question. One reason to be skeptical is that we do not really know what works and the results from credible evaluations often fall short of the heady expectations of the agencies that fund these programs (Banerjee *et al* (2010), Crepon *et al* (2011)). This understandably has led to calls for a stronger focus on evaluation to guide expenditures on anti-poverty programs (Banerjee and Duflo (2011)).

But there are other, more fundamental, reasons to be skeptical. Transfers of various types may simply be consumed without altering the underlying productivity of poor individuals. In many instances, these transfers are sizable relative to baseline wealth levels, and hence wealth effects might dominate. In addition, the poor may have very limited demand for either capital or skills. This often makes them difficult to reach via anti-poverty programs. It has been observed, for example, that even the most innovative finance programs (e.g. microcredit) often fail to reach the poorest who

[28]The work in this chapter was carried out jointly with equal share by Oriana Bandiera, Robin Burgess, Imran Rasul, Munshi Sulaiman and me.

may depend largely on low paid wage labor and hence have limited use for capital. Similarly if the returns to education are perceived to be very low then the poor are unlikely to participate in education or skills programs (Jensen (2010)). This suggests that transforming the economic lives of the poor may require tackling both capital and skill constraints simultaneously.

To shed light on these issues, we conduct a randomized evaluation of a basic entrepreneurship program in Bangladesh – a program that offers both training and assets to the poorest women in rural communities, typified by being largely assetless and low skilled, and generally stuck in low return and insecure occupations. The program is operated by BRAC, the largest and fastest growing NGO in the world, and aims to target over 800,000 ultrapoor women in the poorest areas of the country by 2011. The scale of the program in Bangladesh and the fact that it is now being replicated in a large number of countries imply that the results of the evaluation can be crucial for many of the world's poorest.[29]

The program targets both the lack of assets and the lack of skills by transferring business assets – mostly livestock like cows and goats – and training and support in running small businesses, with the aim of moving them from low return occupations to entrepreneurship. The value of the asset transfer is large relative to the beneficiaries' wealth, half of whom owns no assets at baseline. This has two implications. First, wealth effects are likely to be strong and might entirely drive the way treated households respond to the program. Second, the program is likely to have general equilibrium effects on wages and prices in the treated communities, possibly creating spillovers on non treated households.

To guide our research design and empirical analysis, we develop a simple theoretical model of labor supply and occupational choice that captures the main features of our context and makes precise the conditions under which the program achieves its goal of promoting entrepreneurship and self-employment. In the model, individuals are endowed with exogenous (non-labor) income, entrepreneurial skills and time which can be allocated between self-employment, wage-labor and leisure. Self-employment hours are combined with capital and entrepreneurial skills to produce output. As such, the net return to self-employment depends on the price of output, the agent's skills and the rental rate of capital. The return to wage-labor hours is given by the wage rate which we assume to be the same for everyone[30]. We allow for the possibility that individuals

[29] As of November 2011, ten different pilots were active around the world, http://graduation.cgap.org/pilots/

[30] In the baseline model we assume that all prices (wage, rental rate of capital and price of output) are exogenously determined. We relax this assumption further on when we consider the general equilibrium effects of the program.

may be constrained in the maximum amount of time they can spend in self-employment, due to lack of complementary assets.

The model, though simple in formulation, yields a rich set of equilibrium occupational choices, all of which are observed at baseline. Specifically, if the asset constraint does not bind, individuals who have high exogenous income or low self-employment returns choose to stay out of the labor force (16% of the sample at baseline), while others specialize in self-employment (29% of the sample) or wage-labor (28% of the sample) depending on whether their return to self-employment is higher than the wage rate. Individuals who face a binding asset constraint might engage in both occupations (27% of the sample).

The model illustrates how the effect of the program on occupational choice is ambiguous because its two components -asset transfer and the training- generally push in opposite directions. To be precise, the asset transfer essentially acts an increase in wealth, either through assets sales or rentals, and this reduces labor force participation and the time devoted to either self-employment or wage labor through the standard wealth effect. The only case in which the asset transfer can increase the time devoted to self-employment is by relaxing a binding asset constraint. The extent to which asset transfers promote entrepreneurship therefore depends on whether the ultrapoor face binding asset constraints.

In contrast, the training component should increase the returns to self-employment and thorough this increases labor force participation and the time devoted to self-employment and decrease the time devoted to wage labor. The only case in which training can reduce the time devoted to self-employment is for individuals already engaged in self-employment if the income effect is larger than the substitution effect.

The framework thus makes precise that the effect of the program is generally ambiguous and heterogeneous depending on whether individuals face binding assets constraints at baseline. While the level of the constraint is not observable, the model makes clear how these map into occupational choices at baseline, and hence yields heterogeneous effects depending on baseline occupations that we bring to the data.

The partial equilibrium effects described above imply that the program generates an exogenous drop in the supply of unskilled labor, and an increase in the stock of livestock assets and their produce (e.g. milk, eggs) in treated communities. Hence, in general equilibrium the program weakly increases unskilled wages and has an ambiguous effect on self-employment returns. These general equilibrium effects are likely to induce changes on the occupational choices of non-treated poor households. In particular, using the same theoretical framework as above, we predict that higher unskilled wages will weakly increase labor force participation and labor hours among the non-treated

poor individuals.

Our evaluation strategy was designed explicitly to provide evidence on the occupational choices of the ultrapoor, on general equilibrium effects and on the spillovers on other poor in treated communities. Our research design has three key features. First, we collaborated with BRAC to randomize the roll-out of the program across communities, half of which were treated in 2007 and the rest kept as controls until 2011. The program selected potential beneficiaries, following the same selection criteria in both treatment and control communities, we thus identify the effect of the program by comparing the outcome of the selected poor in treated vs. control communities before and after program implementation. Randomization at the community level, rather than at the individual household level, reduces the risk that the program effects spillover to the control group. Our sampling strategy was designed to measure both the effect on the treated, general equilibrium effects through prices and spillover effects on the non-treated poor in treated communities. To this aim we sampled all of the ultrapoor and other poor households in treatment and control communities, as well as a random sample of the rest of the population in each community.

Second, we collect detailed information on occupational structure, and in particular on the time devoted to different income generating activities. This allows us to identify the effect of the program on occupational choice, and to assess whether it succeeds into its stated aim of transforming the occupational structure and the economic lives of the poor, over and above increasing their wealth and short-run welfare. Third, the scale of the evaluation is large. We survey all eligibles, all poor households and a sample of households at other points of the income distribution for a total of 25,068 households across 1,409 communities. This allows us to quantify general equilibrium effects on wages and prices at the community level and to identify spillovers on non-treated households.

The analysis yields five main findings. First, two years into the program, the treated women retain the assets they were given and change their occupational choices accordingly. On average, treated women increase the hours devoted to self-employment by 135%, decrease the hours devoted to wage-labor by 14%, and increase labor force participation by 13 percentage points. Taken together, this change in occupational structure is associated with an increase in income of 35%, which results in an increase in standard welfare measures such as food security (42%), food PCE (5%), price per calorie (3%) and non-food PCE (22%). Most importantly, the findings suggest that the increase in welfare is due to the transformation of occupational structure rather than to a consumption boost due to the asset transfer. Our results importantly suggest that even the poorest individuals, who lack both capital and skills at baseline, can learn to suc-

cessfully operate small businesses and in doing so significantly improve their welfare to a point where in terms of income and per capita expenditure they resemble the middle classes in the rural communities that they inhabit in Bangladesh.

Second, to shed light on whether findings are driven by the asset component relaxing asset constraints, the training component improving skills or both we allow the effect of the program to be heterogeneous as a function of baseline occupational choices. The findings indicate that the program increases the hours devoted to self-employment both for individuals who were likely to be asset constrained at baseline, namely those engaged in both occupations, and for all the others, namely those specialized in one occupation or out of the labor force. In light of the theoretical framework, this indicates that the effect of the program works through both components.

Third, we provide evidence on the general equilibrium effects of the program at the community level. We find that the program causes a significant increase in the wage level for female unskilled labor, but has no impact on the male wages, suggesting that the unskilled labor market in this setting is highly segmented by gender. Furthermore, we find that the program leads to a fall in the price of goats, but has no impact on the prices of other types of livestock or produce. This is consistent with the fact that the assets transferred by BRAC correspond to a much larger exogenous shock on the aggregate amount of goats within the community compared to other types of livestock.

Fourth, we test for spillover effects on the occupational choices of non-treated women who live in treated communities. We find that – consistent with there being significant general equilibrium effects on female wages and goat prices – non-treated poor women in treatment communities increase labor force participation by 4 percentage points and spend 12% more hours in wage-labor. Taken together, this change in occupational structure is associated with an increase in income of 11%. Finally, we find no impact on the labor supply or income of men from non-treated poor households, which lines up with our finding that there were no general equilibrium effects of the program on the male labor market.

Fifth, our results suggest that the program benefits exceed those that would accrue from an equally costly unconditional cash transfer. The additional income generated by the cash transfer is estimated to be 70% of the income effect on treated individuals and 50% of the income effect once we take into account spillover effects on untreated poor households in treatment communities. The welfare effects of this entrepreneurship program therefore are large relative to those that would be predicted based on an equally costly cash transfer, which suggests that the program is having a transformative effect on the economic lives of the poor in these rural communities. This lines up with effects we observe on labor supply and occupational choice in the data. Moreover the

spillover effects are also sizable which implies that ignoring these effects (as is standard in evaluations that look only at the effect on the treated) would underestimate benefits considerably.

Our paper contributes to a growing literature that tries to identify ways of shifting poor individuals into higher return economic activities as a means of permanently lifting them out of poverty. Much of the literature has focused on expanding access to capital (e.g. Banerjee *et al* (2010); Crepon *et al* (2011)) or upgrading skills (e.g. Schultz (2004)). What is novel about the entrepreneurship program we examine here is the simultaneous provision of both capital and skills[31]. Our results suggest that targeting capital and skills shortages which constrain entrepreneurship may be key to enabling poor individuals to take up higher return economic activities.

Finally the scale of our evaluation which covers both treated and untreated individuals in both treatment and control communities has revealed how important it is when examining the impact of a program to think about both general equilibrium and spillover effects. This is in line with a growing literature on program evaluation in both developed and developing countries which is beginning to document the importance of spillovers and externalities in different settings (see Angelucci and De Giorgi (2009), Angelucci *et al* (2010), Bobonis and Finan (2008), Cattaneo and Lalive (2006), Gertler *et al* (2006), Kremer and Miguel (2004), Ludwig *et al* (2011)). Fuller evaluations which attempt to trace general equilibrium and spillover effects beyond the direct effects on the treated are going to be necessary to make them more informative for policy making.

The rest of the paper is organized as follows: In section 2.2 we describe the details of BRAC's ultra poor program and our evaluation strategy; section 2.3 describes lives of the ultra poor households relative to the rest of the community at baseline; section 2.4 presents the theoretical framework and its predictions; section 2.5 presents empirical results on the effect of the program on treated households; section 2.6 presents results on the general equilibrium effects of the program and on spillover effects on poor households that are not treated and section 2.7 concludes.

2.2 Program and Evaluation

2.2.1 Program Description

BRAC's ultrapoor program targets the poorest women in rural Bangladesh with a multi-faceted intervention aimed at transforming their economic lives to permanently lift them out of poverty. The program started in 2007 and aims to reach 860,300

[31]Our results are also in line with the non-randomized evaluations of an earlier version of BRAC ultrapoor program, see e.g. Akher et al (2009), Das and Misha (2010), Emran et al (2009).

households in 40 districts by 2011, at the cost of TK20,700 (USD 300) per household.

The program has two main components. First, targeted women receive a productive asset, such as cows, goats, poultry or seeds for vegetable cultivation. The average asset value is TK9,500 (USD 140), which is a sizable fraction of the targeted poors' wealth at baseline. In principle, participants commit to retain the asset for two years with the exception that they are allowed to sell it or exchange it for another income generating asset within that period. In practice, however, the commitment cannot be enforced, thus whether the asset is retained or liquidated is itself an outcome of interest that ultimately determines whether the program has the desired effect to transform the lives of the poor or merely increases their welfare in the short run.

Second, the asset transfer is accompanied by skills training, specific to the type of asset provided.[32] The training component is both intensive and long lived. Indeed, besides initial classroom training at BRAC headquarters, households receive regular support by an asset specialist who visits them every 1-2 months for the first year of the program and by BRAC program officers who visit them weekly for the first two years.[33]

Targeting proceeds in three stages. First the BRAC central office selects the most vulnerable districts based on the food security maps by the World Food Program. Second, BRAC employees from local branch offices within those districts select the poorest communities within their branch. Communities or "spots" are self-contained within-village clusters of approximately 100 households.

Third, program officers use a combination of participatory wealth ranking methods (Alatas et al 2011) and survey methods to identify the ultrapoor women who will be targeted in each community. Through a participatory rural appraisal (PRA) households are allocated to one of five community-defined wealth bins.[34] The lowest ranking households are then visited by BRAC officers to determine whether they meet the program's selection criteria to become Specially Targeted Ultrapoor (STUP) households, or STUPs for brevity.[35]

[32]To compensate for the short run fall in income due to the occupational change, a subsistence allowance is provided for the first 40 weeks, that is until the targeted women learn to manage the assets well enough to generate a regular flow of income.

[33]Between 18 and 24 months into the program, the targeted women receive training in microfinance and are enrolled in village-level microfinance organizations. Our followup survey is fielded before the treated women have access to microfinance, hence we do not evaluate the effect of this component.

[34]In a randomized evaluation of different targeting methods, Alatas et al (2011) show that, compared to proxy means tests, community appraisal methods resulted in higher satisfaction and greater legitimacy. Their distinctive characteristic was that community methods put a larger weight on earnings potential.

[35]There are three exclusion criteria, all of which are binding: (i) already borrowing from an NGO providing microfinance, (ii) receiving of government anti-poverty programs, (iii) having no adult women in their members. Furthermore, to be selected a household has to satisfy three of the following five inclusion criteria: (i) total land owned including homestead is not more than 10 decimals; (ii) there

2.2.2 Evaluation Strategy and Survey Design

We collaborate with BRAC to randomize the roll-out of the program across 40 BRAC office branches selected by BRAC central offices in the poorest areas of the country. To reduce unobservable heterogeneity between treatment and control units we stratify by subdistrict and use pairwise randomization between branches in each subdistrict. An average subdistrict (upazila) has an area of approximately 250 square kilometers (97 square miles) and constitutes the lowest level of regional division within Bangladesh with any administrative power and elected members (Hasan,1992). As such, communities within the same upazila are subject to the same local governance structures, experience similar local policies and are likely to have similar characteristics that affect the outcome of interest. Stratifying at the subdistrict level can therefore lead to better balance between treatment and control groups (Bruhn and Mckenzie, 2009).

The randomization was carried out remotely by the research team. We first randomly selected two branches in each subdistrict and then we randomly allocated one to treatment and one to control.[36] Figure 4 shows the location of treatment and control branch offices within Bangladesh, each of which covers 35 communities on average. The randomization design implies that all communities within the 20 treatment branches are treated in 2007 and all communities within the 20 control branches are kept as controls until 2011. We use BRAC branch offices instead of communities as the unit of randomization to minimize the risk of contamination between treatment and control units, both because communities within the same branch office are closer to each other and because, most importantly, this minimizes the risk that program officers, who are based at the branch, do not comply with the randomization.

Four features of the randomization design are of note. First, we asked BRAC officers to carry through the selection process outlined above both in treatment and control communities, so that STUPs are identified in both but only treated in treatment communities. This allows us to estimate the effect of the program by comparing the outcomes of STUPs in treated communities to those of STUPs in control communities before and after the introduction of the program, thus differencing out baseline differences in outcomes between treatment and control communities and common time trends.

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is no adult male income earner in the household; (iii) adult women in the household work outside the homestead; (iv) school-going-aged children have to work; and (v) the household has no productive assets.

[36] For each district located in the poorer Northern region we randomly select 2 sub-districts, and for each district located in the rest of the country we randomly select 1 subdistrict, restricting the draw to subdistricts containing more than one BRAC branch office. For the one district (Kishoreganj) that did not have subdistricts with more than one BRAC branch offices, we randomly choose on treatment and one control branch without stratifying by subdistrict.

Second, the fact that all STUPs within a community are either treated or kept as controls eliminates possible confounding effects due to control contamination by ensuring that control STUPs do not know and hence cannot respond to not having been treated. The average distance between a treatment and a control branch office is approximately 12 kilometers.

Third, to ensure that the estimates are not contaminated by anticipation effects, households are only told about the program when this is actually implemented. Hence, neither treatment nor control STUPs know about the program at baseline, treated households find out in 2007 and control households in 2011. As BRAC already operates in all selected communities, the participatory wealth ranking exercise is justified as part of BRAC's regular activities.

Fourth we survey all STUPs, all poor households and a random sample of non-poor households within each community to evaluate both the direct effect of the program on the treated and its spillover effects on non-treated households in treated communities.

The sample covers 1409 communities and 25,068 households. The cost of the evaluation is estimated at USD 1.95 million. Households in treatment and control communities were surveyed before the program reached their subdistrict and then again two years later. The average number of days between the baseline and the first follow-up is 800. This implies that follow-up outcomes are measured after the end of the most intensive part of the program with weekly visits, and that treated households were free to liquidate their assets.[37]

The survey questionnaire collects a rich set of individual outcomes, and particularly detailed information on occupational choice, labor supply and income that allows us to shed light on whether the program had the desired effect of transforming the economic lives of the poor.

2.3 The Lives of the Ultra Poor at Baseline

The participatory rural appraisal exercise yields a complete classification of households by wealth.Table 15 provides summary statistics on the characteristics of ultra-poor households and other households belonging to other wealth classes as ranked by the communities at baseline.[38] The first row lists the number of households in each class. We sampled all the selected ultrapoor, all the other poor, and 10% from the

[37]The baseline survey was carried out between April-December 2007. The first follow-up survey was carried out on the same households between April-December 2009, a second follow-up survey is currently on the field.

[38]We divide the poor classes (ranks 4 and 5) into those who were selected by the program (the ultra poor) and those who were not (other poor), the middle class comprises households that were ranked 2 or 3, the upper class those that were ranked 1.

other classes. That corresponds to a sample of 6,817 ultrapoor, 8,576 other poor, 7,241 middle class and 2,428 upper class households.

The second row shows that, in line with the program targeting strategy, the ultrapoor households are more likely to be female headed. The share of male headed households is 58% for the ultrapoor, 79% for the other poor, and close to 100% for all other classes. Household size (row three) is increasing in wealth, ranging from 3.3 for the ultrapoor to 5 for the upper class.

The next two rows show basic indicators of human capital, self-reported literacy as a measure of education and BMI as a measure of health. In both cases we report measures for the survey respondent, that is the main female in the household. Measures for household heads are correlated. Only 7% of ultrapoor women are literate and the share increases rapidly with wealth from 27% among leading women in the middle classes to 52% in the upper class. This gives a clear illustration of the low levels of human capital in these villages. The next row shows that BMI of the main female respondent is also increasing in wealth, with the ultrapoor being at the bottom of the lowest class with a 18.4 average, up to 20.3 in the upper class.[39]

The next three rows report measures of food security, expenditure and wealth among the sampled households. We define a household to have food security if its members can afford at least two meals a day on most days. According to this measure, only 41% of the ultrapoor households have food security, compared to 53% of other poor, 81% of middle and 96% of upper class households. Average per capita expenditure[40] by ultrapoor households is just over 2/3 of average per capita expenditure by households in the middle class and just under 1/3 of average per capita expenditure in the upper class. Differences in wealth are much starker. The corresponding figures are 4% and 0.7%. These are mostly driven by the fact that 45% of ultrapoor households have no assets at baseline. The average Gini coefficient for wealth is 0.77 in both treated and control communities.

The average ultrapoor in our sample receives an asset valued 9500TK (140USD). In the context of the distribution of business assets described in Table 15, the value of the assets BRAC transferred to ultrapoor households is roughly twice the mean value of ultrapoors' wealth at baseline. For the 45% of ultrapoor households who had no assets at baseline the transfer obviously entails an even more significant change in wealth. The

[39] In this setting, the relationship between BMI and health status is likely to be positive throughout, as the heaviest among the wealthiest individuals (i.e. those weighing 2 standard deviations above the mean) are just on the overweight threshold (25).

[40] All monetary values are in real terms, in 2007 prices. Values recorded during the 2009 survey have been deflated to 2007 prices using the Bangladesh Bureau of Statistics rural CPI index as of December 2009 (http://www.bbs.gov.bd/).

size of the transfers relative to the value of existing assets in the community implies that the program has a nontrivial impact on the distribution of wealth, pushing the ultrapoor out of the bottom class and possibly above some of the lowest classes. This in turn implies that program might affect occupational choice through a wealth effect.

The differences in business assets translate into differences in occupational structure. Panel B reports the annual hours devoted to wage and self-employment as well as total hours worked by the respondent. Three patterns are notable. First, ultrapoor women spend considerably more time selling labor outside the household compared to all other classes. Further decomposition (not shown for reasons of space) shows that maid and agricultural daily jobs account for 65% of hours devoted to wage labor. The hours devoted to these activities fall rapidly as we move up along the class structure and women in the middle and upper classes are very rarely involved in these activities.

Second, in line with the skewed distribution of assets, and in particular livestock and land, ultrapoor respondents spend less time in self-employment activities. The average for the main female in a ultrapoor household is 414 hours per year, compared to 700 for the main female in a middle class household and 770 in an upper class household.

Third, hours devoted to income generating activities decrease with wealth, and ultrapoor women spend roughly 50% more hours than women belonging to the upper classes. This implies that even in the poorest classes women are not underemployed, rather they are employed in activities (paid labor) that are likely to be less attractive to wealthier women. The average time spent on household chores is 1440, implying the average woman in our sample works 8 hours in a 6 day week.[41]

The last part of Table 15 gives the proportion of households in each wealth class that fall in one of four occupational groups: (i) those who work in wage employment alone, (ii) those who are engaged in self-employed income generating activities and in wage labor, (iii) those who work in self-employment only and (iv) those out of the labor force. Among the ultrapoor households, 28% were working in wage employment only at baseline, 27% worked in both self-employment and wage labor, 29% specialized in self-employment alone and 16% were out of the labor force. Note that, consistent with the distribution of labor hours between self and wage employment, as we go up along the wealth groups, women are more likely to be specialized in self-employment only – 87% of upper class women work only in self-employed business activities.

[41] For brevity we only report the occupational choices of women, but the pattern of self-employment versus wage work across wealth classes is similar for men, with poorer men devoting more time to wage work and less time to self-employed activities. Among wage employment, casual agricultural labor is the most common form, followed by a miscellany of activities such as construction works, rickshaw driving, shop vendors etc. The time devoted to business activities is similar across classes. For all classes, women devote much less time to income generating activities than men do. The women's share of total hours worked is highest for the ultrapoor – 40% – and declines with wealth.

Table 24 in the Data Appendix reports means in treatment and control groups separately, by wealth class. Following Imbens and Wooldridge (2009) the table also reports the normalized difference for each variable, computed as the difference in means divided by the square root of the sum of the variances. This is a scale-free measure and, contrary to the t-statistics for the null hypothesis of equal means, does not increase mechanically with sample size. Three points are of note. First, all normalized differences are quite small and even the largest (.13) are well below .25, the rule of thumb value below which linear regression methods are not sensitive to specification changes (Imbens and Wooldridge 2009). Second, the signs of the differences are consistent across wealth classes, especially between ultrapoor and other poor households, suggesting these are due to common unobservables at the community level rather than different selection criteria of ultrapoor households in treatment and control communities. For instance, women in treatment villages are between 2 and 5 percentage points less likely to participate in the labor force and consequently have lower average income in all wealth classes. Third, our research design allow to evaluate the effect of the program independently of baseline differences, by comparing changes in outcomes for the same household across treated and control communities.

2.4 Theoretical Framework

To guide the empirical analysis we now present a simple model of labor supply and occupational choice that captures the main features of our context and yields testable predictions on labor force participation, hours worked and time allocation between wage labor and self employment. The aim of the model is threefold. First to illustrate the conditions under which the two components of the program -asset transfer and training- can transform the economic lives of the poor by inducing a change in occupational choice. Second, to make precise how the effect of the program is heterogeneous depending on whether the ultra-poor faced binding asset constraints at baseline. Third, to illustrate how, by affecting the occupational choices of treated households, the program can have general equilibrium effects on the wage and the return to self-employment, and, through these, affect the occupational choice of non-treated households.

2.4.1 Set Up

We assume individuals' utility is additively separable in consumption (C) and leisure (R) and is given by $U = u(C_i) + v(R_i)$ where both $u(.)$ and $v(.)$ are increasing and concave. Individuals live for one period only, that is we do not analyze savings and investment decisions.

In the one period model, the value of consumption must equal the value of income. We assume that this consists of an exogenous component I_i, for instance husband's earnings or rental income, and earnings from wage labor and self-employment. Individuals are endowed with one unit of time and choose how many hours to work and how to allocate them between labor (L) and self-employment (S).

Given their low human capital, we assume individuals can only be employed in low-skilled jobs. We assume individuals are price takers in the labor market and each hour of unskilled labor pays wage w.

Self-employment hours are combined with capital K to produce output Y. For simplicity we assume that capital and self-employment hours are perfect complements in production so that $Y_i = \theta_i min(K_i, S_i)$, where θ_i is a measure of individual skills which might differ across individuals. The assumption of perfect complementarity simplifies the analysis and, as discussed below, relaxing it does not change the interpretation of the empirical findings. Given perfect complementarity, in equilibrium $S_i = K_i$ and profits from self-employment for individual i are $\pi_i = p_y\theta_i S_i - p_k S_i = rS_i$, where p_y and p_k are the prices of output and capital, and $r_i = p_y\theta_i - p_k$ is the net return to self-employment hours. This can be individual specific if different individuals have different skills, θ_i.

Finally we allow for the possibility that individuals face asset constraints that restrict the maximum feasible hours of self employment. Feasible self employment hours are constrained by $\overline{S}_i \leq 1$, where $\overline{S_i}$ might be strictly lower than 1 for lack of complementary assets, for instance if credit market imperfections limit the amount of available capital to $\overline{K}_i < 1$. As the program transfers assets and hence potentially relaxes constraints on availability of capital, the analysis focuses on the case where the number of self-employment hours are limited by the lack of capital, although other interpretations are possible.

2.4.2 Optimal Participation Decision, Labor Supply and Occupational Choice.

Individuals choose how many hours to devote to wage labor and self employment to $max_{S_i,L_i} U = u(C_i) + v(R_i)$, subject to the budget constraint $C_i = wL_i + r_i S_i + I_i$, the time constraint $L_i + S_i + R_i = 1$ and the non-negativity and feasibility constraints $0 \leq L_i \; and \; 0 \leq S_i \leq \overline{K_i}$.

The first order conditions for L_i and S_i are, respectively:

$$wu'(wL_i + r_i S_i + I_i) - v'(1 - L_i - S_i) + \alpha = 0 \tag{28}$$

$$r_i u'(wL_i + r_i S_i + I_i) - v'(1 - L_i - S_i) + \beta - \delta = 0 \tag{29}$$

where (α, β, δ) are the Lagrange multipliers for the non-negativity and feasibility constraints on L_i and S_i. When these do not bind, the first order conditions make precise that the optimal level of L_i and S_i are such that the marginal benefit in terms of additional consumption (the first term in (28), (29)) equals the marginal cost in terms of forgone leisure (the second term in (28), (29))

In what follows we describe how the solution depends on the individual specific variables $(I_i, r_i, \overline{K}_i)$, and this yields predictions on the participation decision, total labor supply and occupational choice.

Result 1 Participation, labor supply and occupational choice. *Individuals with sufficiently high exogenous income or sufficiently low return to self-employment stay out of the labor force. Individuals who join the labor force and for whom $r_i \geq w$ will specialize in self-employment if their endowment of capital is not too low, otherwise engage both in self-employment and wage labor. Individuals who join the labor force and for whom $r_i < w$ will specialize in wage labor.*

The analytical proof is provided in the appendix. Intuitively, individuals choose to work if the marginal benefit of doing so in terms of extra income is larger than the marginal cost in terms of forgone leisure. The value of income generated by one hour of work depends on returns (r_i, w) and, due to the assumption of diminishing marginal utility, the utility this generates is a decreasing function of the exogenous level of income I_i. This explains the participation decision. If individuals do choose to work, their time allocation to wage labor and self-employment depend on which activity has the highest returns and whether the asset constraint binds. So, individuals for whom the return to self-employment is higher than the wage and who have sufficient capital, will optimally choose to only engage in self-employment. If their access to capital is limited, they will devote however many hours to self-employment as their endowment of capital allows and top up with labor hours to achieve the utility maximizing level of income.[42] Finally, individuals for whom the return to self-employment is lower than the wage will optimally choose to only engage in wage labor.

The four choices discussed in Result 1 map to the occupational choices observed in the data. Table 1 indeed reports that among the ultra-poor 16% are out of the labor force, 29% are only engaged in self-employment, 28% are only engaged in wage labor

[42]Note that the Leontief assumption generates a linear relationship between the endowment of capital and the hours of self-employment when the capital constraint is binding. Allowing for some substitutability between capital and labor in self-employment would generate different functional forms but as long as capital and labor are complements in production, self-employment hours will be an increasing function of the capital endowment $\overline{K_i}$.

and 27% are engaged in both activities. In line with the theoretical intuition, women out of the labor force are likely to have higher exogenous income. In particular, Table 23 in the Data Appendix shows that women who are out of the labor force at baseline are more likely to have a spouse who is the main income-earner. Moreover, taking average earnings of the male household head as a proxy for I_i, these are 66% higher for poor women who are out of the labor force at baseline compared to women who do work (TK12319 vs. TK7453). Finally, women out of the labor force or solely engaged in self-employment at baseline have higher human capital and household assets, which is in line with the idea that they do not face a binding asset constraint.

Result 1 makes precise the mapping between binding asset constraints and occupational choice. In particular, the 29% of women who devote all their working hours to self-employment must have sufficient assets to do so. The 27% of women who engage in both activities must face a binding asset constraint, otherwise they would specialize in self-employment (wage labor) whenever the return from self-employment is higher (lower) than the market wage.[43] Finally, the 28% of women who are solely engaged in wage labor either face a binding asset constraint at $\overline{K}_i = 0$ or their return to self-employment is lower than the market wage.

2.4.3 The Effect of the Program in Partial Equilibrium.

The program transfers assets (livestock) and provides asset specific training, which, if effective, increases the return to self-employment. The following illustrates how the two program components affect labor force participation, hours worked and the time

[43]The result derives from the assumption of constant marginal returns. The same result would be obtained if the marginal return to one or both occupation were increasing. In our setting this could occur because of fixed costs such as travel time to the fields or to gather livestock fodder. Diversification, namely engaging in both occupations, can only be optimal if the marginal return is strictly decreasing for at least one occupation. In our setting, this is unlikely to occur because unskilled labor is abundant so that individuals are price takers in the labor market, and the product market for self-employment output (e.g. milk from livestock) is competitive so that changes in individual supply do not affect price in that market either. Moreover, diminishing returns cannot explain the fact that 60% of the treated poor are at one of the two corner solutions at baseline, unless one is willing to assume that similar individuals have access to different technologies for self-employment, so that some of them face decreasing returns and others do not. In a more general model that allows for uncertainty and risk, diversification can also be a means to reduce uncertainty if the two occupations have different risk profiles, and individuals have different preferences for risk. As the risk profile of the two occupations is the same for all individuals, the observed baseline pattern can be explained only by very different risk preferences among otherwise similar individuals. This explanation, however, is at odds with the average program effects on occupational choice. As discussed below, 28% of treated individuals who solely engaged in wage labor at baseline devote some time to self-employment at follow-up. To be consistent with the risk smoothing explanation, we would need the program to change either the risk preferences of these individuals or dramatically lower the uncertainty associated with self-employment. Neither seems plausible in our context. In particular, as discussed above, the assets transferred by the program and their production technology was very similar to existing self-employment opportunities.

allocation between wage labor and self employment in partial equilibrium, namely without taking into account the effect that changes in occupational choice can have on the wage and the return to self employment. The analysis makes precise that : (i) the two program components have opposite effects on most occupational choice variables (ii) the effect of both components depends on whether the asset constraint was binding.

The Effect of the Asset Transfer Component

The program transfers assets (livestock) that are identical to those available locally at baseline, so can be sold at the same market price p_k. In terms of the model the effect of the asset transfer depends on whether the asset constraint was binding at baseline. If it was not, the transfer is equivalent to an increase in non-labor income I_i. Namely, if treated individuals already owned the optimal quantity of assets, their optimal response will be to sell or rent out the asset, which will increase I_i. We note that in a more general model with several time periods, these individuals might want to retain the asset in the short run if, for instance, selling it quickly would damage their relationship with BRAC. This however would not preclude them from renting it out or hiring labor to tend to it, which would have the same effect on I_i and occupational choice. We also note that the asset transfer can affect I_i directly, for instance by affecting the husbands' labor supply. The predictions below are derived for the case in which the net effect on I_i is positive, namely the asset transfer does not reduce the total non-labor income available to the individual. In line with this, the husbands' labor supply does not decrease following the implementation of the program.

If the asset constraint was binding, namely if at baseline individuals were willing but unable to purchase more assets at the price p_k, the asset transfer will relax the constraint by increasing $\overline{K}_i$ [44].

Whether asset transfer increases I_i or $\overline{K}_i$ has radically different implications for the program's effects on the economic lives of the poor as summarized in Result 3 below.

Result 2. Asset transfer effect. *The asset transfer weakly reduces participation, hours devoted to self-employment, and hours devoted to wage labor for individuals who did not face biding asset constraints at baseline. It does not affect participation but increases self-employment hours and reduces labor hours for individuals who faced biding asset constraints at baseline.*

Intuitively the asset transfer acts as an increase in I_i for individuals who did not face biding asset constraints at baseline. This has no effect for individuals who were already out of the labor force, but could push some of those who were working at baseline past

[44]This follows from the fact that the unit value of the transferred asset is p_k. If it were higher, even individuals constrained at baseline might prefer to sell it rather than use it for self-employment.

the non-participation threshold. For those who keep working and are not constrained, an increase in I_i reduces the marginal benefit of income because of diminishing marginal utility, thus reducing hours devoted to self-employment, labor and total hours worked. This can be easily seen from (28), (29), which can hold only if an increase in I_i is balanced by a fall in L_i (when $S_i = 0$) or S_i (when $L_i = 0$).

For individuals who faced a binding asset constraint at baseline, the asset transfer relaxes the constraint thus increasing hours devoted to self-employment. As a consequence, hours devoted to wage labor will fall, as constrained individuals only engage in wage labor to top up the earnings from self-employment, which are below first best due to the binding asset constraint. Since the asset constraint can only bind in equilibrium if $r_i > w$, the reduction in labor hours must be larger than the increase in self-employment hours to maintain equality between the marginal utility of income and the marginal utility of leisure (28). Thus total hours worked must fall.

The Effect of the Training Component.

The training component, if effective, increases r by increasing θ, namely by improving self-employment skills. Note that although the training program is asset specific, since the transferred assets (livestock) are identical to those owned by the treated individuals at baseline, the training potentially increases returns to self-employment both for the individuals who decide to retain the asset and for those who do not.

For all individuals, an increase in r makes self-employment more attractive than wage labor and thus should increase hours devoted to self-employment, other things equal. For individuals already engaged in self-employment at baseline, the increase in r has an additional income effect that goes in the opposite direction. For a given number of hours of self-employment, income increases when r increases. As seen in the argument of u' in both (28) and (29), this has the same effect as an increase in I_i, that is it reduces the marginal benefit of income because of diminishing marginal utility and it reduces hours worked. Summarizing, we have:

Result 3. Training effect. *Training weakly increases participation and hours devoted to self-employment, and weakly decrease hours devoted to wage labor for individuals who did not face biding asset constraints at baseline as long as the substitution effect dominates. It leaves self-employment hours unchanged and weakly reduces labor hours for individuals who face binding asset constraints.*

For individuals out of the labor force, participation might increase because as r increases the opportunity cost of leisure increases, thus the threshold level of I above which individuals prefer not to work increases. Unconstrained individuals who previously specialized in self-employment increase the hours devoted to self-employment

as long as the substitution effect prevails. Unconstrained individuals who previously specialized in wage labor are unaffected if their return to self-employment remains lower than the market wage or switch occupations and specialize in self-employment (thus reducing labor hours to zero) if, as a consequence of training, the return to self-employment exceeds the market wage. Finally, constrained individuals cannot increase self-employment hours past $\overline{K}_i$, hence the substitution effect is muted and labor hours fall because of the income effect as long as $\overline{K}_i > 0$.

The Effect of the Program in Partial Equilibrium: Predictions

Results 2 and 3 make precise that (i) the asset transfer and the training component can push in opposite directions and (ii) the effect of both components on the economic lives of the poor depend on whether these face binding asset constraints. Without further information on the share of treated households for which these constraints bind, the average effect of the program on treated households can be summarized as follows.

Prediction 1. Average program effect. *The program weakly decreases labor hours and has ambiguous effects on labor force participation, and hours devoted to self-employment.*

Prediction 1 captures the fact that since the transfer and the training generally push in different directions, the impact of the program is generally ambiguous. The only unambiguous prediction is that labor hours fall both because the program makes individuals richer and because it makes wage employment less attractive. In other words, the program unambiguously discourages treated individuals from engaging in wage labor. Whether it encourages them to engage in self-employment and start on an entrepreneurship path out of poverty is an empirical issue.

More precise predictions can be derived making use of the fact that the effect of both programme components depends on the individuals' occupational choice at baseline, which maps onto whether individuals face a binding asset constraint. In particular, individuals who are out of the labor force or solely engaged in self-employment at baseline do not face a binding constraint; individuals who are engaged in both occupations do; and individuals who are solely engaged in wage labor might. The program effect by baseline occupational choices can be summarized as follows.

Prediction 2. Heterogeneous program effect. *(i) For individuals out of the labor force: the effect on self-employment hours is weakly positive, the effect on labor hours is zero. (ii) For individuals solely engaged in self-employment: the effect on self-employment hours is ambiguous; the effect on labor hours is zero. (iii) For individuals engaged in both occupations: the effect on self-employment hours is positive, the effect on labor hours negative. (iv) For individuals solely engaged in wage labor: the effect on*

self-employment hours is weakly positive, the effect on labor hours negative.

Prediction 2 makes precise that the average program effect hides a substantial amount of heterogeneity among individuals who made different occupational choices at baseline. This will guide our empirical strategy in Section 2.5.

2.4.4 The Effect of the Program in General Equilibrium

The program affects the hours that the treated poor devote to wage labor and self-employment, and brings new livestock assets in these communities. The program thus generates an exogenous shock to the supply of unskilled labor, self-employment output (e.g. milk, eggs) and the stock of livestock assets in the community. These can have general equilibrium effects through changes in wages (w), output prices (p_y) and livestock prices (p_k), which affect both treated and non-treated households in the community.

As the program unambiguously reduces the supply of wage labor hours by treated women, the general equilibrium effect on wages is weakly positive. Similarly, as the program unambiguously increases the endowment of livestock assets, the general equilibrium effect on livestock prices is weakly negative. Finally, as long as some of the assets are kept in the community, the supply of livestock products increases (e.g. milk) and their price falls. Taken together these imply that the general equilibrium effect on the return to self-employment ($r = p_y\theta - p_k$) is ambiguous. Summarizing we have:

Prediction 3. General equilibrium effects. *The program weakly increases unskilled wages, weakly decreases livestock and product prices and has an ambiguous effect on self-employment returns.*

The strength of these effects varies depending on the magnitude of the treatment compared to the size of the local economy and on the level of integration between local and regional markets. The program directly affects the labor supply of 45% of potential unskilled female workers on average (as middle and upper class women do not engage in unskilled wage labor) and local labor markets for unskilled female labor are likely to be isolated as poor women mostly work as domestic servants or casual agricultural laborers in the local community.

In comparison, the program affects the hours devoted to self-employment by 10% of the local women engaged in this activity and local product markets (e.g. milk) are well integrated with regional markets. Finally the average community receives 7.5% more cows and 32% more goats than it had at baseline, and more become available after one year when a share of these has produced offsprings. As transport costs are much higher for large livestock assets than for output, at least a share of these are likely to remain within the local community.

If the program indeed affects prices as suggested above, this will have further ef-

fects on the participation, labor supply and occupational choices of both treated and non-treated households in the community. Comparative statics with respect to self-employment returns are already derived in Result 2, those with respect to wage changes are symmetrical and summarized in Prediction 4 below.

Prediction 4. Spillover effects through increases in wages. *Higher wages weakly increases participation and hours devoted to wage labor, and weakly decrease hours devoted to self-employment as long as the substitution effect dominates.*

2.5 The Effect of the Program on the Treated

2.5.1 Occupational Choice

Average Effects

To evaluate the effect of the program on the treated ultrapoor, we estimate the following difference in difference specification:

$$(y_{i1} - y_{i0}) = \alpha + \beta T_i + \eta_d + \epsilon_{id}, \tag{30}$$

where $(y_{i1} - y_{i0})$ is the difference in outcome of interest for individual i between followup and baseline, $T_i = 1$ if individual i lives in a treated community and 0 otherwise and η_d are subdistrict fixed effects. We estimate (30) on the entire sample of selected ultrapoor individuals, hence β identifies the intent to treat, which in this context coincides with the average treatment on the treated as all selected individuals accepted to participate.

The effect of the program is thus identified by comparing changes in outcomes within the same individual before and after the program in treatment communities to the same changes in control communities within the same subdistrict. We thus control for all time-varying factors common to individuals in treatment and control communities, and for all time-invariant individual heterogeneity. While randomization ensures that individual heterogeneity is orthogonal to treatment in expectation, random differences in individual characteristics at baseline can nevertheless contaminate cross-sectional estimates.

The coefficient β identifies the causal effect of the program on the treated under the assumption that the underlying trends in the outcomes of interest are the same for individuals in treatment and control communities within the same subdistrict

Standard errors are clustered at the community level throughout to account for the fact that outcomes are unlikely to be independently distributed within the same community. As discussed above, treatment is randomized at the level of the BRAC branch office to minimize the risk of contamination among communities served by the same

office. Results are generally robust to clustering by BRAC branch office area but this is less appropriate than village level clustering because the geographical coverage of a single office reflects BRAC's capacity rather than any feature common to all communities in the area.

Following the theoretical framework, we begin by identifying the impact of the program on occupational choices. Figure 5 presents a graphic illustration of the striking change in the occupational structure of the ultrapoor in treated communities relative to their counterparts in control communities. At baseline, the distribution across activities was similar in treatment and control communities: 28% in wage-work only, 27% in both wage and self-employment, 29% in self-employment only and 17% out of the labor force. At followup, all the women in treated communities were in the labor force, and almost all of them were engaged in self-employment, whereas women in control communities experienced no noticeable change relative to baseline.

Table 16 estimates (30) on occupational choices. Column (1) shows that the difference-in-difference estimate for time devoted to self-employment is 557 hours, that is a 135% increase relative to baseline. Column (2) shows hours devoted to wage-employment fall by 80 – a 11% decrease relative to baseline. Column (3) shows that the labor force participation of targeted women increases as a result of the program – treated women are 13 percentage points more likely to be working in an income-generating activity after the program relative to the control group. Finally, column (4) shows that the effects on the occupational choices of treated women correspond to an increase in their earnings from income-generating activities by 1755 TK, that is a 36% increase relative to baseline.

Taken together the results on occupational choice indicate that, on average, the effect of the training component and/or the effect of relaxing binding asset constraints prevail over the wealth effect of the asset transfer, as treated women work more rather than less. In line with this, we find that the ultrapoor retained the assets instead of liquidating them. Table 17 shows that the average treated household has more livestock, in particular they have one more cow, .75 more chicken and 2.6 more goats on average. Total livestock value has increased by 11,306 TK - which corresponds to an additional 19% increase over and above the value of average asset transferred by the program (9,500TK). The difference is significant at conventional levels (test of equality of the coefficient to 9,500 is rejected at 99% confidence level).[45] This additional increase might be due to the production of offsprings or the purchase of additional assets, an issue we

[45]We cannot say whether these are exactly the same animals they were given at the beginning of the program or whether they have been replaced with others. What is key for the interpretation of the results is that two years later the treated poors hold livestock assets of higher value than those they received, which rules out the possibility that they liquidated them to increase short-run consumption.

will discuss further in the concluding section.

Heterogeneous Effects

To shed light on whether the impact of the program on occupational choice is due to the fact that the training increased self-employment returns or to the fact that the asset transfer relaxed previously binding asset constraints we allow the effect of the program to depend on baseline occupational choices. As the theoretical framework makes precise, individuals who were out of the labor force or solely engaged in self-employment must have been unconstrained at baseline. For these individuals, the program can increase self-employment hours only if it increases the returns to self-employment, and if this dominates the wealth effect of the transfer. In contrast, individuals who were engaged in both occupations must have been constrained at baseline. For these individuals, the program can increase self-employment hours both by relaxing the asset constraint and by increasing self-employment returns. Finally, individuals exclusively engaged in wage labor might have been extremely constrained (with zero assets) or unconstrained with a low rate of return in self-employment. For these individuals, the program can increase self-employment hours by relaxing the asset constraint (if relevant) or by increasing the self-employment returns over the wage rate.

Table 18 test for heterogenous effects of the program by baseline occupational choices of treated individuals. We estimate the following specification

$$(y_{i1} - y_{i0}) = \sum_{j=1}^{4} \gamma^j B_{i0}^j + \sum_{j=1}^{4} \delta^j T_i B_{i0}^j + \eta_d + \varepsilon_{id} \tag{31}$$

where y_{it}, T_i and R_t are the same as in (30) before and B_{i0}^j are indicator variables for the occupational choice of individual i at baseline. As described above, there are four categories of occupational choice: engaged in both wage and self-employment, only wage, only self-employment or out of the labor force. The coefficients of interest are the δ^j, that is the impact of the program on the four categories of individuals. For brevity, Table 18 reports the estimates of δ^j only.

The results show that individuals in all 4 groups spend significantly more hours in self-employment. The effect is largest for those who spent no time in self-employment at baseline (either out of the labor force or only engaged in wage labor, 45% of the treated group) and smallest for those exclusively engaged in self-employment at baseline. The findings have two implications. First, the program radically transforms the lives of the ultra-poor, who end up spending as much time in self-employment as women in the top two social classes. This applies both to women who were previously devoting some time to self-employment but perhaps more remarkably to those who were not. Reassuringly,

the magnitude of the increase is in line with BRAC's expectations of time needed to tend to the combinations of assets offered by the program.

Second, the fact that time devoted to self-employment increases for women who did not face asset constraints at baseline indicate that the training component of the program successfully increased the returns to self-employment. In line with this, the difference in difference estimate of the income per hour for those only engaged in self-employment indicates that this increased by 34%.

Column (2) presents results on the heterogenous effects of the program on hours spent on wage employment. Consistent with Prediction 2, individuals who were engaged in wage-employment only and those engaged in both occupations decrease the number of hours they spend in wage work, by 228 and 152 hours respectively. The theoretical model also predicts that, in partial equilibrium, the program should have no effect on the hours spent in wage-employment by individuals who were specialized in self-employment at baseline, or for those who were out of the labor force. Column (2) shows that indeed the program has no effect on the hours spent in wage-employment by individuals previously specialized in self-employment, but it reduces hours dedicated to wage-employment by 96 for individuals out of the labor force at baseline. This can be reconciled with the theoretical predictions by noting that the difference in difference estimate captures the fact that individuals out of the labor force at baseline increase labor hours at followup in control communities. Column (3) shows an increase in labor force participation compared to their counterparts in control communities for all individuals, especially those who were out of the labor force at baseline.

Finally, Column (4) shows that the program leads to an increase in total earnings from income-generating activities for individuals in all groups, and that this is significantly larger for individuals who were either out of the labor force or solely engaged in self-employment at baseline. Since these individuals were better-off on several dimensions as shown in table 23, the findings suggest the program is more effective at generating earnings for the least poor among these very poor individuals.

To summarize, the results on the heterogenous effects of the program by the baseline occupational choices of treated individuals, are consistent with the theoretical predictions for the case in which the increase in self-employment returns due to training dominates the wealth effect due to the asset transfer. Indeed, the program increases hours dedicated to self-employment and participation both for individuals who might have been asset constrained at baseline and for those who were not. The model makes precise that the latter would increase hours worked only if the substitution effect due to an increase in returns dominate the wealth effect due to the transfer.

2.5.2 Welfare

Before delving into general equilibrium and spillover effects, we document the effect of the program on the welfare of the treated. Table 19 provides evidence that the change in occupational structure and resulting increase in income correspond to significant welfare improvements for the ultrapoor households. We begin by analyzing food security, which is the main welfare target of the program. Households are defined to have food security if members can afford two meals per day on most days. Table 19 shows that this measure of food security increases by 0.15 points as a result of the program, corresponding to a 42% increase relative to its baseline level (0.41). Per-capita food expenditure increased by 151 TK (5% relative to its baseline level) and the price per calorie increased by 3%, suggesting that food quality improved as a result of the program. Annual per capita expenditure on non-food items increased by 231 TK (22% relative to baseline). Finally, total annual per capita expenditure increased by 370TK (9% relative to baseline). Given that the average household has 3.3 members, these figures imply that the average households consumes 2/3 of the additional income generated by the program. This suggests that the program also achieves its stated goal of encouraging saving behavior among these households.

2.6 General Equilibrium Effects and Spillovers

2.6.1 General Equilibrium Effects

Table 20 explores the general equilibrium effects of the program. On average we survey 18 households in each community, which represent about 20% of a community of average size. We are thus able to compute prices at the community level by taking means from our individual survey data. We calculate average unskilled wages, the average prices of assets and program-relevant products (such as milk and eggs), as well as the average returns to self-employment. Of these, the wage figures are likely to be less noisy as we survey all the households (STUPs and other poors) who engage in unskilled labor.

We first evaluate the effects of the program on the wages of women and men in unskilled occupations. We find that the unskilled wage for women increases by 10% in treated communities relative to control communities and this effect is estimated precisely at conventional levels. In contrast, the increase in men's wages is small (2%) and not significantly different from zero. This is consistent with the fact that the program targets women, and that we find a significant drop in the total labor supply of women but not of men at the community level. The fact that a drop in the labor supply of women does not effect men's wages indicates that the labor market for unskilled labor

is segmented by gender. This is in line with findings of Foster and Rosenzweig (1996) in rural India, where they show that male and female laborers in specialize in different agricultural tasks, according to comparative advantage.

Columns (3) to (5) evaluate the effect of the program on the prices of livestock. To do so, we calculate the mean resale unit values of cows, goats and poultry at the community level. We find that the average value of a goat falls by 9% in treatment communities relative to control communities, while the average value of cows and poultry are not significantly affected. This is consistent with the fact that goats are much rarer in these communities before the implementation of the program. In particular, the size of the asset transfer compared to baseline stocks is 7.5% for cows, 1% for poultry and 32% for goats.

Columns (6) and (7) evaluate the effect of the program on the prices of the main livestock produce- milk and eggs. While the difference-in-difference estimates are negative for both goods neither is precisely estimated at conventional levels. This is consistent with the fact that the total size of the asset transfer, and hence the additional output generated by the program, is small compared to existing stocks. It might also reflect the fact that the local product markets are well-integrated with markets outside the community.

Finally, columns (8) and (9) show that the average returns to self-employment, measured as total annual earnings from self-employment divided by hours worked, remains constant on average.

2.6.2 Spillover Effects on Other Poor

The changes in wages and prices documented above might affect the occupational choices of other households in the community, as described in Prediction 4. To shed light on this, Table 21 reports the estimate of equation (30) for the occupational choice and labor supply of the main female respondent in other poor households. The coefficient estimate corresponds to the difference-in-difference estimate for the change in the outcomes of interests among other poor households in treated communities, relative to the change of other poor households in control communities. The findings indicate that the average non-treated poor woman devotes 51 more hours to wage employment, a 12% increase relative to baseline and 30 more hours to self-employment, a 6% increase, but only the former is significantly different from zero at conventional levels. The table also shows that other poor women are 4 percentage points more likely to be participating in the labor market (relative to a baseline of 81pp) and their total earnings increase by TK479, a 11% increase relative to baseline.

The finding that other poor women devote more time to wage labor following an

increase in wages is consistent with the theoretical predictions. The fact that they also devote more time to self-employment is however at odds with our previous finding that self-employment returns are unchanged. The two findings can however be reconciled if our estimates of returns, which are based on self-reported annual earnings, do not account for the fall in asset prices and the increased availability, and presumably lower prices, of asset rentals. While we do not have data on rental prices, we find that the probability of other poor women renting livestock increases by 5 percentage points, corresponding to a 20% increase relative to baseline.

Finally, Table 25 in the Data Appendix presents results on the spillover effects of the program on male heads from other poor households. In line with the earlier finding that male wages are not affected by the program, we find no changes in their labor force participation and occupational choice.

2.7 Conclusion

The question of what keeps people mired in poverty is one of the oldest in economics. What we do know is that the world's poor typically lack both capital and skills and different strands of the academic and policy literatures have emphasized inadequate capital or inadequate skills as the root cause of poverty. Capital and skill shortages are also reflected in the poor being employed in low return and often insecure occupations.

A fundamental question then is whether transfers of capital and skills aimed at enabling the poor to operate their own businesses will allow them to permanently exit poverty. This is akin to asking whether one can create successful entrepreneurs – who acquire skills and make productive use of capital – out of poor people who started out without either. Key to this question is whether these transfers allow the poor to alter their occupational and production choices so that they come to resemble non-poor people in their communities, as opposed to merely increasing consumption in the short run. And the question becomes more salient as the world is littered with numerous examples of anti-poverty programs which, despite their best intentions, failed to have any appreciable impact on their intended beneficiaries.

We provide evidence on the matter from an innovative entrepreneurship program in Bangladesh that targets the poorest women in rural communities and transfers them assets and skills to run their own businesses. A simple theoretical framework makes precise that the program succeeds in its stated aim of transforming occupational structure if the poor face binding asset constraints at baseline and/or if the effect of increasing self-employment returns through training dominates the wealth effect of the large capital transfer.

Our findings are striking. Two years after the program, treated women have higher labor force participation, and they allocate more time to self-employment and less to wage-labor. This change in their labor supply and occupational choice corresponds to significant welfare improvements for the treated poor households. More specifically, they have higher income, higher per-capita expenditure, and improved food security.

The program has significant general equilibrium effects on the female labor market in the treated communities – the wage rate for unskilled female labor is higher, and asset prices (goats) are lower. Correspondingly, there are spillover effects on the occupational choices of non-treated poor women: they increase their labor-force participation, spending more time in both wage-labor (in line with higher wages) and self-employment (in line with lower asset prices).

Our results have important policy implications. First, our results imply that constraints on entering into self-employment are driving occupational choices of poor women in rural Bangladesh. This suggests that programs – such as the ultrapoor program – that improve self-employment opportunities of very poor households can lead to significant welfare gains. Second, our findings imply that providing skills as well as capital is important in ensuring effectiveness of such programs and has an effect over that of asset transfer alone. Third, we contribute to a growing body of literature that finds large spillover effects of large-scale welfare programs – such as conditional cash transfer programs or the ultrapoor program. Moreover, we show that these effects are likely to be heterogenous, depending on the underlying market structures – in this case, due to the presence of highly segmented labor markets along gender dimensions, we observe general equilibrium and spillover effects on the female, but not on the male labor market. It is important to take into account these spillover effects while analyzing the cost-effectiveness of welfare programs.

Taken together, our results suggest that the program benefits exceed those that would accrue from an unconditional cash transfer equal in cost to the program. The program costs 20,700 TKs (roughly 300 US$) per household per annum. We find that as a result of the program, the yearly income of the female ultrapoor respondent increases by 1755 TKs, which corresponds approximately to 10% of the initial cost of the treatment.[46] An equivalent cash transfer at the going interest rates (6%) would have yielded 1242 TKs per year.[47] Moreover the program also benefits other poor women via an increase in wages. To quantify the total benefit (direct benefits on beneficiaries and the indirect benefits on the non-treated poor households), we consider the average

[46]The long-run benefit might be higher as the animals produce offsprings.

[47]We implicitly assume that livestock assets and cash are equally long-lived. While this is not literally true, livestock produces offsprings, thus although the life of a given animal is finite, reproduction ensures a replacement.

community which has 84 households, out of which 5 are ultrapoor households and 6 are "other poor" households. We find that the total income effect on the ultrapoor households are 5×1755=8775 TKs, and for other poor it is 6×479=2874 TKs. An equivalent cash transfer at the community level would have yielded 6210 TKs at the going interest rates – only 70% of the income effect on the beneficiaries alone, and 53% of the total effect once we take the spillovers into account.[48] The welfare effects of this entrepreneurship program on the poor therefore are large relative to those that would be predicted based on cash transfer of equal cost which suggests that the program is having a transformative effect on the economic lives of the poor in these rural communities.[49] This lines up with effects we observe on labor supply and occupational choice in the data. Moreover the spillover effects are also sizable which implies that ignoring these effects (as is standard in evaluations that consider only the effect on the treated) would underestimate benefits considerably.

We are in the process of gathering 2011 data on this program. This will allow us to look at the longer term effects of the program and in particular at whether the treated ultrapoor are on a stable path out of poverty. One gauge of this will be whether they have diversified their business activities outside of those for which they received assets and training from the BRAC ultrapoor program. Table 22 shows some preliminarily evidence on this issue. In it we see evidence that treated ultrapoor households have started to invest in other, non-program productive assets. The average treated poor household is 2 percentage points more likely to own land (compared to 6% at baseline) and 8 percentage points more likely to rent in land (compared to 6% at baseline). The average targeted ultrapoor household is also significantly more likely to own a shop. These are activities which the middle classes in these rural communities engage in. This is therefore preliminary evidence that ultrapoor in these communities are graduating into higher return economic activities outside of those which they receive assistance for within the BRAC program. The fact that this is happening just two years after the treatment suggests that the treated ultrapoor have taken a significant step up the ladder out of poverty.

[48]The cost-benefit analysis does not take into account non-monetary costs and benefits, such as, for instance, increased self-confidence and empowerment of the treated women or jealousy and resentment of the non-treated, as these cannot be quantified.

[49]The cost-benefit analysis focusses on benefits to the poor as opposed to the community as a whole. The key difference is that the increase in wage represents a benefit for the poor but a cost for their employers. To the extent that employers live in the community the aggregate welfare gain due to spillovers is lower. In our sample 48% are hired by employers residing in the same community, thus the increase in welfare due to wage increases partially represents a redistribution from employers in other areas.

FIGURE 4: EVALUATION DESIGN

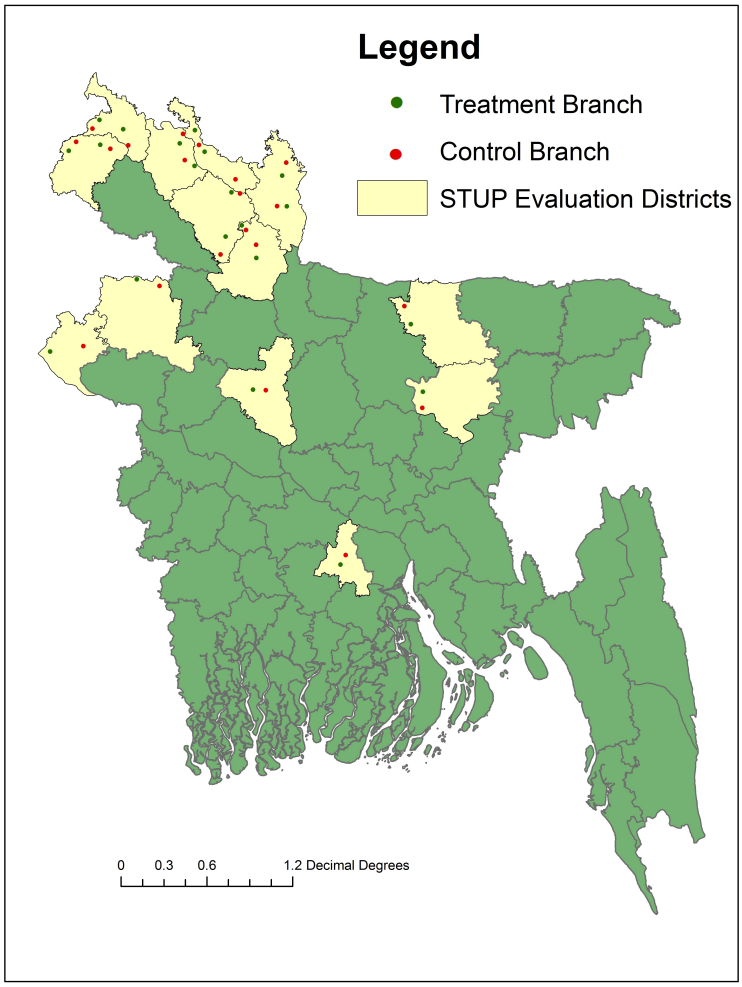

FIGURE 5: OCCUPATIONAL CHOICE OF ULTRA POOR WOMEN AT BASELINE AND FOLLOWUP, BY TREATMENT STATUS

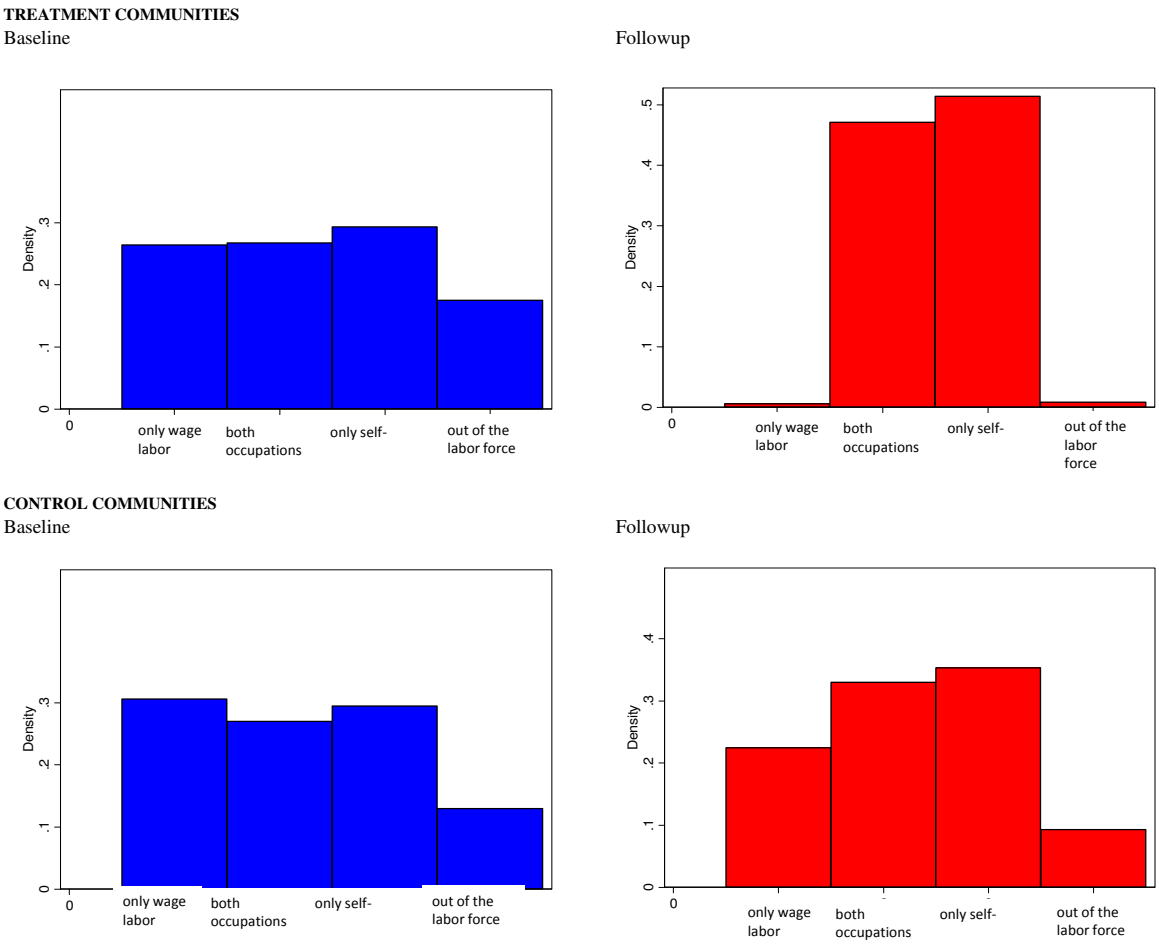

Table 15: Lives of The Ultra Poor at Baseline

	(1) Ultra Poor	(2) Other poor	(3) Middle class	(4) Upper class
Number of households	6817	8576	7241	2428
Household head male	0.58	0.79	0.94	0.95
	(0.49)	(0.41)	(0.23)	(0.22)
Household size	3.26	3.70	4.43	5.03
	(1.69)	(1.65)	(1.66)	(2.02)
Female respondent literate	0.07	0.16	0.27	0.52
	(0.26)	(0.37)	(0.44)	(0.50)
Female respondent BMI	18.36	18.87	19.33	20.27
	(2.24)	(2.37)	(2.46)	(2.90)
Food security	0.41	0.53	0.81	0.96
	(0.49)	(0.50)	(0.40)	(0.19)
Total PCE	3,960.1	4,247.1	5,563.8	11,973.3
	(2,267.9)	(2,990.0)	(5,278.6)	(34,484.8)
Wealth	5,620.9	13,991.2	153,359.5	853,426.6
	(29,931.2)	(69,828.1)	(325,057.5)	(971,623.6)
Livestock value	870.2	2,553.3	12,879.7	31,304.6
	(3,207.7)	(6,786.0)	(26,172.3)	(39,186.4)
Durables value	429.1	713.0	2,263.5	7,862.0
	(509.7)	(1,005.2)	(3,252.6)	(8,900.4)
Savings	142.2	389.9	1,618.0	9,297.1
	(804.5)	(1,291.5)	(10,563.5)	(31,883.9)
Panel B: Occupational Choices at Baseline				
Hours spent in:				
Wage employment	723.1	435.4	110.9	42.6
	(847.5)	(712.6)	(398.4)	(279.1)
Self employment	413.5	502.9	700.5	769.5
	(581.0)	(575.5)	(559.3)	(512.9)
All income generating activities	1,136.5	938.3	811.4	812.1
	(886.3)	(827.3)	(643.1)	(554.3)
Occupation at baseline (% of respondents):				
Wage employment only	28.2	14.6	2.5	0.7
Both self-employment and wage labor	26.8	21.9	7.2	2.1
Self-employment only	29.4	44.3	76.2	87.1
Out of the labor force	15.6	19.2	14.0	10.1

Notes: "Ultra Poor" identifies the households selected to receive the program. "Other poor" are households ranked in the bottom category in the participatory wealth ranking exercise, but who are not selected to receive the program. "Middle" and "Upper" classes are households ranked in the middle and top categories, respectively. Total PCE includes food and non-food expenditure over the previous year. Wealth includes all assets, such as land, livestock, homestead and durables.

Table 16: Average Treatment Effect on Occupational Choice

	(1) Hours spent in self-employment	(2) Hours spent in wage employment	(3) Labor force participation	(4) Total income
Treatment	557.19***	-80.34***	0.13***	1755.8***
	(22.59)	(25.81)	(0.01)	(245.65)
N	6817	6817	6817	6817
Adj. R-squared	0.18	0.03	0.04	0.04

Notes: OLS estimates, standard errors clustered at the community level in parenthesis. (***) (**) (*) indicate the null hypothesis of coefficient equal zero can be rejected at the (1%) (5%) (10%) level. Hours are computed over the past year. Total income includes income from all income generating activities, for all household members if the activity involves more than one member. All regressions include subdistrict fixed effects.

Table 17: Average Treatment Effect on Livestock Assets

	(1) Number of cows	(2) Number of poultry	(3) Number of goats	(4) Livestock value
Treatment	1.22***	0.75***	2.57***	11306.49***
	(0.02)	(0.04)	(0.16)	(230.29)
N	6817	6817	6817	6817
Adj. R-squared	0.47	0.12	0.08	0.31

Notes: OLS estimates, standard errors clustered at the community level in parenthesis. (***) (**) (*) indicate the hypothesis of coefficient equal zero can be rejected at the (1%) (5%) (10%) level. All regressions include subdistrict fixed effects.

Table 18: Heterogenous Treatment Effects by Baseline Occupational Choices

	(1) Hours spent in self-employment	(2) Hours spent in wage employment	(3) Labor force participation	(4) Total income
Treatment effect on those who were:				
in Wage-employment only	660.64***	-228.23***	0.04***	987.11***
	(29.97)	(52.18)	(0.007)	(404.76)
in Both occupations	518.81***	-151.75***	0.03***	1528.91***
	(32.45)	(47.85)	(0.006)	(475.31)
in Self-employment only	470.06***	-39.14	0.10***	2077.49***
	(43.79)	(24.80)	(0.01)	(351.13)
Out of the labor force	618.19***	-97.67**	0.24***	1875.65***
	(34.88)	(38.43)	(0.027)	(413.65)
N	6817	6817	6817	6817
Adj. R-squared	0.30	0.20	0.75	0.06

Notes: OLS estimates, standard errors clustered at the community level in parenthesis. (***) (**) (*) indicate the hypothesis of coefficient equal zero can be rejected at the (1%) (5%) (10%) level. All regressions include subdistrict fixed effects.

Table 19: Average Treatment Effects on Welfare

	(1) Food security	(2) PCE food	(3) Price per calorie	(4) PCE non-food	(5) Total PCE
Treatment	0.15***	150.72***	0.03**	231.49***	369.38***
	(0.03)	(57.82)	(0.01)	(61.84)	(93.63)
N	6817	6295	6294	6500	6295
Adj. R-squared	0.14	0.03	0.04	0.02	0.02

Notes: OLS estimates, standard errors clustered at the community level in parenthesis. (***) (**) (*) indicate the hypothesis of coefficient equal zero can be rejected at the (1%) (5%) (10%) level. All regressions include subdistrict fixed effects. Food security equals 1 if the household could afford two meals per day most of the time over the previous year, 0 otherwise. Per capita food expenditure is imputed at the yearly level on the basis of reported food expenditure in the last three days. Price per calorie is computed as the ratio of total food expenditure over total calories purchased. Per capita non-food expenditure includes all expenditures other than food over the previous year.

Table 20: General Equilibrium Effects on Prices

	Log(wages)		Asset prices			Product prices		Log (return to SE)	
	Women	Men	Cows	Poultry	Goats	Milk	Eggs	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment	0.10***	0.01	0.10	0.04	-0.09***	-0.02	-0.02	0.07	0.05
	(0.03)	(0.02)	(0.07)	(0.09)	(0.02)	(0.02)	(0.03)	(0.05)	(0.03)
N	1288	1380	1402	1291	1406	1224	1238	1409	1400
Adj. R-squared	0.08	0.05	0.05	0.05	0.16	0.08	0.10	0.12	0.09

Notes: OLS estimates, standard errors in parenthesis.(***) (**) (*) indicate the hypothesis of coefficient equal zero can be rejected at the (1%) (5%) (10%) level. All regressions include subdistrict fixed effects. Mean wages are computed as the ratio of wage labor earnings to wage labor hours. Average asset prices are computed as the mean of unit values (value divided by number of assets). Average food prices are per 100g .Mean returns to self-employment are computed as the ratio of self-employment earnings to self-employment hours.

Table 21: Average Treatment Effect on Non-Treated Poor Women

	(1) Hours spent in self-employment	(2) Hours spent in wage employment	(3) Labor force participation	(4) Total income
Treatment	30.32	51.36***	0.04***	478.68**
	(20.99)	(18.37)	(0.01)	(204.36)
N	8576	8576	8576	8576
Adj. R-squared	0.03	0.03	0.03	0.03

Notes: Sample is restricted to main female respondents in other (non-selected) poor households. OLS estimates, standard errors clustered at the community level in parenthesis.(***) (**) (*) indicate the hypothesis of coefficient equal zero can be rejected at the (1%) (5%) (10%) level. All regressions include subdistrict fixed effects. Hours are computed over the past year. Total income includes income from all income generating activities, for all household members if the activity involves more than one member.

Table 22: Average Treatment Effect on Other Business Activities

	(1) =1 if owns land	(2) =1 if rents land	(3) Number of shops
Treatment	0.015***	0.08***	0.01*
	(0.006)	(0.01)	(0.005)
N	6817	6817	6817
R2	0.01	0.04	0.00

Notes: OLS estimates, standard errors clustered at the community level in parenthesis. (***) (**) (*) indicate the coefficient is different than zero at the (1%) (5%) (10%) level. All regressions include subdistrict fixed effects.

2.A Appendix

2.A.1 Proofs

Proof of Result 1 Participation, labor supply and occupational choice. *Individuals with sufficiently high exogenous income or sufficiently low return to self-employment stay out of the labor force. Individuals who join the labor force and for whom $r_i \geq w$ will specialize in self-employment if their endowment of capital is not too low, otherwise engage both in self-employment and wage labor. Individuals who join the labor force and for whom $r_i < w$ will specialize in wage labor.*

The Lagrangian is

$$L = u(wL_i + r_iS_i + I_i) + v(1 - L_i - S_i) + \alpha L_i + \beta S_i + \delta(\overline{K_i} - S_i) \tag{32}$$

taking the derivative with respect to L_i and S_i yields the first order conditions:

$$wu'(wL_i + r_iS_i + I_i) - v'(1 - L_i - S_i) + \alpha = 0 \tag{33}$$

$$r_iu'(wL_i + r_iS_i + I_i) - v'(1 - L_i - S_i) + \beta - \delta = 0 \tag{34}$$

There are four cases:

1.Individuals stay out of the labor force: $L_i = S_i = 0$. This requires $v'(1) - wu'(I_i) = \alpha > 0$ and $v'(1) - r_iu'(I_i) = \beta > 0$, thus it is a solution for all $I_i > \widetilde{I}$, where $min\{v'(1) - wu'(\widetilde{I}); v'(1) - r_iu'(\widetilde{I})\} = 0$. **This proves the first part of the result.**

2. Individuals face no binding asset constraint and choose $S_i = S^*$,$L_i = 0$ where S^* is such that $r_iu'(r_iS^* + I) = v'(1 - S^*)$. This is a solution for $r_iu'(I_i) - v'(1) > 0$; $\overline{K_i} \geq S^*$ and $wu'(r_iS^* + I) - v'(1 - S^*) = -\alpha < 0$, which requires $r_i \geq w$.

3. Individuals face a binding asset constraint and choose $S_i = \overline{K_i}$, $L_i = \widehat{L_i} \geq 0$ where $\hat{L}$ is such that $wu'(r_i\overline{K_i} + w\hat{L}_i + I_i) = v'(1 - \bar{K}_i - \hat{L}_i)$. This is a solution for $r_iu'(I_i) - v'(1) > 0$; $\overline{K_i} < S^*$ and $ru'(r_i\overline{K_i} + w\hat{L}_i + I_i) - v'(1 - \bar{K}_i - \hat{L}_i) = \delta > 0$, which requires $r_i \geq w$. Note that there exist a level of $\widetilde{K_i} :: wu'(r_i\widetilde{K_i} + I_i) = v'(1 - \widetilde{K_i})$ such that for $\overline{K_i} > \widetilde{K_i}$, $\hat{L}_i = 0$, namely individuals specialize in self-employment even if they face a binding asset constraint. **Taken together, 2 and 3 prove the second part.**

4. Individuals choose $S_i = 0$,$L_i = L^*$ where L^* is such that $wu'(w_iL^* + I) = v'(1 - L^*)$. This is a solution for $wu'(I_i) - v'(1) > 0$; and $r_iu'(r_iL^* + I) - v(1 - L^*) = -\beta < 0$, which requires $r_i < w$. **This proves the third part**

Result 2. Asset transfer effect. *The asset transfer weakly reduces participation, self-employment, labor and total hours worked if the asset constraint was not binding at*

baseline. It does not affect participation but increases self-employment hours, reduces labor hours and total hours worked if the asset constraint was binding.

If individuals are out of the labor force (Case 1) so that $I_i \geq \widetilde{I}$ at baseline, this will hold a fortiori after I_i increases. If $I_i < \widetilde{I}$ individuals for whom the increase in I is sufficient to bring $I_i \geq \widetilde{I}$ will drop out of the labor force. If the increase in I is not sufficient to bring $I_i \geq \widetilde{I}$, we have two cases: (a) when $r > w$ and $\overline{K}_i$ is not binding (Case 2), $S_i = S^*$,$L_i = 0$ and $\frac{dS^*}{dI} = -\frac{ru''}{[r^2u''+v'']} < 0$ because $u()$is concave and the denominator is negative by the second order conditions; so self-employment hours fall and labor hours are unchanged. (b) when $r < w$ (Solution 4), $S_i = 0$,$L_i = L^*$ and $\frac{dL^*}{dI} = -\frac{wu''}{[w^2u''+v'']} < 0$, as above, so labor hours fall and self-employment hours remain unchanged.

When $\overline{K}_i$ is binding (Case 3) we have $S_i = \overline{K_i}$,$L_i = \widehat{L}_i \geq 0$, thus the asset transfer increases $\overline{K}_i$ *and* S_i while $\frac{d\widehat{L_i}}{d\overline{K_i}} = -\frac{[wru''+v'']}{[w^2u''+v'']} < 0$, $\frac{d(\widehat{L_i}+\overline{K_i})}{d\overline{K_i}} = -\frac{w(r-w)u''}{[w^2u''+v'']} < 0$ that is labor and total hours fall.

Result 3. Training effect. *An increase in r weakly increases participation, hours devoted to self-employment and total hours worked if the asset constraint does not bind; it leaves self-employment hours unchanged and reduces labor hours if it binds.*

If $\overline{K}_i$ does not bind individuals are either out of the labor force (Case 1), solely engaged in self employment (Case 2) or in wage labor (Case 4). In Case 1, $I_i > \widetilde{I}$, *where* $v'(1) - r_iu'(\widetilde{I}) = 0$. An increase in r increases the threshold to $I' > \widetilde{I}$ so that all individuals for whom $I' > I_i > \widetilde{I}$ now join the labor force. In Case 2, $S_i = S^*$,$L_i = 0$ where S^* is *such that* $r_iu'(r_iS^* + I) = v'(1 - S^*)$ thus an increase in r leaves $L_i = 0$ unchanged and increases self-employment and total hours because $\frac{dS^*}{dr} = -\frac{u'+rS^*u''}{[r^2u''+v'']} > 0$, as the denominator is negative by the second order conditions and the numerator is positive due to the assumption that the substitution effect of an increase in r (the first term) dominates the negative wealth effect (second term). In Case 4 $S_i = 0$,$L_i = L^*$ where L^* is such that $wu'(w_iL^* + I) = v'(1 - L^*)$. An increase in r has no effect unless it is sufficiently large to make $r > w$, in which case the logic of Case 2 applies.

If $\overline{K}_i$ binds, individuals are in case 3, where $S_i = \overline{K_i}$, $L_i = \widehat{L}_i \geq 0$. An increase in r leaves self-employment hours unchanged as these are determined by the binding constraint and reduces labor hours because $\frac{d\widehat{L_i}}{dr} = -\frac{w\overline{K}u''}{[w^2u''+v'']} < 0$ as the denominator is negative by the second order conditions and the numerator is negative because $u()$. is concave.

2.A.2 Appendix Tables

Table 23: The Lives of the Ultra Poor, by Occupation at Baseline

	(1) Wage employment only	(2) Both self and wage employment	(3) Self-employment only	(4) Out of the labor force
Number of households	1921	1830	2003	1063
Household head male	0.38	0.50	0.71	0.85
	(0.49)	(0.50)	(0.45)	(0.35)
Household size	2.71	3.18	3.56	3.82
	(1.60)	(1.62)	(1.72)	(1.60)
Female respondent literate	0.03	0.05	0.11	0.12
	(0.18)	(0.21)	(0.31)	(0.33)
Female respondent BMI	18.43	18.35	18.31	18.31
	(2.21)	(2.23)	(2.28)	(2.23)
Total PCE	3994.1	4126.2	3879.4	3761.0
	(2346.1)	(2386.2)	(2316.0)	(1736.3)
Durables value	282.7	398.8	555.1	508.6
	(342.2)	(402.8)	(642.5)	(567.2)
Total hours worked,	1459.7	1725.6	891.6	0.0
main female respondent	(742.5)	(722.9)	(710.4)	(0.0)
Total income,	7303.5	7734.6	2923.3	51.5
main female respondent	(4606.0)	(5114.2)	(5045.6)	(465.2)

Notes: Sample restricted to ultra poor households only. Total PCE includes food and non-food expenditure over the previous year. Wealth includes all assets, such as land, livestock, homestead and durables.

Table 24: Normalized Differences – Household Level Variables

	Ultrapoor			Other Poor			Middle class			Upper class		
	Mean		Normalized	Mean		Normalized	Mean		Normalized	Mean		Normalized
	Treat	Control	Difference	Treat	Control	Difference	Treat	Control	Difference	Treat	Control	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Male household head	0.63	0.52	0.15	0.77	0.80	-0.05	0.94	0.94	0.00	0.95	0.95	-0.01
Household size	3.38	3.11	0.11	3.67	3.73	-0.02	4.46	4.40	0.03	4.97	5.08	-0.04
Main female respondent literate	0.08	0.07	0.03	0.15	0.17	-0.05	0.26	0.28	-0.02	0.50	0.54	-0.06
Main female respondent's BMI	18.27	18.47	-0.06	18.77	18.95	-0.05	19.21	19.44	-0.07	20.20	20.33	-0.03
Food Security	0.45	0.36	0.13	0.58	0.49	0.12	0.82	0.79	0.06	0.97	0.96	0.01
Total per capita expenditure	3,996.04	3,909.85	0.03	4,281.72	4,216.81	0.02	5,450.17	5,681.02	-0.03	10,833.23	13,083.55	-0.05
Per capita non-food expenditure	1,024.48	1,034.28	0.00	1,309.93	1,304.15	0.00	2,133.50	2,368.00	-0.03	6,330.70	8,723.13	-0.05
Per capita food expenditure	2,968.66	2,876.92	0.05	2,970.03	2,918.99	0.03	3,314.56	3,311.21	0.00	4,505.56	4,382.12	0.04
Price per calorie	0.91	0.89	0.06	0.92	0.91	0.01	0.99	0.99	0.00	1.21	1.21	-0.01
Wealth	4,907.04	6,585.65	-0.04	13,564.29	14,369.12	-0.01	144,761.50	162,254.70	-0.04	827,596.30	878,337.30	-0.04
Livestock Value	882.40	853.66	0.01	2,562.86	2,544.91	0.00	12,724.35	13,040.51	-0.01	30,874.50	31,719.36	-0.02
Number of cows	0.08	0.06	0.02	0.26	0.24	0.02	1.13	1.18	-0.02	2.65	2.78	-0.03
Number of poultry	0.15	0.18	-0.03	0.23	0.28	-0.04	0.71	0.75	-0.02	1.32	1.15	0.06
Number of goats	1.74	1.75	0.00	2.35	2.74	-0.03	5.64	5.16	0.02	11.83	9.32	0.06
Value of durables	419.09	442.68	-0.03	668.54	752.33	-0.06	2,143.01	2,388.12	-0.05	7,578.33	8,135.49	-0.04
Value of savings	113.60	180.84	-0.06	340.35	433.79	-0.05	1,353.37	1,891.77	-0.04	8,677.25	9,894.96	-0.03
Business activities of the main female respondent												
Hours spent in income generating activities	1,065.94	1,231.97	-0.13	897.94	973.96	-0.07	822.36	799.99	0.02	814.37	809.95	0.01
Hours spent in self-employment	405.97	423.64	-0.02	483.66	519.96	-0.04	711.27	689.25	0.03	773.64	765.53	0.01
Hours spent in wage employment	659.97	808.33	-0.12	414.28	454.00	-0.04	111.08	110.74	0.00	40.72	44.42	-0.01
Wage employment only	0.26	0.31	-0.07	0.15	0.14	0.03	0.03	0.03	0.00	0.01	0.01	-0.02
Both wage and self employment	0.27	0.27	0.00	0.21	0.23	-0.03	0.08	0.07	0.03	0.02	0.02	-0.03
Self employment only	0.29	0.29	0.00	0.42	0.46	-0.05	0.75	0.78	-0.06	0.85	0.89	-0.10
Out of the labor force	0.18	0.13	0.09	0.21	0.17	0.07	0.15	0.13	0.05	0.13	0.07	0.14
Labor force participation	0.82	0.87	-0.09	0.79	0.83	-0.07	0.85	0.87	-0.05	0.87	0.93	-0.14
Total income of the main female respondent	4,607.81	5,533.27	-0.12	3,890.46	4,441.85	-0.06	4,453.39	5,172.37	-0.04	8,398.25	10,930.32	-0.07
Business activities of the male household head												
Hours spent in self-employment	527.65	549.83	-0.02	699.51	740.00	-0.03	1,206.61	1,232.83	-0.02	1,584.46	1,461.99	0.07
Hours spent in wage employment	1,081.47	1,096.08	-0.01	974.16	1,048.62	-0.05	643.76	608.85	0.03	230.20	256.78	-0.03
Labor force participation	0.84	0.86	-0.04	0.81	0.84	-0.06	0.81	0.81	-0.01	0.83	0.80	0.06
Total income of the male household head	11,472.87	11,002.85	0.04	13,844.45	15,332.41	-0.08	22,172.42	25,220.34	-0.08	54,042.31	61,933.85	-0.08

Notes: The normalized differences between the treatment and control observations are calculated based on Imbens and Wooldridge (2009) where the normalized difference is given by $\Delta_X = \frac{\overline{X_1} - \overline{X_0}}{\sqrt{S_0^2 + S_1^2}}$ where $\overline{X_1}$ is the sample mean of treatment observations, $\overline{X_0}$ is the sample mean of control observations, S_0^2 is the sample variance of control observations and S_1^2 is the sample variance of treatment observations.

Table 25: Average Effect of the Program on Other Poor (men)

	(1) Hours spent in self-employment	(2) Hours spent in wage employment	(3) Participation (=1 if hours >0)	(4) Total income
Treatment	-33.98	-10.48	-0.02	-431.86
	(27.86)	(28.81)	(0.013)	(437.06)
N	8576	8576	8576	8576
Adj. R-squared	0.02	0.04	0.04	0.06

Notes: Sample is restricted to male household heads in other (non-selected) poor households. OLS estimates, standard errors clustered at the community level in parenthesis. (***) (**) (*) indicate the coefficient is different than zero at the (1%) (5%) (10%) level. Hours are computed over the past year. Total income includes income from all income generating activities, for all household members if the activity involves more than one member. All regressions include subdistrict fixed effects.

3 Do Formal Transfers Crowd Out Informal Transfers to the Poor? Evidence from Bangladesh

3.1 Introduction

The poor in developing countries often depend on assistance provided by their social networks. As such, while analyzing the impact of programs targeted to the poor, it is important to take into account their effect on the informal transfer arrangements of the poor. This explains the large body of literature that is concerned with the question of whether formal transfers crowd out informal ones (Cox and Jakubson (1995), Jensen (2003), Rosenzweig and Wolpin (1994)). Theoretically, several different models of informal transfers predict a negative effect of formal transfers on informal ones. Yet, depending on which model of informal transfers one considers, the effect of formal transfers can be positive, or non-monotonic, depending on the characteristics of the recipient households. As such, empirical evidence on the impact of formal transfers on informal ones is important, both for policy-making – to improve the design and effectiveness of anti-poverty programs – and theoretically – to distinguish between different models of informal transfers.

Empirically, testing for the effect of formal transfers on informal transfer arrangements is a challenge as the beneficiaries of formal transfers are not a random sample of the population. Therefore, an identification strategy based on comparison of beneficiaries of programs that entail a formal transfer to non-beneficiaries is likely to suffer from selection bias. In this paper, I exploit the randomized roll-out of a large-scale asset transfer and training program in rural Bangladesh, the ultra poor program, to identify the causal impact of the program on the informal transfers received by the targeted poor. The program targets the poorest women in rural Bangladesh, transfers them an asset (often livestock) and trains them about how to use this asset to generate income. As a result, the income of targeted women and their households increases significantly. This unconditional formal transfer received by the targeted poor women is likely to impact the informal transfers they receive, and I test for these effects in this paper. As such, the paper provides the first piece of experimental evidence on the effects of an unconditional transfer program on the informal transfer arrangements of the targeted poor.

In addition to providing an asset transfer and complementary services to the poor, the ultra poor program also entails a component that was designed explicitly to address

the potential crowding out of existing transfer networks of the targeted poor households. In every village that the program operates, village-level poverty alleviation committees were established to bring together representatives of the local elites with representatives of the targeted poor households. The elites in these committees were instructed specifically to focus on the needs of the targeted poor households, to provide assistance to them, and in particular to help out those among the targeted poor who are most in need of such assistance. As these committees were established in every treated village, I cannot empirically identify the effect that the establishment of the committee has on informal transfers in isolation from the effect of the asset transfer and other components of the program. However, in order to shed some light on the potential mechanisms of program's impact on informal transfers, I collected data on incidence and types of assistance offered by the elites who are members of the committee to households in treatment villages and I use this data to document how the member elite re-allocate their assistance towards the more vulnerable households among the targeted poor in treated villages.

The key findings of the paper are as follows:

First, the program crowds out informal transfers to the treated households, both on the extensive margin (likelihood of receiving a transfer) and on the intensive margin (value of transfers received). Average targeted household is 10% less likely to receive an informal transfer, after the treatment. Moreover, conditional on receiving an informal transfer, the value of transfers received by the average targeted household is lowered by 57% as a result of the program.

Second, this crowding out is driven by transfers received from others within the same village, while transfers from outside the village are not affected. The average targeted household is 12% less likely to receive an informal transfer from a neighbor within the same village, while the corresponding effect for transfers from outside the village is only 2% and insignificant at conventional levels. Similarly, conditional on receiving an informal transfer, the value of informal transfers received from within the village is reduced significantly, while the value of transfers from outside the village is not affected. This finding is consistent with models of informal transfers under asymmetric information, as the effects of the program on the ultra poor are likely to be more observable for their neighbors who live in the same village[50].

Third, this crowding out effect on the informal transfers received by the targeted poor households is highly heterogenous, depending on their vulnerability at baseline –

[50]This is not to say that the finding cannot be explained by other models, as will be discussed below. An alternative explanation could be the existence of different types of motivations for within village transfers and remittances from outside, as in Lucas and Stark (1985)

as measured by the food security of the household. Those facing greater food insecurity at baseline (who report having less than enough food more frequently) experience significantly lower crowding out than others.

Fourth, I provide supportive evidence that suggests that the heterogeneity in the crowding out of informal transfers of the targeted poor is likely to be driven by the targeting of transfers from members of the village-level committees that are established as part of the program. I show that, within the treated villages, the elites who are members of the committee are more likely to provide assistance (in terms of transfers, help with accessing services and resolving disputes) to the targeted poor. Moreover, the member elites change the way they target their assistance to the targeted poor such that they are more likely to target those who had greater food insecurity at baseline. This re-allocation of assistance by the elites towards the more vulnerable poor could explain why the informal transfers received by vulnerable poor households is crowded out *less* relative to those with less vulnerability to start with.

Empirically, there is a large literature that tests for the correlation between income of the agent and the amount of informal transfers he/she receives. Most empirical studies find modest negative correlation between household income and transfers received (Cox and Jakubson (1995), Altonji *et al* (1997)). Cox *et al* (2004) show that the responsiveness of transfers to resources may be highly non-linear and they find, using data from the Phillipines, that in low-income households transfers are more sensitive to changes in income of the recipient. Similarly, Schoeni(1997) finds that poorer households in the US PSID dataset are more likely to receive transfers, both in money and in time.

Jensen (1998) tests directly for the crowding out hypothesis, focusing on remittances received by households in South Africa. He finds that migrant remittances are significantly reduced due to the introduction of a public pension scheme in the home country. In a study very similar in spirit to this paper, Albarran and Attanasio (2002) exploit the exogenous increase in income of poor households as a result of a conditional cash transfer program in Mexico, PROGRESA. They find that beneficiaries of the program are less likely to receive informal transfers, and conditional on receiving any, they receive lower amounts of transfers. Key distinction between their paper and the current study is the program they analyze is "not a pure transfer programme" (Albarran and Attanasio (2002)), and although it has a sizeable unconditional component, it is in essence a change in the relative prices of education and health services for the targeted poor households.

This paper contributes to the literature by providing the first piece of experimental evidence on the impact of an *unconditional* transfer program in Bangladesh on the in-

formal transfer arrangements of the targeted poor households. As such, it tests directly the question at hand – whether a formal transfer crowds out informal ones. Furthermore, it shows that the crowding out effect is likely to be heterogenous, depending on the location of the sender and on the characteristics of the targeted households.

The rest of the paper is organized as follows: Section 3.2 describes the context and ultra poor program in greater detail, section 3.3 discusses the key theoretical mechanisms through which the program may crowd out informal transfers to the poor, section 3.4 describes the data used in the study, section 3.5 presents the empirical results and section 3.6 concludes.

3.2 Context

BRAC, formerly known as the Bangladeshi Rural Advancement Committee, is the world's largest and fastest growing NGO with programs in microfinance, education, health, environment and social empowerment. BRAC has been a pioneer in implementing programs that target extreme poverty in Bangladesh. This paper focuses on the second phase of this program, which was started in 2007 and aims to target 860,300 households in 40 districts of the country by 2011. The program targets women living in rural parts of the country who are unable to access and benefit from mainstream poverty reduction programs and is currently being replicated in a number of countries around the world.

BRAC's ultra poor program aims to economically, socially and psychologically empower the poorest women in Bangladesh through a multi-faceted intervention. The program provides a massive wealth shock for the targeted women, relative to their baseline wealth. Targeted women receive a combination of assets, such as cows, goats, poultry or seeds for vegetable cultivation. The value of the average asset transferred is double the baseline wealth of the average targeted poor household. The asset transfer is accompanied with skills training, specific to the type of asset provided. A trained asset specialist visits treated households every 1-2 months for the first year of the program. In addition, BRAC program officers visit weekly for the first two years to ensure that these very poor women who have no prior experience of running a business are fully supported[51].

[51] Other components of the program are a savings scheme, preventive and curative health care services and social development support involving training on legal, social and political rights. The ultra poor households receive monthly visits from a health volunteer and have access to BRAC's legal services. To compensate for the short run fall in income due to the occupational change, a subsistence allowance is provided for the first 40 weeks, that is until the treated learn to manage the assets well enough to generate a regular flow of income. Between 18 and 24 months into the program, the beneficiaries also take part in confidence-building sessions about how to use microfinance and are enrolled in village-level

The ultra poor program identifies the most vulnerable poor women in villages through a combination of community wealth ranking and the program's pre-determined selection criteria. Initially, in every village that the program operates, they carry out a participatory wealth ranking where community members divide the village population into, typically, 5 wealth ranks. Households ranked at the bottom wealth category become the "community-selected poor". Among the community-selected poor households, BRAC officials select households that will receive the asset transfer based on a number of pre-determined criteria. There are three exclusion criteria, all of which are binding. If the household is already borrowing from a microfinance-providing NGO, is a recipient of a mainstream government anti-poverty program, or if there is no adult woman in the household, then it is automatically excluded from the program. Furthermore, a selected household has to satisfy three of the following five inclusion criteria: (i) total land owned including homestead is not more than 10 decimals; (ii) there is no adult male income earner in the household; (iii) adult women in the household work outside the homestead; (iv) school-going-aged children have to work; (v) the household has no productive assets.

Throughout the analysis, poor households that are selected as eligible beneficiaries for the program will be referred to as "the ultra poor", while households who are ranked as poor by the community (i.e. are community selected poor) but not found eligible for program participation by BRAC according to their selection criteria will be referred as "the other poor".

As part of the program, BRAC establishes "Gram Daridro Bimochan Committees" (Village Poverty Alleviation Committees, henceforth "the committees") in every village where the ultra poor program operates. The idea to establish these committees originated during the initial pilot stage of the program. Program officers realized that involvement of the local elites would ensure their support was behind the targeted poor households and the ultra poor program, this being crucial to its success. Committees consist of 11 members: 8 elite members (selected by the community, through a meeting[52] organized by BRAC), 2 representatives of the ultra poor and 1 BRAC staff. Main goals of the committee is to create awareness among the elite on the needs of the poor, to enable them to coordinate their transfers so that they can target the poor and the

microfinance organizations.

[52]Selection of the elite members in the committee happen in the following way: BRAC officers invite all villagers to the meeting. They describe the ultra poor program and the responsibilities of the committee. Then the villagers are asked to nominate people they think should be on the committee. Consent of the participants on all the names is taken. If anyone thinks a nominated elite should not be on the committee, then their name is taken off the list of committee members. Finally, the nominated members are asked if they accept to be on the committee. For more details on this selection procedure, see Dutta *et al* (2010).

needy and/or invest in costly public projects that may be difficult for one individual to carry out (e.g. public sanitation), and last but not least to ensure that the assets transferred to the ultra poor are protected by the local elite from any harm (Hossain and Matin (2004), Dutta *et al* (2010)). They meet regularly (at least once a month) and during every meeting the representatives of the ultra poor talk about who among the ultra poor is experiencing difficulty and is in need of assistance. The meetings are held in open space, usually in the village center and in principle anyone is free to attend and to participate in the discussions. Although the main goal of the committee is to assist the ultra poor, they are welcomed to help whoever may be in need of assistance in the village.

3.3 Conceptual Framework

The literature has highlighted three key motivations for informal transfers: The first is that of altruism (Becker (1974)), where the donors simply care about the well-being of the recipients of their transfers. Alternatively, donors may be motivated by exchange where they make transfers in expectation of some service that will be delivered by the recipient (Bernheim *et al* (1985)). Recent theoretical and empirical studies on informal transfers have focused on informal insurance (Townsend (1994)) where transfers may represent the pooling of resources among a group of households that enables them to cope with stochastic income shocks and doing so, to insure themselves against risks.

Under altruism, formal transfers would unambiguously crowd out informal transfers. Barro (1974) shows that if informal transfers stem from pure altruism in an overlapping generations model, formal transfers will be completely undone by informal transfers. An increase in the amount of formal transfers received by an agent would reduce both the probability that the agent will receive any informal transfers and conditional on receiving any informal transfers, will imply a lower level of informal transfers.

If informal transfers take place in exchange for a service provided by the recipient, then the effect of an increase in the amount of formal transfers on the amount of informal transfers received would be ambiguous. For instance, Cox *et al* (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 of 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. Following a similar intuition, Cox *et al* (2004) show that, the combination of altruism and exchange motives may result in a highly non-linear response to an increase in the income of the recipient.

Alternatively, if informal transfers are part of informal insurance arrangements between agents, an increase in formal transfers (or formal insurance) received by an agent will partly be undone by transfers to insurance partners of the agent (see Albarran and Attanasio (2002) for a discussion)[53]. Under informal insurance with asymmetric information, if formal transfers are observable (or they are a function of some observable characteristic of the agent such as lack of assets), informal transfers will be reduced through the same channel as under perfect risk-sharing. If the observability of the formal transfer is better for certain network members (such as those who live in the same village as the beneficiary as opposed to those who live far away), the crowding out effect is likely to be stronger for transfers received from the network members for whom the observability is easier.

An important aspect of the program at hand with respect to its potential effects on informal transfers is the establishment of village-level poverty alleviation committees. As described above, these committees are established with a view towards minimizing the crowding out of informal transfers that may occur due to the program, and in particular enabling the elite to focus their transfers on the more vulnerable households among the poor. This implies, to the extent that the committee component of the program succeeds in achieving its goal, the crowding out effect is likely to be heterogenous and depend on the vulnerability of the recipient household. In addition to testing for crowding-out effects, I will test explicitly for whether these effects are heterogenous based on this. Moreover, I will provide supportive evidence to demonstrate that this is driven by the role of the committees.

3.4 Data Description

Data used in this study comes from a data collection exercise that was undertaken to evaluate the effects of the ultra poor program in Bangladesh. The evaluation strategy was designed to exploit the roll-out of the program across the country. The timing of the roll-out was randomly chosen at the branch office level. A branch office covers a large area with a radius of approximately 4km. The ultra poor program determined 40 branch

[53]Under perfect risk-sharing (Townsend (1994)), 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 he/she receives will lead to a decrease in the amount of informal transfers he/she receives. Generalizing the perfect risk-sharing model to allow for imperfect insurance due to, for instance, imperfect enforceability (Coate and Ravallion (1993), Ligon *et al* (2002)) or asymmetric information (Ligon (1998)) yields similar predictions, although the mechanism at work might be slightly different. Attanasio and Rios-Rull (2000a, b) show that, under imperfect enforceability, introduction of unconditional formal transfer 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.

offices that would implement the ultra poor program. Standard procedures to identify who would be the beneficiaries of the program were carried out in all 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, the rest in 2011. All villages in treatment branches were treated in 2007.

In every village that was part of the study, an initial census of all households was carried out between April 2007 and January 2008. Following the census of all households in the village, a detailed questionnaire was carried out on a smaller sample that included all poor households and a random sample of the rest of the village. Households in this sample were surveyed at baseline (between April 2007 and February 2008) and two years after (January-December 2009). The poor households eligible for treatment were selected at the same time in both treatment and control branches, using the same method outlined above. The only difference between them is that the poor in treated branches receive the assets immediately whereas the poor in control branches will receive them in 2011.

The main survey modules were directed towards the main female in the household, as the program is targeted towards women. The survey questionnaire measures a rich set of individual outcomes, including occupational choices, income and expenditure, business and household assets, and – most importantly for the purpose of this study – transfers. The respondent were asked to report the up to 5 most important sources of transfers received during the last one year. Less than 1% of the respondent in ultra poor and other poor households reported 5 sources of transfers at baseline, thus the "up to 5" constraint does not seem to be a binding limitation and is unlikely to affect the results. For every transfer source, the respondents were asked to report the value of transfers in cash/kind received during the last year from this source and the location of the sender (whether in the same village).

Finally, in order to provide supportive evidence on the role of the committee component of the program on targeting of informal transfers, the respondents were asked if they knew any of the committee members, and for each individual committee member if they had received any assistance from him/her in the last two years. Given that this information could only be collected at the followup survey (since the committees were not established at the time of the baseline survey), respondents were also asked to report, whether they had received any assistance from any of the members prior to the establishment of the committees (before 2 years) and if they had any social connections to each member.

3.4.1 Informal Transfers to the Poor at Baseline

Table 26 provides summary statistics on the informal transfers received by the poor households at baseline, in treatment (Column (1)) and control (Column (2)) communities. Column (3) provides normalized differences[54] between control and treatment samples, verifying that the ultra poor in treatment and control villages at baseline were not significantly different with respect to their informal transfer arrangements and key observable characteristics at baseline.

Row 1 of Table 26 shows that 20% of the ultra poor households in treatment communities report having received some transfer, in cash or in kind, during the year preceding the baseline survey. The corresponding figure is 25% among the sample of ultra poor in control communities. Column (3) shows that the normalized difference between the two is 0.09, which is less than the benchmark level of 0.25. Rows 2 and 3 break down the informal transfers received by ultra poor households into those received from their social network within the village and from their outside-village networks respectively. 8.4% of the ultra poor households in treatment communities (10% in control) have received some transfer from another household within their village. Row 3 shows that the ultra poor households were more likely to have received transfers from outside the village, compared to within – proportion of households that have received a transfer from outside the village is 13% among the ultra poor in treatment communities and 18% in control. The fourth row of Table 26 shows that in terms of the value of the transfers received, the ratio of within village transfers to overall transfers is 0.37 for the ultra poor in treatment communities and 0.35 for those in control.

The next two rows provide summary statistics on the value of transfers received. Row 5 shows that the average ultra poor household in treatment communities reports having received 212 Takas of transfers at baseline, the corresponding figure is 228 Takas for the sample of control households. Conditional on having received any transfer, the ultra poor households have received 1079 Takas worth of transfers in treatment and 916 Takas in control communities. In order to get an idea about the value of informal transfers relative to total household expenditure the next two rows provide summary statistics on the total annual expenditure[55] of the sampled households, and those who have received an informal transfer respectively. Total expenditure of the average ultra poor household is around 11,000 Takas in treatment communities. This implies that the value of reported informal transfers relative to household expenditure is rather

[54]The normalized differences between the treatment and control observations are calculated based on Imbens and Wooldridge (2009).

[55]Food expenditure is imputed at the yearly level on the basis of reported food expenditure in the last three days.

low (around 2%) for the average ultra poor household. On the other hand, among those who reported having received any transfers, total expenditure is roughly 10,500 Takas, which implies that the value of transfers correspond to roughly 10% of their total household expenditure. This suggests that among those who received informal transfers at baseline, value of transfers was likely to be a key source of income for the household and thus any impact of the program on them is likely to have important consequences for the lives of the ultra poor.

The final row of Table 26 provides summary statistics on the vulnerability of the ultra poor households. When asked "How often did your household could not eat enough within the last month", 32% of the ultra poor (36% in control) respond that their household didn't have enough food to eat at least once a week.

3.4.2 Identification Strategy

To test whether the ultra poor program leads to crowding out of informal transfers received by the targeted poor households, I use the following identification strategy:

$$y_{it} = \alpha + \beta T_i + \delta R_t + \lambda T_i \times R_t + \gamma' X_{it} + \epsilon_{it} \tag{35}$$

where the sample is restricted to the ultra poor households selected by the program in treatment and control villages, y_{it} is a measure of informal transfers received by household i in period t; $T_i = 1$ if household i lives in a treated village and $= 0$ if they live in a control village, $R_t = 1$ after the program and 0 otherwise. The parameter of interest is λ, the difference in difference between treatment and control before and after the program. The standard errors are clustered at the village level in all the regressions. Under the identifying assumption that the control villages represent a valid counterfactual for the treated villages in the absence of the program, namely that trends in all outcomes of interests are the same in treatment and control, λ identifies the causal effect of the treatment.

In order to test for heterogenous crowding out effects, based on baseline vulnerability of the targeted poor households, I estimate heterogenous diff-in-diff specifications where I use a measure of food security of the household at baseline as the interaction term. More specifically, the model I estimate for the heterogenous effects is:

$$y_{it} = \alpha + \beta_1 T_i + \beta_2 R_t + \beta_3 Z_{i0} + \beta 4 T_i Z_{i0} + \beta_5 R_t Z_{i0} + \lambda_1 T_i R_t + \lambda_2 T_i R_t Z_{i0} + \gamma' X_{it} + \epsilon_{it} \tag{36}$$

where y_{it}, T_i, R_t and X_{it} will be the same as in (35) and Z_{i0} will be either a measure of the extent of food insecurity in household i at baseline or a dummy variable for

whether household i experienced severe food insecurity at baseline. The parameters of interest are now λ_1 and λ_2: λ_1 will be the difference in difference between treatment and control households with $Z_{i0} = 0$ (e.g. those who did not experience any food insecurity at baseline) before and after the program; λ_2 will be the additional effect of the program on those households with $Z_{i0} = 1$ (e.g. those who experienced food insecurity at baseline). In the case where Z_{i0} corresponds to a continuous measure of food insecurity, λ_2 will capture the change in the effect of the program depending on the food insecurity of the household at baseline.

3.5 Empirical Results

3.5.1 Average Effects on Informal Transfers Received by the Ultra Poor

In order to test for crowding out of informal transfers to the targeted poor households, I estimate (35) on the sample of ultra poor households selected by the program in treatment and control villages. Table 27 provides the results. In Column (1), the dependent variable is a dummy variable equal to 1 if the respondent's household has received any transfers during the last year. The coefficient of "treat x post" gives the difference-in-difference estimate. I find that, on the extensive margin, the program crowds out informal transfers. The treated ultra poor households are 9.5 percentage points less likely to receive transfers relative to the ultra poor in control villages. This is a very large effect relative to the baseline level – at baseline 20% of the ultra poor had reported having received any transfer.

Column (2) tests for the crowding out of informal transfers on the intensive margin. The dependent variable is the log value of transfers received, and to account for the fact that many observations are truncated at 0, I use a Tobit model to estimate the effect. The diff-in-diff estimate is -.84 and significant at 5%, which implies that the informal transfers to the ultra poor were crowded out both on the extensive and the intensive margins by the program[56].

[56] A striking result is that the "post" dummy, which captures the time trend in informal transfers that is common to both treated and control households, is large, positive and significant, in both columns (1) and (2) of Table 27. This implies that the informal transfers reported by ultra poor in both the treatment and control communities at the followup survey is much higher relative to baseline, both on the extensive and on the intensive margins. A complete explanation of this time trend is beyond the scope of this paper. however a potential reason for this effect is the food price hike experienced in Bangladesh and other developing countries in South Asia during the time of the baseline survey. Average price of rice, the main staple, in the country increased from Taka 15.9 per kg in January 2006 to Taka 30.8 per kg in August 2008. That is an increase of over 94% during this period (Sulaiman *et al* (2009)). To the extent that this increase in prices also increase the cost of transfers, this could explain why informal transfers received by all households were much lower at the time of the baseline survey, relative to the followup.

Next, I test for the effect of the program on informal transfers by the location of the sender. There are many reasons why one would expect the effects to be different for transfers from within and outside the village. One key mechanism highlighted in the literature is that of asymmetric information (for example Ligon (1998), Kinnan(2010)). Households who live in the same village as the treated ultra poor are much more likely to be aware of the extent of the wealth shock and the assistance provided by BRAC to the ultra poor, relative to their network outside the village. Moreover, given that the committees established by BRAC are village-level institutions targeted toward improving the effectiveness of assistance from the village elite to the poor, the effects of the program on within village transfers are likely to be highly different than those from outside the village.

Using data at the transaction level on whether each transfer was received from someone inside or outside the village, I define total value and incidence of transfers received from within the same village and from outside the village. Table 28 provides the results of estimating (35) for value and incidence of informal transfers received by the ultra poor from within the village and outside.

In Column (1), the dependent variable is a dummy variable equal to 1 if the household received any transfers from another household that lives in the same village. Treated ultra poor households are 12% less likely to receive transfers from others in the same village relative to baseline and relative to the ultra poor in control villages, and this effect is estimated precisely at conventional levels.

Column (2) shows that the value of transfers received from within the village, conditional on having received any, is also lower for treated ultra poor relative to the poor. The difference-in-difference estimate on the log value of transfers received from within the village is -2.42 and significant at conventional levels. On the other hand, columns (3) and (4) show that transfers received from outside the village are not significantly affected by the program. The difference-in-difference estimate for whether the household received any transfer from outside the village is -0.02 and insignificant at conventional levels. Similarly, the effect on the value of transfers from outside the village, conditional on receiving any, is 0.15 and insignificant.

Overall, the results on the average effect of informal transfers received by the ultra poor imply that the formal transfer studied (i.e. the ultra poor program) crowds out informal transfers to the targeted households from within their village considerably, but it leaves the incidence and the level of transfers they receive from outside the village largely unaffected.

In light of the conceptual framework described in section 3.3, the finding that formal transfers do crowd out informal ones are consistent with the altruistic and risk-sharing

models, but not with the exchange model of informal transfers. On the other hand, the fact that it is only informal transfers within the village that are crowded out, while transfers from outside the village are not affected is consistent with both models of altruistic transfers or informal insurance under asymmetric information. An alterative explanation is that different mechanisms might be at work behind transfers from within and those from outside the village – e.g. remittances could be modeled as payments for services carried out in the past (such as investing in one's human capital, or paying for migration costs) or in the present (such as taking care of the land left behind, or investing in the home country – as in Lucas and Stark (1985)), and thus more likely to be affected by the exchange motive relative to transfers from within the village.

3.5.2 Heterogenous Effects on Informal Transfers

As discussed in Section 3, due to the design of the program (in particular, the committee component), one would expect that the crowding out effect may be heterogenous depending on the vulnerability of the ultra poor households. In order to test for this directly, I estimate the model in (36), where I estimate the differential effect of the program by the food security of the ultra poor households at baseline. For brevity I only report the estimates for λ_1 and λ_2, but the full set of results including the estimates of all the parameters in (36) is provided in the Data Appendix. I use food security measured at baseline in order to abstract from erogeneity problems.

Table 29 provides the results on heterogenous effects of the program on the intensive (Panel A) and extensive (Panel B) margins of informal transfers received by the ultra poor. In columns (1)-(3), the measure of food insecurity (or food deficiency) at baseline is a question from the household survey where the respondents were asked to report the frequency with which during the last month their household couldn't eat enough. Their answers were coded such that "never" is coded as [1], 1-3 times a month as [2], 1-2 times a week as [3] and more than 3-4 times a week as [4]. Column (1) shows that the triple interaction term is positive, which implies that the crowding out of informal transfers is decreasing in the baseline food insecurity of the ultra poor households, however this effect is estimated imprecisely at conventional levels. Column (2) and (3) estimate the same relationship for within village and outside village transfers respectively, and the triple interaction term, though insignificant at conventional levels, is positive for transfers within the village while it is negative for transfers from outside. This implies that the crowding-out effect is decreasing in the vulnerability of the household for transfers within the village, but not for those outside.

In columns (4)-(6), the measure of baseline food deficiency is a dummy variable equal to 1 if the respondent's household didn't have enough food to eat at least once

a week during the last month. In Column (4), the triple interaction term is positive and large. The difference-in-difference estimate for those households who did not face severe food insecurity at baseline is -1.78 and significant, implying that program led to a crowding out of informal transfers for these households. On the other hand, the difference-in-difference estimate for those who faced severe food insecurity at baseline ($\lambda_1 + \lambda_2$ in (36)) is -0.15 and insignificant, implying that the program did not lead to a crowding out of informal transfers to the targeted households who faced severe food insecurity at baseline. Looking at Panel B, one can see that the crowding out effect on the extensive margin is similarly lower (the estimate for λ_2 is 0.06 and significant) for households who faced severe food insecurity to start with.

Column (5) repeats the same analysis for transfers received from within the village. Panel A shows that the crowding out effect on informal transfers from within the village is lower for households that had greater food insecurity at baseline – the difference between the effect on households facing severe food deficiency and those not is 2.2 and significant at 5% level. This implies that, on the intensive margin, the program crowded out informal transfers from the village to the targeted poor who did not face severe food insecurity at baseline, but the within-village informal transfers received by those who faced severe food insecurity were unaffected.

Panel B repeats the same exercise on the extensive margin and shows that the crowding out effect was similarly lower on the extensive margin (households that did not face severe food insecurity at baseline were 13% less likely to receive a transfer from within the village at followup relative to control, while those who faced severe food insecurity were 8% less likely to do so). This supports the hypothesis that the crowding out effect of the program on informal transfers was lower for more vulnerable households.

Finally, column (6) confirms that there was no significant crowding out effect on transfers from outside the village, regardless of the baseline vulnerability of the targeted households. In the next section, I provide evidence on one potential channel that may explain this heterogeneity of crowding out – the village-level poverty alleviation committees established by BRAC as part of the program.

3.5.3 Mechanisms: Evidence on the Role of the Committee

As described in detail in Section 3.2, the ultra poor program is a multi-faceted program that entails many components. One of its components, the village-level "poverty alleviation committees", has direct relevance for the effects of the program on informal transfer arrangements of the ultra poor households. In every village that the program operates, BRAC arranges community meetings where community members select vol-

unteers among the elite who become members of these committees. Table 34 in the Data Appendix provides summary statistics on the characteristics of the selected elite members of the committee, relative to the ultra poor and other poor households in the sample. They are relatively wealthy land-owners who employ people and who are relatively well-educated compared to the poor. These elites who are members of the committee are instructed by BRAC workers to assist the ultra poor in particular and to use the information provided by ultra poor representatives on the committee to target those among the ultra poor who are in greater need of such assistance. As such, to the extent that the establishment of the committee is successful in changing the way the member elite target their assistance so that they start targeting the more vulnerable ultra poor households, this could explain the previous finding on the heterogeneity in crowding out of informal transfers to the ultra poor.

Table 30 provides descriptive statistics on the transfers and other types of assistance provided by elite who are members of the committee to the ultra poor and the other poor households in the sample, before and after the introduction of the committee. The first row in table 30 shows that before the inception of the committee, only 10% of the ultra poor and 6% of the other poor households had received any assistance from any committee member, while after the establishment of the committee 43% and 9% of them respectively had received some type of assistance. Thus, while the committee members increased the number of poor they target in both groups, they direct their targeting more towards the ultra poor households after the establishment of the committee, relative to the other poor.

The next 4 rows break down this assistance by the type of assistance received. Among the ultra poor, the proportion that had received transfers in cash or in kind from a committee member increased from 6% to 26%, the proportion who received help to access services (such as government programs, health or education services) increased from 3% to 11%, and the proportion that received a sanitary latrine or a tubewell from 0 to 11%. Moreover, 6% of the ultra poor had received help from a committee member to resolve disputes with other households in the village (only 1% had received such assistance from the member elite before the introduction of the committee). The corresponding increases are much smaller for the other poor households.

The final row of Table 30 shows the proportion of ultra poor and other poor households that have family connections to anyone on the committee. 11% of ultra poor and 7% of other poor have family links to a member on the committee.

Next, I test whether the committee members change the way they target their assistance to the ultra poor towards those who are more vulnerable. Ideally, in order to identify the impact of the program on targeting of transfers from committee members

to the poor, I would compare the change in transfers from the member elite to the poor in treatment villages to transfers from a comparable group of elites to the poor selected by the program in control villages. Since the committees were not established in control villages, I do not observe who among the elite would be the comparable group in control villages to the member elite in treatment villages. Moreover, majority of the member elite live outside the villages that are the sampling unit for the household survey, hence a methodology based on predicting who among the control households would be selected if the committees were to be established is not feasible. Thus, to test for the pattern of change in the targeting of assistance from member elite to the poor in treatment villages, I will compare targeting before to targeting after in treatment villages. The lack of a proper control group, of course, implies that the results may suffer from omitted variable bias due to any factor that might be correlated with the pattern of change I estimate, and the following results on targeting of transfers from committee members to the ultra poor should be interpreted with caution.

In order to identify the change in targeting of assistance from the member elites to the ultra poor, I estimate the following model on the sample of ultra poor in treatment villages:

$$y_{it} = \alpha + \delta P_t + \gamma' X_{i0} + \zeta' P_t X_{i0} + \epsilon_{it}, \tag{37}$$

where y_{it} is a dummy variable equal to 1 if poor household i received any assistance from any of the committee members in period t, P_t is a dummy variable equal to 1 if the observation refers to the period after the inception of the committee, and X_{i0} are baseline characteristics of household i that capture its wealth, vulnerability or social connections to the committee members before the establishment of the committee. The coefficient of interest is ζ: the change in the marginal effect of an underlying characteristic of household i on the probability that the household is targeted by committee members, relative to baseline.

Table 31 provides results of estimating (37) where the dependent variable is a dummy variable equal to 1 if the respondent in ultra poor household i reported having received any assistance from any committee member in period t. Column (1) re-iterates the finding in the descriptive statistics on assistance provided by committee members to the ultra poor. It shows that the average ultra poor household was 34% more likely to have received some form of assistance from a committee member after the introduction of the committee, relative to before. Column (2) controls for food deficiency of the household at baseline and its interaction with the "post" dummy. The coefficients "food deficiency" as measured at baseline is practically zero, which implies that prior to the program (and the introduction of the committee) the committee members were

not targeting their assistance differentially more towards the more vulnerable ultra poor that were facing food deficiency. On the other hand, the coefficient for "post × food deficiency" is 0.03 and significant at 5% level, implying that after the program, a one point increase in the frequency of food deficiency faced by household i increases the likelihood that it will receive assistance from the elites who are members of the committee by 3%. This could be a potential mechanism through which the heterogenous effects of crowding out described in the previous section arise – the emphasis of the committee on targeting more vulnerable poor in their transfers could explain why the crowding out of informal transfers as a result of the program happens less for the more vulnerable ultra poor. Having said this, this doesn't rule out alternative mechanisms through which this effect might have taken place.

Columns (3) and (4) add additional controls to control for other observable characteristics of the ultra poor households that are likely to be correlated with their food deficiency at baseline. In column (2), I control for baseline wealth (total value of household assets in Takas) of the household. The coefficient for "post × food deficiency" is practically unchanged. The coefficient of baseline wealth is negative and insignificant. Column (4) controls for whether ultra poor household i has any family connection to anyone on the committee. As expected, those who have a family connection are more likely to receive assistance from the committee members, both before and after the program (the coefficient of "post × family" is negative, suggesting that the marginal effect of having a family connection to the members is lower after the program, but this is imprecisely estimated at conventional levels). The coefficient of "post × food deficiency" is practically unchanged.

In columns (5)-(8), I repeat the same analysis for the sample of "other poor" households (those that were ranked as poor by the community but not selected for treatment by the program, as they do not satisfy the inclusion/exclusion criteria described above) as a placebo test. If the pattern of reallocation of assistance from the committee members to the ultra poor has nothing to do with the introduction of the committee, then one would expect to see a similar pattern in the assistance from the committee members to the other poor. Results in columns (5)-(8) show that this was not the case. The change in the pattern of targeting of other poor households by the committee members is in stark contrast to that for the ultra poor: There is no evidence to suggest that the differential targeting of more vulnerable households by the committee members also occurs for the other poor. Interestingly, committee members seem to target other poor who have a family connection to them differentially more at followup.

In Table 32, the results of estimating (37) for the different types of assistance (transfers in cash/kind, help with gaining access to services such as poverty cards, schools

or health-centres, help with resolving disputes, and receiving a sanitary latrine or a tubewell) of the committee members are provided. The key result is that the finding that assistance from committee members is more likely to be provided to ultra poor who are more vulnerable face greater food deficiency) is driven by assistance in terms of transfers and help with getting access to services as opposed to other types of assistance. Column (1) shows that ultra poor who faced greater food insecurity at baseline are more likely to receive a transfer from a committee member at followup (coefficient of "post × food deficiency" is 0.023, although insignificant at convectional levels), and column (2) shows that this is robust to controlling for wealth of the ultra poor and their family connections to the committee members. Columns (3) and (4) show that assistance in terms of help with getting access to services (such as poverty cards) was more likely to be targeted to the ultra poor who faced greater food insecurity at baseline. On the other hand, columns (5)-(6) show that there is no evidence to suggest that such differential targeting of assistance took place for help in resolving disputes or receiving sanitary latrines or tubewells – the coefficient of "post × food deficiency" is practically zero in these regressions. This supports the intuition that crowding out of informal transfers are likely to be lower for more vulnerable ultra poor as a result of this differential targeting of transfers from the committee members.

3.5.4 Robustness Checks

In the above discussion, I showed that the crowding out effect of the program on the informal transfers received by the poor is heterogeneous by their baseline food security, and argued that this is likely to be driven by the role of the village elite committees established by the program which reallocate assistance provided by the member elite from the less vulnerable poor to those who faced greater food insecurity at baseline. Naturally, one can think of alternative mechanisms to explain why the crowding-out effect may be heterogenous depending on the food insecurity of the targeted poor households, regardless of the role that the village elite committees may have played. In this section, I carry out some robustness checks to rule out some of these alternative mechanisms.

One could argue that baseline food insecurity is acting as a proxy for some unobservable characteristic of the targeted poor households that is correlated with it and causes the observed heterogeneity in the crowding out effect. An important candidate for such a characteristic is the heterogeneity in the social networks of the ultra poor. To the extent that the ultra poor who faced greater food insecurity at baseline were also the ones who had stronger social networks that were less likely to diminish their transfers to them after the program, food insecurity could be a proxy for this heterogeneity in the network structure of the targeted households. In order to control for

this explicitly, I use data on the family network of the surveyed households as a control variable in the previous analysis. More specifically, every respondent were asked about their first-degree family networks and for every member of this network, whether they lived within or outside the village. The average ultra poor household in the sample had roughly 9 members in their first degree family network at baseline, and on average 32% of their family network was located within the village. In Table 33, columns (1)-(2) I control for the size of the family network of the respondent at baseline. The dependent variable is the value of transfers from within the village in column (1) and a dummy variable for whether any transfers were received from within the village in column (2). As expected, the coefficient for "size of the family network" is positive and significant in both regressions, implying that those who had larger family networks at baseline received larger transfers and were more likely to receive transfers at baseline. Yet the coefficients for food insecurity and its interaction with "treat × post" are practically unaffected. Similarly, when I include "the proportion of family that lives within the village" in the regression (columns (3) and (4)), the results are unchanged. This partly rules out that baseline food insecurity might be acting as a proxy for heterogeneity in social networks of the poor in the previous analysis.

An alternative explanation for why the crowding out effect is likely to be heterogenous depending on baseline vulnerability of the targeted households is if the effects of the program on the income and welfare of the beneficiaries is heterogenous along the same dimension. If, for example, households who had greater food insecurity at baseline also benefit less from the program in terms of improved income (due to, for example, the link between malnutrition and productivity), and to the extent that this is observed by the rest of the village, informal transfers from the villagers to these households may be crowded out less by the program relative to the other ultra poor. In order to test whether this might be the case, I estimate the same model as in (36), but replace the dependent variable with the income of the main female respondent (i.e. the targeted woman within the ultra poor households who is charge of the asset transferred) from her business activities. The result is given in column (5) of Table 33. The diff-in-diff estimate for the income of the main female respondent is large, positive and significant, but the triple interaction term is 3.5 and insignificant at conventional levels – which implies that the program does not have a differential effect on the income of the female respondent with respect to the food insecurity of the household at baseline. Similarly, column (6) provides the result of carrying out the same exercise for the per capita expenditure in ultra poor households – there is no evidence to suggest that the effect of the program on the expenditure within ultra poor households is heterogenous with respect to their food insecurity at baseline. This rules out the argument that previous

findings were driven by heterogeneity of the effects of the program on the income and welfare of targeted poor households.

3.6 Conclusion

This paper tests whether a large scale asset transfer and training program targeted to the poorest in rural Bangladesh – the ultra poor program – crowds out informal transfers received by the targeted poor households. As such, it provides the first piece of experimental evidence on the effects of an *unconditional* formal transfer on the informal transfer arrangements of the poor. I find that the targeted poor are less likely to receive informal transfers, and conditional on receiving any transfer, the value of transfers they receive is reduced. This crowding out effect is driven by crowding out of transfers they receive from within their village. Moreover, the crowding out of within-village transfers is highly heterogenous by the food security of targeted poor households at baseline – those who faced greater food insecurity at baseline experience less crowding out relative to others. I provide evidence that suggests that this is likely to be driven by an innovative component of the program that aims explicitly to reduce the crowding out effect, and to induce the elite within the treated villages to target their transfers to those in greatest need. In fact, the elites who are members of these committees are more likely to assist the poor who are more vulnerable.

Taken all together, these results have important policy implications. They imply that the effects of large-scale poverty alleviation programs, such as the ultra poor program, are likely to be partly undone by the crowding out effect on informal transfers received by the targeted households. It is important to take this into account while evaluating the effects of public transfer programs. Incorporating components that aim to minimize this crowding-out effect, like the committees that are part of the ultra poor program, to reduce this effect, in particular for the poor who are more vulnerable thus are in greatest need of assistance from their social networks, is an important aspect to consider while designing such programs.

Table 26: Descriptive Statistics on Informal Transfers at Baseline

	Ultra poor in treatment (1)	Ultra poor in control (2)	Normalized difference (3)
received any informal transfer	0.196	0.249	0.09
	(0.397)	(0.433)	
received any informal transfer within village	0.084	0.101	0.004
	(0.277)	(0.301)	
received any informal transfer outside village	0.133	0.178	0.09
	(0.340)	(0.383)	
ratio of within village informal tr's to total	0.373	0.346	-0.04
	(0.461)	(0.452)	
value of informal transfers	212	228	0.08
	(1022)	(990)	
value of informal transfers, conditional on having received any	1079	916	-0.06
	(2096)	(1818)	
total hh expenditure	11257	10357	-0.08
	(7043)	(8887)	
conditional on having received an informal transfer	10490	9370	-0.12
	(6990)	(6466)	
hh couldn't eat enough more than once a week	0.321	0.364	0.06
	(0.467)	(0.481)	
N	3894	2876	6770

Notes: Observations are from the baseline survey. Sample is restricted to ultra poor households that are selected by the program. Column (1) provides summary statistics for the ultra poor in treatment branches, Column (2) for those in control branches and column (3) presented the normalized difference between the two. The normalized differences between the treatment and control observations are calculated based on Imbens and Wooldridge (2009) where the normalized difference is given by $\Delta_X = \frac{\overline{X_1}-\overline{X_0}}{\sqrt{S_0^2+S_1^2}}$ where $\overline{X_1}$ is the sample mean of treatment observations, $\overline{X_0}$ is the sample mean of control observations, S_0^2 is the sample variance of control observations and S_1^2 is the sample variance of treatment observations. "hh couldn't eat enough more than once a week" is based on the answer, at baseline, to the question "During the past month how many times has it happened that your household couldn't eat enough?". It is coded as: [1] Never; [2] 1-3 times a month; [3] 1-2 times a week; [4] more than 3-4 times a week.

Table 27: Crowding Out of Informal Transfers for the Ultra Poor

	Extensive Margin: Whether received any informal transfer	Intensive Margin: Value of informal transfers received
	(1)	(2)
treat	-0.034**	-0.773**
	(0.016)	(0.346)
post	0.315***	4.984***
	(0.017)	(0.282)
treat × post	-0.095***	-0.841**
	(0.021)	(0.382)
N	13540	13540
Model	OLS	Tobit

Notes: Sample is restricted to ultra poor households selected by the program. *** stands for p-value < 0.01, ** stands for p-value < 0.05, * stands for p-value < 0.10. Standard errors are clustered at the village level. Dependent variable in column (1) is a dummy variable equal to 1 if the respondent's household received any transfers in cash or in kind during the last year. Dependent variable in column (2) is log total value of transfers received by the respondent's household during the last year. "treat" is a dummy variable equal to 1 if the observation belongs to a household in a treatment village. "post" is a dummy variable equal to 1 if the observation is from the followup survey. All regressions include the following controls: age of the respondent, literacy and numeracy of the respondent, gender of the household head, household size and religion.

Table 28: Crowding Out of Informal Transfers for the Ultra Poor

	tranfers from within village		transfers from outside village	
	extensive margin	intensive margin	extensive margin	intensive margin
	(1)	(2)	(3)	(4)
treat	0.000	-0.340	-0.036***	-1.185***
	(0.010)	(0.520)	(0.013)	(0.416)
post	0.209***	6.483***	0.188***	4.352***
	(0.014)	(0.444)	(0.015)	(0.358)
treat × post	-0.115***	-2.416***	-0.022	0.145
	(0.017)	(0.615)	(0.019)	(0.473)
N	13540	13540	13540	13540
Model	OLS	Tobit	OLS	Tobit

Notes: Sample is restricted to ultra poor households selected by the program. *** stands for p-value < 0.01, ** stands for p-value < 0.05, * stands for p-value < 0.10. Standard errors are clustered at the village level. Dependent variable in column (1) is a dummy variable equal to 1 if the respondent's household received any transfers in cash or in kind, from any household within the same village, during the last year. Dependent variable in column (2) is log total value of transfers received by the respondent's household from households within the same village during the last year. Dependent variable in column (3) is a dummy variable equal to 1 if the respondent's household received any transfers in cash or in kind, from any household outside the village, during the last year. Dependent variable in column (4) is log total value of transfers received by the respondent's household from households outside the village during the last year. "treat" is a dummy variable equal to 1 if the observation belongs to a household in a treatment village. "post" is a dummy variable equal to 1 if the observation is from the followup survey. All regressions include the following controls: age of the respondent, literacy and numeracy of the respondent, gender of the household head, household size and religion.

Table 29: Heterogenous Effects by Baseline Food Security of the Recipient

Panel A: Intensive Margin						
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Village	Non-village	All	Village	Non-village
model						
treatXpost	-1.386	-3.572**	0.353	-1.159**	-3.075***	0.074
	(0.947)	(1.556)	(1.236)	(0.457)	(0.745)	(0.598)
treatXpost x deficiency	0.250	0.498	-0.061	0.781	1.517	0.185
	(0.369)	(0.601)	(0.476)	(0.624)	(1.016)	(0.812)
marginal effect	-1.136	-3.074	0.292	-0.378	-1.559	0.260
	(0.626)	(1.031)	(0.818)	(0.531)	(0.848)	(0.655)
N	13536	13536	13536	13536	13536	13536
Model	Tobit	Tobit	Tobit	Tobit	Tobit	Tobit
Panel B: Extensive Margin						
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Village	Non-village	All	Village	Non-village
treatXpost	-0.130**	-0.134***	-0.009	-0.113***	-0.125***	-0.024
	(0.052)	(0.040)	(0.050)	(0.025)	(0.020)	(0.024)
treatXpost x deficiency	0.017	0.010	-0.004	0.047	0.032	0.007
	(0.021)	(0.016)	(0.019)	(0.035)	(0.028)	(0.032)
marginal effect	-0.113	-0.124	-0.013	-0.066	-0.093	-0.017
	(0.034)	(0.026)	(0.033)	(0.030)	(0.025)	(0.026)
N	13536	13536	13536	13536	13536	13536
Model	OLS	OLS	OLS	OLS	OLS	OLS

Notes: Sample is restricted to ultra poor households selected by the program. *** stands for p-value <0.01, ** stands for p-value < 0.05, * stands for p-value < 0.10. Standard errors are clustered at the village level. In Panel A, the dependent variables are log total value of transfers received in columns (1) and (4), transfers from within the village in columns (2) and (5) and transfers from outside the village in columns (3) and (6). In Panel B, the dependent variables are dummy variables equal to 1 if the household received any transfers in columns (1) and (4), any transfers from within the village in columns (2) and (5) and any transfers from outside the village in columns (3) and (6). "treat" is a dummy variable equal to 1 if the observation belongs to a household in a treatment village. "post" is a dummy variable equal to 1 if the observation is from the followup survey. In columns (1)-(3) "food deficiency" is the answer, at baseline, to the question "During the past month how many times has it happened that your household couldn't eat enough?". It is coded as: [1] Never; [2] 1-3 times a month; [3] 1-2 times a week; [4] more than 3-4 times a week. In Columns (4)-(6) "food deficiency" is a dummy variable equal to 1 if the respondent reported at baseline survey that the household couldn't eat enough more often than once a week during the past month. All regressions include the following controls: age of the respondent, literacy and numeracy of the respondent, gender of the household head, household size and religion.

Table 30: Descriptive Statistics on The Effects of the Committee

	(1) Pre	(2) Post	(3) Pre	(4) Post
received any help from members	0.097	0.434	0.060	0.088
	(0.296)	(0.496)	(0.237)	(0.284)
received transfer from members	0.056	0.262	0.031	0.042
	(0.230)	(0.440)	(0.175)	(0.200)
received help to access services	0.032	0.113	0.025	0.041
	(0.175)	(0.317)	(0.157)	(0.198)
received help to resolve dispute	0.013	0.060	0.007	0.018
	(0.115)	(0.238)	(0.086)	(0.133)
received latrine/tubewell	0.002	0.101	0.001	0.003
	(0.039)	(0.302)	(0.032)	(0.055)
any family member on committee	0.108	0.108	0.073	0.073
	(0.310)	(0.310)	(0.261)	(0.261)
N	3894	3894	4002	4002
Sample	ultra poor	ultra poor	other poor	other poor

Notes: Sample is restricted to treatment villages. Columns 1 and 2 provide descriptive statistics for the sample of ultra poor households in treatment villages at baseline and followup respectively. Columns 3 and 4 provide descriptive statistics for the sample of other poor households in treatment villages at baseline and followup respectively. "Received any help from members" is a dummy =1 if the respondent reports having received any assistance from any committee member in the past 2 years. "Received transfer from members", "received help to access services", "received help to resolve dispute", "received latrine/tubewell" are all dummy variables =1 if the respondent reports having received assistance in the forms of transfers in cash/kind; help with receiving government benefits, education or health services; help with resolving disputes or obtaining a latrine/tubewell respectively. "Any family member on the committee" is a dummy variable =1 if the respondent has any extended family members on the committee. All regressions include the following controls: age of the respondent, literacy and numeracy of the respondent, gender of the household head, household size and religion.

Table 31: Targeting of Assistance by Committee Members to the Poor

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
post	0.335***	0.259***	0.264***	0.273***	0.028***	0.029***	0.032***	0.028***
	(0.018)	(0.039)	(0.040)	(0.040)	(0.005)	(0.011)	(0.011)	(0.010)
food deficiency		-0.001	-0.001	-0.001		0.010**	0.010*	0.010**
		(0.008)	(0.008)	(0.008)		(0.005)	(0.005)	(0.005)
post × deficiency		0.033**	0.032**	0.032**		-0.000	-0.001	-0.001
		(0.016)	(0.016)	(0.016)		(0.005)	(0.005)	(0.005)
wealth			-0.000	-0.000			-0.000	-0.000
			(0.000)	(0.000)			(0.000)	(0.000)
post × wealth			-0.000	-0.000			-0.000***	-0.000***
			(0.000)	(0.000)			(0.000)	(0.000)
family on committee				0.178***				0.126***
				(0.034)				(0.029)
post × family				-0.041				0.061**
				(0.037)				(0.027)
N	7788	7784	7784	7784	8004	7996	7996	7996
Sample	Ultra poor	Ultra poor	Ultra poor	Ultra poor	Other poor	Other poor	Other poor	Other poor
Model	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS

Notes: In column (1)-(4) sample is restricted to ultra poor households selected by the program, in treatment villages. In columns (5)-(8), sample is restricted to other poor households in treatment villages. *** stands for p-value <0.01, ** stands for p-value < 0.05, * stands for p-value < 0.10. Standard errors are clustered at the village level. Dependent variable in columns (1)-(8) is a dummy variable equal to 1 if the respondent's household received any assistance from any committee member in the last two years. 'post" is a dummy variable equal to 1 if the observation is from the followup survey. "food deficiency" is the answer, at baseline, to the question "During the past month how many times has it happened that your household couldn't eat enough?". It is coded as: [1] Never; [2] 1-3 times a month; [3] 1-2 times a week; [4] more than 3-4 times a week. "severe food deficiency" is a dummy variable equal to 1 if the respondent reported at baseline survey that the household couldn't eat enough more often than once a week during the past month. "wealth" is log value of assets owned by the household, as measured at baseline, in Takas. "food deficiency" is a dummy =1 if the household experienced any food deficiency at baseline. "family" is a dummy =1 if the respondent has family links to anyone on the committee. All regressions include the following controls: age of the respondent, literacy and numeracy of the respondent, gender of the household head, household size and religion.

Table 32: Types of Assistance from Committee Members to the Ultra Poor

	Transfers		Access to services		Resolving disputes		Sanitation	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
post	0.152***	0.159***	0.043**	0.037**	0.054***	0.055***	0.092***	0.097***
	(0.040)	(0.040)	(0.019)	(0.018)	(0.014)	(0.015)	(0.022)	(0.022)
food deficiency	0.004	0.004	-0.003	-0.003	-0.002	-0.002	0.001	0.001
	(0.007)	(0.007)	(0.004)	(0.004)	(0.003)	(0.003)	(0.001)	(0.001)
post × food deficiency	0.023	0.023	0.016**	0.016**	-0.003	-0.004	0.003	0.003
	(0.016)	(0.016)	(0.008)	(0.008)	(0.005)	(0.005)	(0.009)	(0.009)
wealth		-0.000		-0.000		-0.000		-0.000**
		(0.000)		(0.000)		(0.000)		(0.000)
post × wealth		0.000		0.000		-0.000***		-0.000
		(0.000)		(0.000)		(0.000)		(0.000)
family on committee		0.141***		0.051***		0.034**		0.006
		(0.028)		(0.017)		(0.016)		(0.006)
post × family		-0.039		0.056**		0.025		-0.019
		(0.029)		(0.025)		(0.023)		(0.019)
N	7784	7784	7784	7784	7784	7784	7784	7784

Notes: Sample is restricted to ultra poor households selected by the program, in treatment villages. *** stands for p-value <0.01, ** stands for p-value < 0.05, * stands for p-value < 0.10. Standard errors are clustered at the village level. Dependent variable in columns (1)-(2) is a dummy variable equal to 1 if the respondent's household received any transfers in cash/kind from any committee member in the last two years. Dependent variable in columns (3)-(4) is a dummy variable equal to 1 if the respondent's household received any assistance to access services (e.g. obtaining a poverty card) from any committee member in the last two years. Dependent variable in columns (5)-(6) is a dummy variable equal to 1 if the respondent's household received any assistance from any committee member to resolve disputes in the last two years. Dependent variable in columns (7)-(8) is a dummy variable equal to 1 if the respondent's household received any sanitary latrine and/or tubewell from any committee member in the last two years. "post" is a dummy variable equal to 1 if the observation is from the followup survey. "food deficiency" is the answer, at baseline, to the question "During the past month how many times has it happened that your household couldn't eat enough?". It is coded as: [1] Never; [2] 1-3 times a month; [3] 1-2 times a week; [4] more than 3-4 times a week. "severe food deficiency" is a dummy variable equal to 1 if the respondent reported at baseline survey that the household couldn't eat enough more often than once a week during the past month. "wealth" is log value of assets owned by the household, as measured at baseline, in Takas. "family" is a dummy =1 if the respondent has family links to anyone on the committee. All regressions include the following controls: age of the respondent, literacy and numeracy of the respondent, gender of the household head, household size and religion.

TABLE 33: ROBUSTNESS CHECKS

	(1)	(2)	(3)	(4)	(5)	(6)
	tranfers from within village				income	total pce
	intensive	extensive	intensive	extensive		
treat × post	-4.072***	-0.160***	-3.602**	-0.144***	2324.020***	329.897***
	(1.261)	(0.037)	(1.576)	(0.043)	(335.625)	(96.466)
treat × post × severe food deficiency	2.226**	0.052*	2.362**	0.054*	3.527	-81.571
	(1.009)	(0.028)	(1.009)	(0.028)	(501.369)	(159.932)
size of family network	0.149*	0.004**	0.173**	0.004**		
	(0.078)	(0.002)	(0.082)	(0.002)		
treat × post × size of family network	0.109	0.004	0.121	0.004		
	(0.123)	(0.004)	(0.127)	(0.004)		
proportion of family within village			1.951	0.029		
			(1.394)	(0.029)		
treat × post × proportion of family within village			-1.899	-0.070		
			(1.980)	(0.058)		
N	13536	13536	13482	13482	13533	12570
Model	Tobit	OLS	Tobit	OLS	OLS	OLS

Notes: Sample is restricted to ultra poor households selected by the program. *** stands for p-value <0.01, ** stands for p-value < 0.05, * stands for p-value < 0.10. Standard errors are clustered at the village level. Dependent variables are: in column (1) and (3) log total value of transfers received from within the village, in columns (2) and (4) dummy variable =1 if received any transfer from within the village, in column (5) total income of the main female respondent from her business activities in the last one year, in column (6) per capita expenditure of the household where expenditure on non-food items is recorded for the last year while expenditure for non-food items is recorded for the last 3 days and imputed for one year. "treat" is a dummy variable equal to 1 if the observation belongs to a household in a treatment village. "post" is a dummy variable equal to 1 if the observation is from the followup survey. "severe food deficiency" is a dummy variable equal to 1 if the respondent reported at baseline survey that the household couldn't eat enough more often than once a week during the past month. All regressions include the following controls: age of the respondent, literacy and numeracy of the respondent, gender of the household head, household size and religion.

3.A Appendix

3.A.1 Appendix Tables

Table 34 in this Appendix provides summary statistics on the characteristics of the elite members of the committee, relative to the ultra poor and the other poor. Column (1) gives descriptive statistics of the elite members of the committees. The elite who are on the committee are wealthy individuals who are typically landowners (90% of them own some land) or run a business (96% of them have their own business). Among those who have their own business, the majority work in land cultivation (84% of the member elite cultivate land, 70% rear livestock or poultry while 23% run a small non-agricultural business). Many of the member elites are employers: 77% of the member elite employ at least one person for a business activity (excluding household servants). The average elite member on the committee employs 25 workers. Only 24% of the member elite work for a wage and majority of these (59%) are teachers. Nearly all (97%) of the member elite are literate and the average elite on the committee has 9 years of schooling.

The characteristics of the member elite are, perhaps not surprisingly, in stark contrast to the characteristics of the households at the bottom of the wealth distribution in these villages, and among them the ultra poor are relatively poorer than the other poor in many dimensions. Only 6% of the ultra poor and 11% of other poor own any land, yet the household head in 49% and 58% of the two groups respectively are self-employed. Majority of the poor who are self-employed work in animal husbandry (27% of the ultra poor and 31% of the other poor). A very small proportion (1-2%) of the heads of poor households have a non-agricultural business or employ a non-household-member. On the other hand, the household head in the majority of the poor households work in wage-employment: 72% in ultra poor and 66% in other poor households. The type of wage-employment for the poor is very different from the type of wage-work that the elite does - the majority of the poor is either employed as day-laborers in agriculture or in construction sites (56% of both ultra poor and other poor households) or as household servants for the wealthy (19% and 10% of the household heads in ultra poor and other poor households respectively). Moreover, the poor households have much lower levels of human capital than the elite: only 8% of the household heads in ultra poor and 16% of the other poor households know how to read. The head of the average poor household has been in school for less than a year.

Table 34: Summary Statistics on Characteristics of Committee Members

	member elite	other poor	ultra poor
	(1)	(2)	(3)
wealth	1217998.0	12298.8	4637.0
	(1933896.3)	(63002.9)	(18164.0)
owns land	0.891	0.110	0.060
	(0.312)	(0.313)	(0.237)
self-employed	0.957	0.583	0.486
	(0.204)	(0.493)	(0.500)
land cultivation	0.841	0.143	0.058
	(0.366)	(0.350)	(0.235)
animal husbandry	0.692	0.310	0.269
	(0.462)	(0.462)	(0.444)
runs small non-agricultural business	0.231	0.018	0.011
	(0.422)	(0.134)	(0.103)
employs people	0.767	0.032	0.015
	(0.423)	(0.177)	(0.121)
no of workers	24.862	0.110	0.049
	(49.059)	(0.768)	(0.519)
works for a wage	0.239	0.658	0.720
	(0.426)	(0.475)	(0.449)
works for government/private company	0.049	0.005	0.003
	(0.215)	(0.069)	(0.058)
teacher	0.140	0.002	0.001
	(0.348)	(0.046)	(0.029)
day-laborer	0.041	0.558	0.565
	(0.199)	(0.497)	(0.496)
maid	0.002	0.096	0.189
	(0.046)	(0.295)	(0.391)
head literate	0.965	0.164	0.084
	(0.183)	(0.371)	(0.277)
head's schooling (years)	8.758	1.135	0.614
	(2.997)	(2.372)	(1.760)
N	947	7877	6437

Notes: Column (1) provides summary statistics for the elite members of the committees. Column (2) provides summary statistics for the poor households that were selected as poor by the community but not by the program. Column (3) provides summary statistics for the poor households that were selected by the program. Self-employed is a dummy =1 if the household head is self-employed for any business activity. All occupational variables are with respect to the occupation(s) of the household head where they were asked to report all business activities they were involved in.

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