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LABOR-TYING AND POVERTY IN A RURAL ECONOMY: EVIDENCE FROM BANGLADESH*

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Abstract

This paper shows labor-tying can be an informal insurance mechanism for the poorest workers in a rural economy. Exploiting the randomized roll-out of an asset and complementary skills transfer program targeted to poor women in rural communities, I show that an exogenous increase in self-employment opportunities of eligible women reduces their likelihood to receive transfers and loans from their employers and increases the volatility of their earnings from wage-employment. On the other hand, they become more likely to engage in reciprocated transfer arrangements with other, wealthier households in the community. The increase in poor women's outside options leads to an increase in the equilibrium wage for women. The findings demonstrate that anti-poverty programs that improve self-employment opportunities of poor women may reduce their participation in labor-tying and have general equilibrium effects on the labor market.

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1 Introduction

In rural parts of developing countries, individuals are faced with substantial risks and have limited formal insurance opportunities. As a result, they often rely on informal insurance mechanisms to deal with shocks to their earnings. One mechanism that has been highlighted in the literature is risk-sharing through the exchange of transfers and gifts (Udry (1994), Fafchamps and Lund (2003)). While risk-sharing mechanisms through reciprocated transfers are widespread, their scope is often limited to close friendship and family networks (Ambrus et al (2014), Angelucci et al (2014)). An alternative source of informal insurance for poor individuals may be their employers. The idea that a risk-neutral employer may provide a risk-averse worker with insurance against income fluctuations is long-established (Knight (1921), Baily (1974), Azariadis (1975)) and recent studies provide empirical evidence from developed economies (Guiso et al (2005), Lagakos and Ordoñez (2011)). Yet, we have limited evidence on whether workers in rural labor markets receive insurance from their employers and which workers are likely to do so.

The literature on labor-tying in rural labor markets has studied how a tied labor contract may provide a smoother income to the workers compared to casual labor, while lowering employer's labor costs. The exact definition of what constitutes a tied as opposed to a casual labor contract varies across different settings and models of labor-tying. In this paper, I focus on labor-tying arrangements as studied by Bardhan (1983) and Mukherjee and Ray (1995) where tied workers need not carry out any duties that are different to the ones performed by casual workers[1]. These types of tied labor contracts are often thought to involve a patron-client relationship where the employer helps the worker out in times of need (Mukherjee and Ray (1995)), often through the provision of consumption loans or transfers[2]. They are likely to be particularly widespread if consumption loans are hard to get (Caselli (1997)).

The first objective of this paper is to assess if labor-tying is an empirically relevant mechanism through which poor workers in rural labor markets may try to smooth their earnings. Previous studies have noted a secular decline in the incidence of labor-tying in rural labor markets in India (Mukherjee and Ray (1995), Caselli (1997)). While the categorical differentiation between casual versus attached or semi-attached workers[3] may have ceased to exist in most parts of the world, this doesn't necessarily imply that workers do not turn to their employers for insurance, particularly through asking for help (through transfers or loans) in bad times. Using original survey data on social networks in more than 1,000 rural communities in Bangladesh, I show that 16 percent of

[1] In contrast, Eswaran and Kotwal (1985) model tied (or attached) workers as those who perform special tasks that are difficult to monitor while casual workers perform mundane, easy-to-monitor tasks.

[2] 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.

[3] Classification of agricultural workers into categories of "attached" and "casual" and sometimes "semi-attached" dates back to 1950s. The distinction was first adopted in the First (1950-51) and the Second (1956-57) Agricultural Labor Enquiries. Yet the distinction between attached and casual workers 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).

female workers reported their employers as a source of transfers or loans. The corresponding figure is lower for male workers (8 to 6 percent, depending on the wealth of the worker). Overall, 10% of poor households in the sample reported that at least one of their members received transfers or loans from an employer. While this is correlated with lower wages for both male and female workers (hourly wage is 3% lower for both), it is also associated with a smoother wage earnings profile, particularly for women.

The second objective of the paper is to identify the causal effect of an improvement in poor workers' outside options on their participation in labor-tying. For this, I exploit the randomized roll-out of an asset-transfer and training program[4] targeted to the poorest women in rural communities. Before this exogenous shock, members of eligible households rely primarily on wage-employment and on the transfers they receive from the rest of the community. The program identifies poorest women in the community and provides them with productive assets (mainly livestock), training and supervision to ensure that they become able to generate income from self-employment. By doing so, the program potentially improves the beneficiaries' outside options to wage-labor. To identify the effects of the program, I exploit its randomized roll-out which was conducted between 2007 and 2011. The randomization was done at the NGO's branch office level which corresponds to a large cluster that contains several communities. Prior to the randomization, eligible households were identified in evvery community that was part of the evaluation. Hence the sample includes both eligible and ineligible households from treatment and control areas. The sampled households were surveyed at baseline (2007), midline (2009) and endline (2011). In every household, the main respondent was the primary female of the household. In addition, when the head of the household was a man, he was surveyed separately about his labor supply and earnings.

Standard models of labor-tying predict that an improvement in the outside option of a worker should lead to a reduction in her labor supply into both tied and casual labor contracts. Moreover, if a sufficiently large number of workers' labor supply is affected, the equilibrium in the labor market may change. The fall in the supply of tied and casual workers may increase the wage in both types of labor contracts[5]. Such an increase in wages would partly offset the increase in the outside option of the worker, making the overall impact of the program ambigous. Empirically, these effects are typically hard to disentagle. The cluster randomization of the program and the fact that both eligible and ineligible households were surveyed, allows me to test for not only the effects of the program on the beneficiaries, but also the spillover effects on the rest of the community. In particular, the unit of randomization was large enough to study general equilibrium effects through the labor market. Therefore, an inherent part of the empirical analysis of the effects of the program will be to test for both its direct effects on targeted workers, as well as the general

[4] "Targeting the Ultra Poor" (henceforth TUP) program is implemented by the Bangladeshi NGO BRAC. The TUP program was pioneered by BRAC in 2002 in Bangladesh and today it is being replicated in different countries. The details of the TUP program are explained in Section 2.

[5] In the Appendix of the paper, I formally show this for the Bardhan (1983) model of labor-tying.

equilibrium effects on the equilibrium in the labor market.

To identify the program's effects on targeted workers' labor supply and their participation in labor-tying, I compare the changes in eligible women's outcomes in treatment communities to the changes in eligible women's outcomes in control communities. Three findings are of note: First, the program led to a fall in eligible women's supply of labor into wage-employment, both on the extensive and the intensive margins. Second, eligible women were less likely to receive transfers or loans from their employers. Third, their wage earnings became more volatile. Taken together, the last two findings imply that the intervention led to a fall in labor-tying among eligible women. Although the program was targeted primarily to women, it may have had intra-household spillover effects. In particular, I test whether the male heads of eligible households were indirectly affected by the program. I find that the treatment did not have a significant impact on their labor supply, but it decreased their propensity to receive transfers or loans from their employers and increased the volatility of their wage earnings.

These findings suggest that the program decreased labor-tying in eligible women's households. A related question is whether it increased their participation in alternative insurance mechanisms. In particular, reciprocal transfer and credit arrangements between households have been shown to play an important role in similar settings (Udry (1994), Fafchamps and Lund (2003)). In the survey, the respondents were asked to report households they would typically receive transfers from when in need. They were also asked to report households that would turn to the respondent's household in a similar situation. Based on these, I construct measures of reciprocity of eligible households' food transfer connections. I find that treated women were more likely to report the same households as both sources and targets of transfers. In other words, the program increased the degree of reciprocity in food transfers received/given by eligible women's households. Moreover, eligible women in treated communities were more likely to make transfers to wealthier households in their communities. This suggests that as poor individuals in a rural economy get wealthier (in this case by having better opportunities in self-employment) they may move away from obtaining informal insurance from their employers, and increase their participation in exchange of reciprocated transfers with other households. Moreover, the increase in the wealth of eligible households' transfer connections imply that for such reciprocal transfer arrangements, households are matched assortatively according to their wealth.

To test if the program had any general equilibrium effects on the labor market, I compare the terms of labor contracts for *ineligible* individuals in treatment villages to those for *ineligible* individuals in control villages. I find that the program increased the hourly wage of ineligible women in treated communities by 7%, while it did not have a significant effect on men's wages. Further examination of this effect suggests that, in line with the theoretical predictions, the returns to both tied and casual female workers increased. Moreover, I show that the positive effect in female workers' wages was not only driven by ineligible women who had social connections to eligible households. Both women who had social connections to eligible women and those who did

not experienced an increase in their wages. This is in line with a change in the equilibrium wage rate that affected all female workers.

The paper contributes to the literature on informal insurance in developing countries by demonstrating that labor-tying can be a mechanism through which poor individuals smooth their consumption. Previous empirical work on labor-tying has studied some of the key properties of this institution (Bardhan and Rudra (1978, 1981), Richards (1979), Anderson(1990)), but these studies are mainly descriptive and do not provide evidence on the causal determinants of participation in labor-tying. On the other hand, recent empirical studies on informal insurance have focused on alternative mechanisms of consumption-smoothing such as exchange of loans and transfers (Udry (1994), Fafchamps and Lund (2003)) or pre-cautionary savings (Paxson (1992), Rosenzweig and Wolpin (1993)). This paper contributes to the literature by showing that labor-tying may be another insurance mechanism in rural settings, particularly for individuals with limited outside options to wage-employment. If individuals have higher non-wage income (in this case by having better opportunities in self-employment) they may change the mechanisms through which they smooth their consumption: they engage less in labor-tying and more in reciprocal exchange of transfers with other (wealthier) households in their communities.

The paper is also related to a growing literature on the general equilibrium effects in labor markets related to anti-poverty programs and credit access[6]. Jayachandran (2005) shows that fluctuations in agricultural productivity in India are associated with changes in wages, particularly when credit marklets are thinner and migration costs are higher. Imbert and Papp (forthcoming) evaluate the general equilibrium effects of a large rural workfare program (the National Employment Guarantee Act) on the labor market in India. They find, through non-experimental methods, that the program is associated with a fall in the labor supply and an increase in the wages in the private sector. The current paper contributes to this literature by showing that an antipoverty program designed to improve self-employment opportunities of poor women had significant general equilibrium impacts on the rural labor market for women, but not for men. This highlights the high degree of segmentation, by gender, in the rural labor markets in Bangladesh.

The rest of the paper is organized as follows: section 2 describes the setting of the study and the program studied; section 3 discusses theoretical predictions on the effects of the program on labor-tying and the labor market equilibrium; section 4 describes the data and baseline characteristics of the sampled households; section 5 presents the findings on the effects of the intervention; section 6 concludes.

[6]The paper's methodology for identifying spillover effects on ineligible households is closely related to Angelucci and de Giorgi (2009). In order to test for spillover effects of a conditional cash transfer program in Mexico, they compare outcomes of ineligible households in treated communities to ineligible households in control communities.

2 Targeting the Ultra Poor (TUP) Program

The data used in this study comes from a data collection exercise implemented to evaluate the effects of BRAC's "Targeting the Ultra Poor" (TUP) program in Bangladesh[7]. TUP program aims to lift the poorest women out of poverty by improving their self-employment opportunities. In order to do so, the program combines a large asset transfer with asset-specific skills transfer to ensure that the beneficiaries are able to generate income from the assets that they receive. In addition, the program also provides a subsistence allowance to beneficiary women for the first 40 weeks after the livestock asset transfer. This is meant to alleviate any short-run fall in earnings due to occupational changes away from wage labor and into self-employment. This allowance runs out fifteen months before the beginning of the first follow-up survey and is therefore not part of the earnings measures reported below. Other components of the program include health support, training on legal, social and political rights, and a savings scheme[8].

To identify the poorest women who are eligible for the program, BRAC carried out a detailed procedure: First, prior to the asset transfer, the BRAC officers identied a community, or cluster of households that form a natural geographical unit, similar to a hamlet. These communities consisted of 387 individuals in 90 households on average. Then, within each community, BRAC officials carried out a participatory wealth ranking exercise during which the community members allocated every household into 5-6 wealth ranks[9]. The households assigned to the lowest two wealth ranks were visited by BRAC officials to verify pre-determined selection criteria into the program[10]. Roughly half of the households in the bottom wealth ranks were selected as eligible and were offered to participate in the TUP program.

As mentioned above, two key components of the program were assets and skills transfers. The assets transferred were different combinations of livestock[11]. Beneficiaries could choose between six different livestock packages containing either one or two animal types (e.g. only cows or a cow and five goats), and all packages were on average of similar value at TK9500 (USD 140). BRAC encouraged program recipients to commit to retain the asset for two years, although this commitment was not strictly enforceable. After two years, beneficiaries were under no obligation or no encouragement to retain the livestock asset.

[7]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.

[8]Further details on specific components of the TUP program can be found at http://tup.brac.net/

[9]The participatory wealth ranking exercise used by the TUP program is similar to community appraisal methods studied by Alatas et al (2012).

[10]There were three exclusion criteria, all of which were binding. Households who were borrowing from an NGO providing microfinance, who were recipients of any government benefits, who had no able-to-work adult female members were excluded from the program. To be selected a household had to satisfy three of the following five inclusion criteria: (i) total land owned including homestead is no more than 10 decimals; (ii) there is no adult male income earner in the household; (iii) adult women in the household work outside the homestead; (iv) school-going-aged children have to work; and (v) the household has no productive assets.

[11]The specific assets were chosen by the beneficiaries from a menu offered by BRAC. The original menu included assets related to various activities (such as livestock rearing, vegetable cultivation, setting up small retail shops, production of small crafts) but all eligible women in the sample opted for livestock rearing.

The skills transfer component of the program was intended to teach the beneficiaries skills that were complementary to the transferred livestock asset (such as maintaining livestock health; best-practices related to feeding the animals; insemination to produce offspring and milk; rearing calves; bringing outputs to market etc). The skills transfer was conducted through a combination of classroom training (at BRAC regional offices) with regular assistance by a livestock specialist and the program officers[12].

3 Conceptual Framework

In order to predict the way in which the TUP program may affect the participation of eligible women in labor-tying and casual labor contracts, this section uses the Bardhan (1983) model of labor-tying as a conceptual framework to guide the empirical analysis[13]. As described above, the main goal of the TUP program is to enable eligible women to run their own small-scale businesses. Prior to the program, these women typically worked on farms or households of others, as agricultural workers or maids. If the TUP program succeeds in its mission, it essentially improves the outside options of poorest women within the rural labor market relative to before, by providing them with self-employment opportunities. The Bardhan (1983) model provides a suitable framework to think about the effects of the program, as it allows for heterogeneity in workers' outside options, which is the key parameter that the program is likely to alter.

In the Bardhan (1983) model, there are two types of agents in the economy: a continuum of risk-averse workers who own no land and risk-neutral landowners who may employ workers for cultivation. Labor demand varies across the seasons and depending on weather. In particular, there are two stylized seasons of production in the model: a peak season where there is labor demand on landowners' farms, and a lean season where there is no labor demand. During the peak season, the amount of labor demanded by the landowners depends on the realization of weather shocks. If the realized weather conditions have been good, the landowners have higher labor demand, otherwise labor demand is low. During the lean season there is no labor demand.

Workers differ in terms of their outside options. In this paper, the outside options of workers is referred to as self-employment but in general it can be any employment opportunity outside of the agricultural market (e.g. a new factory opening nearby). In particular, workers' outside options are distributed according to a cumulative distribution function $G(.)$.

There are two types of labor contracts available. Tied labor contracts in which the landowners pay a constant wage z in both seasons; and casual labor contracts which are only available in the harvest season and if the realization of the weather shock is such that the landowners need to hire additional workers (i.e. their tied workers are not enough to meet the entire labor demand) at rate w. Tied contracts are made in the lean season (i.e. before the weather shock is realized).

[12] As part of this, every beneficiary was visited by a livestock specialist every one to two months for the first year, and by BRAC program officers weekly for the first two years of the program.

[13] The formal model and the comparative statics are presented in the Appendix.

The workers have two options: either they enter into a tied contract, which provides them with a constant income of z in both seasons, or they remain self-employed. If they choose the latter, then in the lean season, they receive their outside option y_i and in the harvest season, depending on the labor demand and the equilibrium wage rate w, they can either work under a casual contract or remain self-employed. The key incentive for risk-averse workers to enter a tied-labor contract in the model is to smooth their income across the two seasons. On the other hand, the risk-neutral landowners have an incentive to offer tied-labor contracts to guarantee the supply of cheap labor during the peak season and thus to maximize their expected profits.

In equilibrium, workers whose outside options are below a threshold $\widehat{y}$ opt for tied-labor, while workers whose outside options are above the threshold choose to remain out of labor-tying and work for the employer as a casual worker whenever the realized village market wage rate exceeds their outside option y_i. This (by construction) implies that casual workers will receive a higher wage rate on average.

Within this framework, one can think about the TUP program as a positive shock to the self-employment opportunities of eligible workers. In partial equilibrium (assuming that z; w and $\widehat{y}$ remain unaffected), the rise in the outside option of the worker implies that her labor supply into wage work may be affected in two different ways: First, if the worker was previously 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 the utility that a casual wage contract (which would depend on the realized equilibrium wage w in that harvest season) would provide. Second, if the program corresponds to a large enough shock that moves the worker's outside option above the utility that a casual wage contract would provide, the worker may choose to remain self-employed and not enter into any wage-work. This would correspond to a fall in the overall labor supply of the worker for wage labor.

The model also yields general equilibrium predictions. In particular, if the program is a large enough shock to the distribution of workers' outside options in the economy, then it may lead to a change in the equilibrium wages, z and w, and the threshold $\widehat{y}$. To analyze this, one can think about the program as a second[14] order stochastic shift in the distribution of outside options $G(.)$, moving some (but not all) workers' outside options up, relative to the baseline distribution. If this shift is large enough to reduce the supply of tied and casual workers, then both z and w will increase in equilibrium. Moreover, the threshold level $\widehat{y}$ will increase, pushing the outside option level required to stay out of labor-tying higher. As such, in general equilibrium, the effect of the program on the labor supply of eligible workers or their participation in labor-tying is ambigous, depending on which effect dominates.

To summarize, the model yields the following predictions with respect to the partial and general

[14]The reason why the program is modelled as a second-order stochastic shift rather than a first-order one is because it affects the outside options of some, and in particular worse off, workers and not all of them. A first-order shift would require an overall improvement in all workers' outside options, which is not the case for the TUP program as it's targeted only to women at the bottom of the wealth distribution.

equilibrium effects of an exogenous shock to the outside options of poorest workers in the economy:

1. In partial equilibrium (if there is no effect on the terms of tied or casual labor contracts)
   (a) Treated workers will be less likely to be working for a wage.
   (b) 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, if the program corresponds to a large enough shift in the distribution of workers' outside options, wages for both tied and casual workers will increase. Moreover, the threshold level of the outside option below which workers enter labor-tying will increase.

An implication 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 is ambiguous as the direct effect on their outside option and the general equilibrium effects through the labor market have opposing effects. Which of these two effects dominates is ultimately an empirical question.

4 Data Description

In order to evaluate the TUP program in Bangladesh, the timing of the program's roll-out was randomized at the implementing NGO's branch office level. A branch office covers a large area with a radius of approximately four kilometers. The TUP 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 (by BRAC program officers) in all of these branches in the same way. Following the identification of potential beneficiary households, 20 branch offices were randomly selected to receive the program in 2007, the rest in 2011. The randomization was stratified at the subdistrict level whereby, within each subdistrict, one branch was randomly allocated to treatment and one to the control group. All of the selected communities in treatment branches were treated in 2007 while the control communities were not treated until after the endline survey in 2011.

In every community 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 at baseline lived in a sampled community. This is essential for the empirical analysis, as it allows me to identify the baseline characteristics of every household in the community that the respondent's household interacts with.

Following the census of all households in the village, a detailed household questionnaire was carried out on a smaller sample that included all poor households and a random sample of the rest of the village[15]. Households in this sample were surveyed at baseline (between April and December 2007), at midline (2009) and endline (2011). The survey questionnaire measured a

[15]At baseline, the household survey sample contained 7953 eligible poor households in 1409 communities in 40

rich set of individual outcomes, including occupational choices, labor supply, income, social and economic networks of the household. The main survey modules were directed towards the main female in the household, 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.

To capture the social and economic networks of the household, respondents were asked to list households they interacted with for each of the surveyed activities. For example, in the business activities module, the respondent listed all the households she/he worked 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 analysis, I will be considering the effects of the program from the workers' perspective.

4.1 Baseline Characteristics

Table 1 reports baseline descriptive statistics on key characteristics and relevant outcomes of households in the sample. The table is organized such that columns (1) and (2) provide statistics for poor households that were selected to be beneficiaries of the program (henceforth "eligible" households) in treatment and control communities respectively, while columns (5) and (6) do the same for households not selected by the program ("ineligible" households) in treatment and control communities respectively. The rest of the table provides tests for differences between treatment and control observations.

The first panel on Table 1 provides descriptive statistics on some key characteristics of eligible and ineligible households at baseline. As expected, eligible households fair poorly in terms of both physical and human capital, compared to ineligible households. For example, the first row of the table shows the average wealth (defined as total value of household assets including land, livestock, other productive assets and household durables) of eligible and ineligible households from treatment and control communities. The assets owned by the average eligible household in treatment (control) communities was worth TK5,373 (TK6,571) at baseline, while the corresponding figure was TK183,663 (TK184,063) for the ineligible households. The rest of Panel A shows that eligible poor households also had lower human capital (as proxied by the primary female respondent's

BRAC branches, and an additional 19,012 households from all other wealth classes within the same communities. Over the four years from baseline to endline, 13% of eligible poor households and 15% of ineligible households attrited from the original sample. Table A1 estimates the probability of not attriting as a function of treatment status and being in wage-labor or receiving transfers or loans from an employer at baseline (the main outcomes of interest) for primary female and household head respondents and for the sample of eligible and ineligible households. Two findings are of note. First, attrition rates are the same in treatment and control communities for both respondents from eligible and ineligible households. Second, attrition is not correlated with receieving transfers/loans from an employer at baseline, nor is there differential attrition by being in wage-employment between treatment an control communities. Therefore, to ease comparability across different specifications, I restrict the sample to households that appear in all three waves throughout. The working sample thus contains 6698 eligible households and 16,245 households from other wealth classes.

literacy) and were smaller (had fewer members) compared to ineligible households.

The rest of Table 1 provides baseline descriptive statistics on the key outcome variables. Panel B does so for women's labor market outcomes. Three points are of note: First, the proportion of eligible women who were engaged in wage-labor (i.e. working for an employer) was 52% (58%) in treatment (control) communities at baseline. The corresponding rate was 20% (22%) for ineligible households in treatment (control) communities. Similarly, hours spent in wage-labor was lower among female respondents from ineligible households compared to those from eligible ones. This highlights the fact that in this setting, being involved in wage-labor is associated with lower socio-economic status, particularly for women. In fact, wealthier women are more likely to specialize in self-employment or household work compared to poor women (Bandiera *et al* (2013)). Second, the proportion of women who reported that they received transfers from or had outstanding loans to their employers was 8%. This implies that, conditional on being in wage-labor, about 15% of eligible poor women reported their employers as a source of transfers or loans[16]. Third, the average wage rate for female workers is low (only TK6 per hour) and wage earnings are highly volatile. The range of monthly wage earnings (calculated as the difference between the monthly income from wage-labor in the lowest and the highest wage-income months) is about TK1,200. This demonstrates the high seasonality in the availability of wage-labor opportunities and the corresponding earnings from wage-labor in rural Bangladesh. To further illustrate this point, Figure 1 plots average monthly earnings from wage-labor, for male and female workers separately. Across one year, monthly wage earnings for the average worker (both male and female) is highly volatile. Particularly severe are the months during the lean season, often associated with high food insecurity and hunger – know localy as "monga" – among the poor households with limited outside options to wage labor and limited insurance opportunities (Bryan *et al* (2014)).

Descriptive statistics on men's labor outcomes[17] are presented in Panel C of Table 1. First, participation of men in wage-labor is higher than for women. As the first row of Panels C shows, within eligible households, 62% of men were engaged in wage-labor at baseline. The rate is lower among ineligible households (45%) but still much higher than women. Second, only 5% of men in eligible and 2% of men in ineligible households reported receiving transfers or having outstanding loans from their employers. This implies that among the male workers, employers are much less likely to be sources of transfers or loans, relative to female workers. Finally, the rest of Panel C shows that while the hourly wage rate for men is much higher (almost double) than it is for

[16]Since only employers within the same village were included in the network mapping, the identities of the employers outside the village are not observed. I assume that no employer outside the village is a source of transfer or loans. This implies that the rate of labor-tying reported here is potentially a lower bound for the true rate of labor-tying. One the other hand, since 96% of food transfer sources are within the village, the treatment of outside-village labor relationships as non-transfer relations seems like a reasonable assumption.

[17]Men's outcomes refer to the outcomes for the male head of the surveyed households. I focus on the labor market outcomes for household heads only, as for other household members the identity of their employer(s) was not recorded in the survey. 59% of eligible poor households had male household heads, while the corresponding rate is 88% for the ineligible households. 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.

women, wage earnings are almost as volatile throughout the year. Monthly wage earnings range from TK450 in the worst month (in terms of wage earnings) to TK2600 in the best.

The last panel (Panel D) of Table 1 reports statistics on informal insurance mechanisms of respondents' households at baseline. In particular, food transfers are likely to be an important source of insurance against consumption risk, especially for poor families. These in-kind transfers are typically smaller than cash transfers, but more frequent. At baseline, 92% of eligible poor women reported that when in need, their household receives food transfers from other households. As one would expect, the proportion of respondents that reported receiving food transfers from others was lower among ineligible households. Nevertheless it remains quite high (around 80%). This implies that informal support networks are important in these communities and even the wealthiest households rely, at times, on informal transfers from others when they are in need[18]. The next row shows the proportion of respondents that reported ever giving out food transfers. Only 46% (41%) of eligible poor women in treatment (control) communities reported ever giving out food transfers, implying that more than half of their food borrowing links were not reciprocated by lending at baseline. In fact the next row shows that only 37% (34%) of eligible poors' food borrowing links were reciprocal (reported also as food lending links) at baseline in the treatment (control) communities. Reciprocity of food transfers was positively correlated with the socio-economic standing of the household: the fraction of food borrowing sources who were also reported as recipients of food transfers increases to 52% for ineligible households. The final row in Table 1 shows that 10% of the eligible poor reported either a female or a male members' employer as a source of transfers or loans at baseline and the corresponding rate was 5% among the ineligible households.

Finally, Table 1 also presents test results for the differences between treatment and control observations along all the dimensions described above. Column (3) reports the normalized difference for every variable between the treatment and control observations (computed as the differences in the means divided by the square root of the sum of the variances) for eligible households, and col-

[18]In the survey, the specific question asked to identify food transfer links was: "Does your household ever need to borrow (or lend to those in need) rice or other food items from other households?" If the answer to this question was "yes", the respondent was asked "If you had to borrow food from another household, which households would your household typically ask for rice or other food items?" This method of identifying informal insurance partners is commonly adopted in the literature (Fafchamps and Lund (2003), De Weerdt and Dercon (2006), Barr and Genicot (2007)). Importantly, this method captures not only the realization of transfer links but potential transfer links. This is important especially to capture reciprocity of these connections, since transfers received in a given period may not necessarily be reciprocated by transfers given out in the same period. On the other hand, the questions in the survey were designed to capture the most important sources (and destinations) of food transfers *ever*, making it possible to see if transfers received from a households are ever reciprocated food transfers given out to the same household. For every (female) respondent, up to three sources of food transfers were recorded. The respondents were also asked whether they were expected to pay back the amount of food borrowed (or whether they expected 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

umn (7) does the same for the sample of ineligible households[19]. For all variables, the normalized differences are below the .25 threshold recommended by Imbens and Wooldrige (2009), suggesting that the randomization was successful and that the sample is balanced along these observable characteristics. Moreover, columns (5) and (8) report the raw differences between treatment and control observations in the samples of eligible poor and near poor households respectively. For only two out of the 46 variable-respondent-sample combinations presented in Table 1 do we have a marginally significant difference – household size and hours devoted to wage-employment by the female respondent in eligible households. Taken together, the comparisons of treatment and control communities at baseline show that the randomization was succesful and there were no systematic baseline differences between the female and male respondent samples in eligible and ineligible households at baseline.

As discussed in the introduction section, empirically identifying labor-tying is challenging, mainly because the concept of "tied-labor" did not correspond to a practical distinction that one could directly ask about in the survey. As such, in order to capture the degree of labor-tying in a given labor contract, I will be relying on two key aspects of the relationship: whether the employer was also reported as a source of transfers and loans, and the degree of volatility in wage earnings of the worker. In fact, Table A2 in the Appendix shows that these two indicators are correlated with one another. Workers who report their employers as a source of transfers, often have less volatile wage earnings during the year. Moreover, the average wage rate tends to be significantly lower for workers who report receiving transfers or loans from their employers (after controlling for worker characteristics such as age, literacy, nutritional status and household wealth). These correlations correspond to characteristics of tied labor contracts in the theoretical literature (for example in Bardhan (1983) and Mukherjee and Ray (1995)) whereby tied workers receive a lower wage rate in equilibrium but have less volatile earnings across the different seasons (harvest vs lean season), relative to workers in casual labor contracts.

5 Results

To test for the effects of the TUP program, I estimate:

$$y_{idt} = \alpha + \sum_{t=1}^{2} \lambda_t S_t T_{id} + \sum_{t=1}^{2} \gamma_t S_t + \beta T_i + \eta' X_{id0} + \delta_d + \epsilon_{idt}, \quad (1)$$

where y_{idt} is the outcome of interest for individual or household (depending on the outcome) i from subdistrict d at survey wave t with time periods referring to 2007 baseline (t=0), 2009 midline (t=1) and 2011 endline (t=2); T_i is an indicator variable = 1 if individual/household i lived in a treatment branch and = 0 if lived in a control branch, S_t are indicator variables for survey waves and δ_d are subdistrict (strata) fixed effects. The parameter of interest is λ_t, the difference

[19]The normalized difference is a scale-free measure that does not mechanically increase with sample size, in contrast to the p-value for the simple t-test of equal means (Imbens and Wooldrige (2009)).

in difference between treatment and control observations at survey wave t relative to baseline. The standard errors are clustered at the BRAC branch office level (the unit of randomization) in all the regressions. Under the identifying assumption that the control branches represent a valid counterfactual for the treated branches in the absence of the program, namely that trends in all outcomes of interests are the same in treatment and control branches, λ identifies the causal effect of the TUP program on y_{idt}.

First, I estimate (1) on the sample of eligible households, focusing on outcomes related to employment and insurance arrangements within these households. In particular, I test if the increase in the outside options of workers in eligible households (due to the TUP program), affected their participation labor-tying and alternative insurance mechanisms. Subsection 5.1 presents the findings. Then, in order to test whether the program had any general equilibrium effects through the labor markets in treated communities, I restrict the sample to the sample of ineligible households and estimate (1) on their labor market outcomes. The findings on the spill-over effects of the program on ineligible households are described in subsection 5.2.

5.1 Direct Effects on Eligible Poor Households

Table 2 provides results on the effects of the program on wage-employment among eligible women. The difference-in-difference estimate for midline (λ_1 in (1)) is given in the row "Treatment effect after 2 years" and for endline (λ_2 in (1)) is given in the row "Treatment effect after 4 years". The first two columns of the table test for the effects of the TUP program on the labor supply of eligible poor women for wage-employment. Column (1) shows that, four years after the baseline, women in eligible poor households were 8.5ppt less likely to be working for a wage, corresponding to a 16% fall relative to the baseline level. Similarly, column (2) of the table shows that the program also led to a drop on the intensive margin of wage-labor. By the endline survey, women in eligible poor households in treatment communities worked 170 hours (26% relative to baseline) less relative to baseline and relative to eligible women in control communities. Moreover, comparison of the effects at midline and endline show that this negative effect on the supply of labor into wage employment got stronger over time. The difference in treatment effects at midline and endline is statistically significant, both for the extensive and the intensive margins, as demonstrated by the p-value of the test of equality between the two effects reported at the bottom of the table. This dynamic effect suggests the expected outside options of eligible women improved not immediately following the asset transfer but gradually over time. This could be due to learning how to generate income from their new assets, or because they got more confident in their ability to generate income from these assets so their expected earnings from self-employment improved. Overall, these findings imply that the increase in the outside options of eligible poor women led to a significant reduction in their labor supply for wage employment.

The rest of Table 2 presents the effects of the program on labor-tying among eligible women. In order to test whether the increase in the outside options of eligible women affected their par-

ticipation in labor-tying, I estimate (1) on the likelihood that eligible women received transfers or loans from an employer and the volatility of their income from wage-labor. Two findings are of note: First, eligible women in treatment communities are significantly less likely to report their employer as a source of transfers or loans. This can be seen in column (3) of the table, where the dependent variable is a dummy variable equal to one if the primary female respondent worked for someone who they also reported as a source of transfers or loans. The results show that the program led to a fall of 2.3 (3.4) percentage points by the endline (midline) survey for this outcome. These effects are precisely estimated at conventional levels and relative to the baseline level of the dependent variable, correspond to a 29% (43%) fall in labor-tying by endline (midline). Column (4) shows that, conditional on being in wage-employment, eligible women were 3.3 (6) ppt less likely to report their employer as a source of transfers by the endline (midline). Second, consistent with a fall in labor-tying among eligible women, the volatility of their earnings from wage-employment increased. Column (5) shows that four years after the baseline, the program had led to an increase of TK263 in the range of monthly wage earnings. While this is effect is not precisely estimated at conventional levels, its magnitude is large, corresponding to a 23% increase relative to baseline. Columns (6) and (7) show that this effect was mainly driven by an increase in the upper tail of their montly earnings. During the month in which their earnings were highest, eligible women in treatment communities earned TK290 more (precisely estimated at 10% level) from wage-labor, relative to eligible women in control communities.

Taken together, the drop in transfers received from employers and the rise in the volatility of wage earnings suggests that the treatment led to a fall in labor-trying among the eligible women. As discussed in section 3, this is consistent with an increase in the outside options of targeted women, coming from the TUP program, leading them to quit labor-tying in favor of casual labor contracts. Yet another mechanism behind this fall in labor-tying may be the fact that treated women have to spend some time taking care of their new livestock businesses, which could lower their ability to commit their labor to an employer. Such an effect could be particularly strong for tied-labor contracts, which (according to the conceptual framework in section 3) would typically require a longer and steadier commitment on the part of the workers. One would expect this latter mechanism to be especially pronounced for women living in smaller households, such as single women or single mothers. However, results reported in Appendix Table A3 demonstrate that the fall in labor-tying was in fact stronger for eligible women living in larger households. Both the fall in the incidence of transfers from employers and the rise in the volatility of wage earnings was driven by women from larger households, where the time pressure coming from the livestock businesses were likely to be shared among the household members.

The final column of Table 2 shows that the changes in women's labor caused by the program resulted in an increase in their hourly wages. The difference in difference estimate at endline (midline) shows an increase of TK1.3 (TK0.8) in hourly wage rate for treated women. These effects are both statistically and economically significant. Relative to the baseline hourly wage of

TK5.5, the effect of the program corresponds to a 24% (15%) increase in daily wage rate of eligible poor women[20].

Although the TUP program is targeted primarily to women, other members of their households can be indirectly affected by the improvement in their household's self-employment opportunities. In particular, more than half of eligible women lived in households with a male head of household who, as described in Section 4, were surveyed separately and their labor activities were recorded in detail. In particular, it is possible to construct the same indicators of labor-tying for the male head of the household – namely whether he receives transfers or loans from his employer(s) and the volatility of his wage earnings. Table 3 reports the impacts of the program on the labor supply and terms of labor contracts for male heads of eligible households, where the effects are estimated using the same difference-in-difference specification in (1). Columns (1) and (2) of Table 3 show that the program did not have a significant impact on the labor supply of male heads of eligible households, neither on the extensive nor on the intensive margin. While the point estimates are negative, they are small and statistically insignificant. These findings imply that the program had little or no effect on men's overall labor supply for wage-employment. In contrast, estimates in columns (3) to (7) of Table 3 suggest that the program led to a fall in labor-tying for men in eligible households. Column (3) shows that, at midline, male heads of eligible households were 2 ppt less likely to work for someone who was also reported as a source of transfers or loans. While this effect loses its significance at endline, the estimate remains at 1.6 ppt and is statistically indistinguishable from the midline effect. In column (4), the sample is restricted to men who reported working for a wage. The estimates show that male respondents in eligible households were 3 ppt less likely to turn to their employers for transfers or loans (again, similarly at midline and endline). Finally, column (5) shows that the program led to a significant increase (of 25%) in the range of monthly wage earnings of male workers in eligible households. All together, findings in Table 3 imply that while the program did not lead to a significant change in the lamount of labor men in eligible households supplied for wage work, it decreased their participation in labor-tying.

The findings presented above suggest that the program led to a fall in labor-tying for both male and female workers in eligible households. A natural question is whether eligible households entered into other, alternative insurance mechanisms, in order to insure themselves against fluctuations in their earnings. In particular, one such mechanism highlighted in the literature is reciprocal transfer and credit arrangements between households. Table 4 presents results on the impacts of the program on the reciprocity of food transfers received and given by eligible households. First column shows the impact of the program *at the household level* on the likelihood of receiving

[20]Since the wage rate is observed only for those women who work for a wage, the effect on the wages of teligible women could be the result of three different mechanisms: (i) eligible women who decided to stay in wage employment may be the ones who had higher wages to start with; (ii) the change in type of contract (from tied to casual labor) could lead to an increase in eligible women's wages; (iii) the fall in the supply of female workers in the labor market could have led to an increase in the wage level. I cannot plausibly disentangle the first two mechanisms, but in order to test if the general equilibrium channel in (iii) is at work, I will analyze the effects on wages of ineligible female workers in Section 5.2.

transfers (or loans) from employers of household members. The dependent variable is a dummy variable equal to one if the household received any transfers (or loans) from an employer of either the primary female or the male head of the household respondents. By the midline survey the incidence of transfers and loans from employers of eligible households had diminished by 4.4 ppt (44% relative to a baseline level of 10%) more in treatment communities, relative to eligible poor in control communities. This effect is stable – by the endline survey the effect was -3.3 ppt and precisely estimated. The two estimates – at midline and endline – are not statistically different (with a p-value of 0.522, the test of equality between the two estimates cannot be rejected).

Second column of Table 4 shows the impact of the TUP program on reciprocity of eligible poor households' food transfer links. At baseline, among households who were reported as potential food transfer sources by eligible women, only 37% were also reported as ever recieving any food transfers back from the eligible households. In other words, 64% of people who were transferring food to eligible poor households were not receiving food in return[21]. By the midline survey there was a 6.8 percentage point increase in reciprocity of eligible poor households' food transfer connections. By the endline the impact had more than doubled – to 16 percentage points – and this increase in the impact is statistically significant (test of equality is rejected with a p-value of 0.023). In columns (3) to (6), the reciprocity index is broken down by eligible households' connections to different wealth classes. For instance, in column (3) the dependent variable is the fraction of eligible poor households in the respondent's food borrowing network who were also reported as a food lending connection. Looking at the baseline levels across columns (3) to (6), one can see that the reciprocity of food transfers of eligible poor households was higher with poorer households. At baseline, 66% of the eligible poor's food borrowing sources from their own wealth class (other eligible poor) were also reported as people they lent food to, while reciprocity was 54% with ineligible poor, 36% with middle class and only 11% with upper class households[22]. The estimates in columns (3) to (6) show that by the endline, reciprocity of eligible poor's food transfer connections with all wealth classes had increased but the largest impact was in reciprocity of their connections with wealthiest (upper class) households in the community. The endline impact on reciprocity of eligible poor's transfer links with upper class households is 24 ppt. This is significantly higher than that of reciprocity with middle class (15ppt), near poor (13ppt) or eligible poor (12ppt) households. This shows that the increase in reciprocity of eligible household's transfer network is not only driven by their connections with other eligible households – which is what one would expect if the effects were driven solely by the new connections formed among eligible households due to the TUP program. On the contrary, the change in their transfer networks' reciprocity is happening to a larger extent

[21] As described in Section **??**, the survey recorded up to three households for borrowing and up to three households for lending sources/destinations. For the reciprocity measure, this is not likely to be a major constaint as the average eligible household reported giving food transfers to only one household, 55% reported that they never lend food to other households and only 20% reported lending to three households.

[22] "Ineligible poor" refers to households who were ranked in the lowest wealth rank by the community ranking, but not found eligible by the TUP program officers based on the pre-determined eligibility criteria described in Section 2; "middle class" refers to households who were ranked neither in the lowest nor in the highest wealth ranks; and "upper class" refers to households who were ranked in the top wealth rank in the community ranking.

with households who at baseline were placed in higher wealth classes according to the community ranking.

Finally, columns (7) and (8) of Table 7 present the effects of the program on wealth of food borrowing and food lending networks respectively. The wealth of network members is measured at baseline, so it can only be affected through the program's effect on the composition of network members (of eligible poor households) and not by transfers from the program or within the community. The difference-in-difference estimates in column (7) show that the program had no significant impact on the wealth of households that the eligible households borrowed food from. On the other hand, column (8) shows that the program caused eligible poor in treatment communities to lend food to households who were wealthier at baseline. The magnitude of the estimates imply that by the endline (midline) eligible poor in treatment communities were lending food to households who were 33 (26) percent wealthier at baseline compared to lending partners of eligible poor households in control villages. This suggests that the program enabled eligible households to enter into transfer arrangements with wealthier households in their communities and they were more likely to reciprocate transfers of food with similar transfers (of food) made by themselves.

To summarize, the findings presented in this section imply that eligible women who were offered the TUP program reduced their supply of labor for wage employment, and were less likely to engage in tied-labor. On the other hand, men living in the same households as eligible women did not change their labor supply significantly, but they switched from labor-tying to casual wage-labor relationships. The increase in women's self-employment opportunities due to the program seems to have enabled their households to switch from labor-tying to a combination of casual labor contracts (with higher yield but more risk) and reciprocal transfer arrangements with other, wealthier households in their communities. In the following section, we turn to other (ineligible) households living in their communities and test if these changes in the labor arrangements of eligible households had any spill-over effects on the rest of the community, particularly through the local labor markets.

5.2 General Equilibrium Effects Through the Labor Markets

The conceptual framework discussed in section 3 suggest that the program could affect the equilibrium in the local labor market. In particular, if the decrease in labor supply of female workers is large enough, the equilibrium wages in tied and casual labor contracts may increase. Whether the program corresponds to a large enough shock at the community level to cause such general equilibrium effects is an empirical question and in this section I test for this by evaluating the *indirect* effects of the program on *ineligible* households in treatment communities. These households did not experience a direct increase in their self-employment opportunities through the TUP program, so any impact on their labor is likely to be an indirect effect, either through the local labor market (as highlighted in the conceptual framework) or through alternative channels. One alternative channel may work through informal transfers from eligible to ineligible households (Angelucci and

de Giorgi (2009)) and I discuss below which of these channels are likely to be driving the results. But first, I test for the effects of the program on labor contracts of ineligible women[23]. The identification strategy is same as the difference-in-difference methodology in (1), except now the sample includes ineligible households. In order to take into account different sampling weights across the different wealth ranks, weighted OLS regressions where each observation is weighed by the inverse of the sampling weight for the relevant wealth class in that community will be used.

Table 5 presents findings on the spillover effects of the program on ineligible women's labor contracts. The first five columns of the table shows the difference-in-difference estimates for the outcomes related to labor-tying among ineligible women. In columns (1) and (2), the dependent variable is whether the respondent was receiving transfers or loans from any of employer(s). While the point estimates are negative, the estimates are not statistically different from zero at conventional levels and their magnitudes are very small. Similarly, column (3) estimates a positive but imprecisely estimated effect on the volatility of wage earnings (i.e. the range of monthly wage income) at midline and endline. According to column (4), there was a significant effect (at 10% level) on the maximum monthly earnings that ineligible women had from wage employment; while column (5) shows a positive (but insignificant effect) also on the minimum monthly earnigns from wage labor. Overall, the estimates suggest that the TUP program did not lead to a major change in the degree of labor-tying among ineligible women.

The remainder of Table 5 shows the effects of the program on the wages of ineligible women. Column (6) shows that on average, the program had led to a precisely estimated increase of TK 0.8 (0.7) in the hourly wage of ineligible women by the endline (midline) survey. Relative to the baseline wage level, this correspond to a 13% (12%) increase in the average hourly wage received by ineligible female workers – which implies that the effect was not only statistically but also economically significant. To disentangle the effects of the program on wages of women in tied and casual contracts, in column (7), I test whether there was a differential effect on wages of women who received transfers or loans from their employers[24]. Assuming that having received transfer or loans from an employer is a suitable indicator for being in a tied contract, this model allows me to test if the program had a differential impact on wages of ineligible women who were in tied

[23]I will be testing the spillover effects of the program on females and males separately as the labor markets are segmented by gender in this setting. Foster and Rosenzweig (1996) show that male and female workers in rural India work in different types of jobs, depending on their comparative advantage. This is similar in rural Bangladesh where men often work in physically-demanding tasks, while women work in tasks that require less physical strength, such as sowing seeds, taking care of livestock, working as a maid etc. Moreover, the wage rate for male workers is much higher compared to that of females (average wage rate for a male worker is 75% higher at baseline relative to a female worker). Due to these reasons, I analyze the effects on male and female labor markets separately.

[24]In particular, I estimate:

$$y_{idt} = \alpha + \sum_{t=1}^{2} \psi_t S_t T_{id} + \sum_{t=1}^{2} \phi_t S_t T_{id} Z_{id0} + \sum_{t=1}^{2} \gamma_t S_t + \beta T_i + \zeta Z_{id0} + \theta T_i Z_{id0} + \sum_{t=1}^{2} \kappa_t S_t Z_{id0} + \delta_d + \epsilon_{idt}, \quad (2)$$

where y_{idt} is the hourly wage for individual i at survey wave t; T_i , S_t , δ_d are as before (in specification (1)); Z_{id0} is a dummy variable equal to one if equal to one if respondent i received any transfers or loans from her employer(s) at baseline ($t = 0$). In this model ϕ_t is the differential effect on women who were receieving transfers or loans from their employers at baseline.

contracts at baseline relative to those who were in casual labor contracts. Column (7) of Table 5 shows the results. The difference-in-difference estimate for women who were in casual contracts at baseline is TK0.7 (at both endline and midline) and statistically significant, and the effect on women who were in tied contracts at baseline is TK0.7 (TK0.4) at endline (midline). Although the latter effect on wages of women in tied contracts is imprecisely estimated, the difference between the two subsamples is not significant – as demonstrated by the imprecisely estimated triple interaction terms. This suggests that, due to the intervention, the female wages in treated communities went up, both for women who were tied and casual contracts.

Next, I test for any spillover effects of the program on the terms of men's labor contracts in ineligible households. The findings in section 5.1 showed that the program had no significant impact on the labor supply of male workers from eligible households , but it caused a fall in their participation in labor-tying. To test if these changes had any consequences on the labor contracts of ineligible men, I estimate (1) and (2) for the sample of male heads of ineligible households. Table 6 provides the results. The program had no significant spillover effects on the labor contracts of ineligible men. Neither the indicators of labor-tying, nor the hourly wage rate of ineligible men were significantly affected by the treatment.

The findings presented in this section imply that the program led to an increase in the hourly wages of women but had no impact on wages of men in ineligible households of treated communities. This is in line with the conceptual framework discussed in section 3, which predicted that the program could lead to a change in the equilibrium in the labor market. On the other hand, an alternative mechanism behind these effects could be through informal transfers from eligible to ineligible households which may be indirectly improving their (the ineligible households') outside options. For example, if the eligible households transferred part of their livestock to ineligible households in their social networks and if such transfers were large enough to increase the outside options of the recipients, this may have led them to quit any low-wage labor contracts in favor of more lucrative employment opportunities. As such, informal transfers from eligible to ineligible households may constitute an alternative channel driving the observed increase in the wages of ineligible women. If this were the case, one would expect to see a higher increase in wages of ineligible women who had social connections to eligible households. To test for this, I use data on social networks and identify which ineligible households had social or economic connections to the eligible households in their communities. Then, I test if the program had any differential spillover effects on ineligibles who were connected to eligible households. At baseline 17% of ineligible households were connected to at least one eligible household within their community. Among them, 59% were connected to eligibles via extended family networks[25], 64% via informal insurance networks (food or other transfers) and 10% via economic networks (employment, land tenancy, asset transaction etc).

Table 7 reports the results of estimating the heterogenous spillover effects of the program on

[25]The survey instruments collected information on the first-degree family networks: parents, children, siblings, parents-in-law, children-in-law, siblings-in-law.

ineligible households, as a function of being connected to eligible households at baseline. The first column shows the effects on ineligible women's wages, while column (2) does the same for men's wages. Three findings are of note: First, the effect on the wages of ineligible women who were not connected to eligible households at baseline is positive and statistically significant, both at midline (TK 0.6) and at the endline (TK 0.7) survey. Second, even though the differential effect on the wages of ineligible women who had baseline connections to eligible households is not precisely estimated, it is large in magnitude. In particular, the midline estimate on wages of ineligible women who were connected to eligibles is nearly double that for ineligible women who were not connected to any eligible household (TK 1.09 vs TK 0.6). While the difference is smaller and imprecisely estimated at the endline survey (TK 0.97 vs TK 0.7), it still corresponds to a sizable 27% difference. Third, there was no impact on the wages of ineligible men, irrespective of whether or not they were connected to eligible households. These findings are in line with the presence of general equilibrium effects through the labor market, but also suggest that ineligible women who were connected to eligible households may have experienced an even bigger increase in their wages relative to those who had no such connections. One possible explanation for this may be, as descrived above, through informal transfers from eligible to ineligible households, which may have enabled women in ineligible households to quit low-wage jobs.

To summarize, the findings presented in this section suggest that the program had significant general equilibrium effects throught the labor market. The fall in eligible women's labor supply caused by the program led to an increase in wages in the female labor market, while men's wages were not affected. This highlights the highly gender-segmented nature of the labor markets in this setting.

6 Conclusion

Despite as extensive literature that highlights how a risk-neutral employer may provide a risk-averse worker with insurance against income fluctuations, evidence on the phenomenon from developing country settings is rare. This paper provided evidence that in a rural labor market, some workers may receive transfers and loans from their employers and while doing so, attain smoother earnings from wage-employment. Thus, labor-tying can be one of the informal insurance mechanisms available to individuals in rural settings with imperfect credit markets. Moreover, it will be the poorest workers with the most limited outside options who typically partake in this mechanism. To show this, I used data from the randomized evaluation of an antipoverty program that targeted poorest women in rural communities in Bangladesh and provided them with productive assets (mainly livestock) and training on skills complementary with these assets. In effect, the program improved self-employment opportunities of women who, at baseline, had limited outside options to wage-employment on farms or households of rich employers. This exogenous increase in poor women's self-employment opportunities reduced their likelihood to receive transfers and

loans from their employers and increased the volatility of their earnings from wage-employment. Thus, it decreased their engagement in labor-tying. On the other hand, they became more likely to engage in reciprocated transfer arrangements with other, wealthier households in the community.

These findings demonstrate an interlinkage between insurance and labor markets in rural parts of developing countries, 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 options of poor workers causes the link between their labor and insurance arrangements to weaken, as they move to riskier labor opportunities while increasing their participation in reciprocal transfer arrangements with other households in the community[26].

The analysis also yields results on the general equilibrium effects of the program. In particular, the increase in poor women's outside options reduces their labor supply which leads to an increase in the equilibrium wage for women. As such, the paper contributes to a growing literature that highlights the general equilibrium effects of antipoverty programs in developing country settings (Imbert and Papp (forthcoming)). In evaluating the impacts of entrepreneurship programs and other labor-market interventions, it is essential to take into account general equilibrium effects through the labor market.

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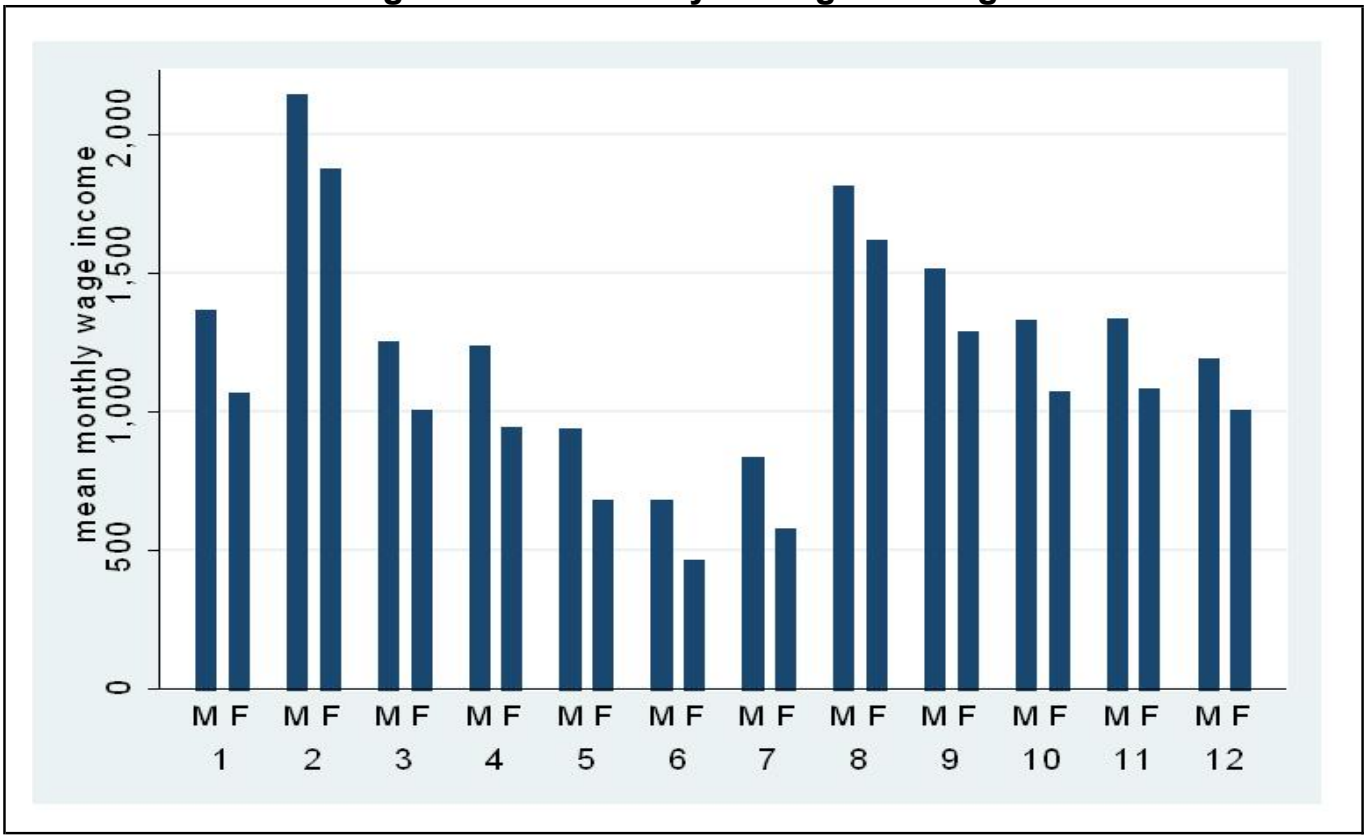
Figure 1: Seasonality of Wage Earnings



Notes: The 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.

Table 1: Baseline Characteristics of Eligible Poor and Near Poor Households, by Treatment Status

	Sample: Eligible Households				Sample: Ineligible Households			
	Treatment Communities	Control Communities	Normalized Differences	Raw Differences	Treatment Communities	Control Communities	Normalized Differences	Raw Differences
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Household Characteristics								
Total household wealth [TAKAs]	5373.04	6570.46	-0.026	-1197.42	183662.7	184062.8	-0.001	-400.113
	(20145.37)	(41750.82)		(1566.24)	(488673.6)	(494904.7)		(19201.68)
Primary female is literate [Yes=1]	0.073	0.067	0.017	0.006	0.250	0.258	-0.014	-0.009
	(0.260)	(0.250)		(0.012)	(0.433)	(0.438)		(0.020)
Household size	3.392	3.101	0.118	0.283*	4.234	4.181	0.021	0.053
	(1.694)	(1.708)		(0.078)	(1.741)	(1.735)		(0.106)
B. Women's Labor Outcomes								
Works for a wage [Yes=1]	0.521	0.578	-0.081	-0.057	0.197	0.218	-0.037	-0.021
	(0.500)	(0.494)		(0.055)	(0.398)	(0.413)		(0.038)
Hours devoted to wage employment	646.76	810.36	-0.137	-163.60*	219.63	269.49	-0.062	-49.85
	(805.55)	(886.67)		(81.70)	(535.97)	(606.98)		(43.69)
Receives transfers or loans from employer [Yes=1]	0.081	0.086	-0.013	-0.005	0.031	0.030	0.002	(0.0004)
	(0.273)	(0.280)		(0.014)	(0.171)	(0.171)		(0.008)
Conditional on working for a wage:								
Receives transfers or loans from employer [Yes=1]	0.155	0.149	0.012	0.006	0.155	0.138	0.034	0.017
	(0.362)	(0.356)		(0.021)	(0.362)	(0.345)		(0.023)
Hourly earnings from wage-labor [TAKAs per Hour]	5.517	5.422	0.028	0.096	5.801	6.191	-0.105	-0.384
	(2.320)	(2.452)		(0.475)	(2.386)	(2.793)		(0.557)
Range of monthly wage earnings [TAKAs per Month]	1157.58	1256.98	-0.051	-99.396	1174.19	1323.06	-0.092	-148.87
	(943.77)	(1704.53)		(154.28)	(990.93)	(1275.67)		(142.13)
Maximum monthly wage earning [TAKAs per Month]	1281.97	1452.32	-0.088	-170.36	1301.68	1505.11	-0.127	-203.42
	(909.28)	(1713.89)		(143.66)	(969.78)	(1280.14)		(125.47)
Minimum monthly wage earning [TAKAs per Month]	124.38	195.34	-0.143	-70.96	127.49	182.04	-0.102	-54.55
	(300.60)	(394.72)		(43.87)	(335.91)	(415.28)		(43.58)
C. Men's Labor Outcomes								
Works for a wage [Yes=1]	0.631	0.595	0.052	0.036	0.441	0.457	-0.023	-0.016
	(0.483)	(0.491)		(0.032)	(0.497)	(0.498)		(0.028)
Hours devoted to wage employment	1099.98	1073.11	0.019	26.87	730.48	782.04	-0.037	-51.56
	(1005.19)	(1042.68)		(86.31)	(966.30)	(991.73)		(60.64)
Receives transfers or loans from employer [Yes=1]	0.053	0.049	0.012	0.004	0.027	0.031	-0.020	-0.005
	(0.224)	(0.216)		(0.013)	(0.161)	(0.174)		(0.006)
Conditional on working for a wage:								
Receives transfers or loans from employer [Yes=1]	0.084	0.082	0.004	0.001	0.060	0.069	-0.025	-0.009
	(0.277)	(0.275)		(0.019)	(0.238)	(0.253)		(0.013)
Hourly earnings from wage-labor [TAKAs per Hour]	8.896	8.945	-0.013	-0.047	9.875	9.970	-0.015	-0.095
	(2.583)	(2.657)		(0.414)	(4.387)	(4.628)		(0.325)
Range of monthly wage earnings [TAKAs per Month]	2178.35	2164.25	0.006	14.095	2403.58	2221.16	0.038	182.42
	(1809.46)	(1797.87)		(172.89)	(4286.55)	(2224.33)		(259.75)
Maximum monthly wage earning [TAKAs per Month]	2603.12	2623.19	-0.008	-20.06	2946.07	2819.91	0.026	126.17
	(1799.98)	(1885.22)		(188.10)	(4245.62)	(2334.23)		(269.91)
Minimum monthly wage earning [TAKAs per Month]	424.78	458.93	-0.033	-34.16	542.50	598.75	-0.036	-56.25
	(677.89)	(774.72)		(122.88)	(1090.33)	(1110.16)		(104.93)
D. Insurance Mechanisms of the Household								
Received food transfers [Yes=1]	0.925	0.914	0.027	0.011	0.794	0.832	-0.068	-0.038
	(0.264)	(0.280)		(0.042)	(0.404)	(0.374)		(0.037)
Gives food transfers [Yes=1]	0.455	0.410	0.064	0.045	0.640	0.663	-0.033	-0.022
	(0.498)	(0.492)		(0.062)	(0.480)	(0.473)		(0.052)
Reciprocity of food transfer links	0.366	0.337	0.046	0.029	0.522	0.532	-0.016	-0.010
	(0.445)	(0.438)		(0.052)	(0.457)	(0.450)		(0.042)
Household receives transfers or loans from any employer [Yes=1]	0.101	0.100	0.004	0.002	0.047	0.050	-0.009	-0.003
	(0.302)	(0.300)		(0.016)	(0.212)	(0.218)		(0.010)
Number of Observations:								
Number of households / female respondents	4045	2687	6732	6732	7633	8664	16297	16297
Number of male respondents	2561	1412	3973	3973	6740	7611	14351	14351
Clusters	20	20	40	40	20	20	40	40

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. All data refers to the baseline survey. Columns 1 and 2 report statistics on the sample of eligible households in treatment and control communities respectively, while columns 5 and 6 report statistics on the sample of ineligible households in treatment and control communities respectively. Columns 3 and 7 report normalized differences computed as the difference in means in treatment and control communities divided by the square root of the sum of the variances for eligible and ineligible households respectively. Columns 4 and 8 report the difference in means and its standard error clustered at the branch office level for eligible and ineligible households respectively. Panel A refers to household characteristics, Panel B refers to characteristics of wage employment for the lead woman in the household and Panel C refers to characteristics of wage employment for the male head of the household. Panel D refers to characteristics of the household's informal insurance mechanisms. Total household wealth equals the sum of the values (as reported by the respondent) of household durables and productive assets (e.g. land, livestock etc.) owned by the household. Household size gives the number of household members. All labor outcomes are defined over the year prior to the baseline survey. The woman/man is defined to be employed in wage labor (the dummy equals one) if the she/he engages in income generating activities where she/he is employed by others. Hours spent in wage employment are computed by multiplying the number of hours worked in a typical day by the number of days worked in a year for each wage labor activity and then summing across all wage labor activities. "Receives transfers or loans from employer" is a dummy variable equal to 1 if the respondent is working for an employer who is also reported as a source of food transfers or has given transfers or loans to the respondent during the past year. Hourly earnings from wage labor is the sum of earnings in cash and in kind during the past year from wage employment, divided by total hours spent on wage employment. Range of monthly wage earnings is the difference between the maximum and minimum monthly earnings of the respondent from wage employment during the past 12 months. Maximum (minimum) monhtly wage earnings is the amount of income that the respondent had from wage employment during the month (in the last year) when wage-labor income was at its peak (lowest). "Received food transfers" is a dummy variable equal to 1 if the respondent said "Yes" to the following question: "Does your household ever borrow rice or other food items from other households". "Gives food transfers" is a dummy variable equal to 1 if the respondent said "Yes" to the following question: "Does your household ever give out rice or other food items from other households". Reciprocity of food transfer links is the fraction of food borrowing links who are also reported as food lending links. "Any employer is reported as a source of transfers" is equal to 1 if any of the employers of the primary female or the male household head are reported as a source of food transfers. In 2007, 1USD=69TK.

Table 2: Effects on Women's Labor in Eligible Households

	Labor Supply of Eligible Women		Labor Contracts of Eligible Women					
					Volatility of Wage Earnings			
	Extensive Margin: Works for a wage [Yes=1]	Intensive Margin: Hours devoted to wage employment	Receives transfers or loans from employer [Yes=1]	Receives transfers or loans from employer, conditional on working for a wage [Yes=1]	Range of monthly wage earnings [TAKAs per Month]	Maximum monthly wage earning [TAKAs per Month]	Minimum monthly wage earning [TAKAs per Month]	Hourly earnings from wage-labor [TAKAs per Hour]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment effect after 2 years	-0.026	-82.334	-0.034**	-0.060**	182.841	182.219	-0.622	0.765**
	(0.024)	(53.599)	(0.014)	(0.023)	(174.493)	(170.993)	(40.774)	(0.331)
Treatment effect after 4 years	-0.085***	-169.139***	-0.023*	-0.033*	262.770	290.274*	27.505	1.275***
	(0.023)	(61.004)	(0.013)	(0.019)	(156.908)	(153.274)	(43.274)	(0.271)
Mean of outcome variable in treated communities at baseline	0.521	646.76	0.081	0.155	1157.58	1281.97	124.38	5.517
Two year impact = Four year impact [p-value]	0.016	0.084	0.500	0.274	0.530	0.385	0.320	0.170
Adjusted R-squared	0.094	0.086	0.021	0.024	0.053	0.050	0.073	0.304
Observations (Clusters)	20196 (40)	20196 (40)	20196 (40)	10686 (40)	10436 (40)	10436 (40)	10436 (40)	10436 (40)

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. The table reports ITT estimates based on a difference-in-difference specification estimated by OLS. The sample includes eligible households who were surveyed at baseline (2007), midline (2009) and endline (2011) surveys. The program effect after two (four) years is the coefficient on the interaction term between the treatment indicator and indicator for the midline (endline) survey wave. At the end of the table, the p-value for the hypothesis test that the two and four year program impacts are equal is reported. All specifications control for the level effect of the treatment, survey waves and subdistrict fixed effects. Standard errors are clustered at the BRAC branch office level. All variables are measured at an annual basis and refer to the labor characteristics of the primary female respondent. The individual is defined to be employed in wage labor (the dummy equals one) if she was engaged in income generating activities where she was employed by others. Hours spent in wage employment are computed by multiplying the number of hours worked in a typical day by the number of days worked in a year for each wage labor activity and then summing across all wage labor activities. "Receives transfers or loans from employer" is a dummy variable equal to 1 if the respondent is working for an employer who is also reported as a source of food transfers or has given transfers or loans to the respondent during the past year. Range of monthly wage earnings is the difference between the maximum and minimum monthly earnings of the respondent from wage employment during the past 12 months. Maximum (minimum) monthly wage earnings is the amount of income that the respondent had from wage employment during the month (in the last year) when wage-labor income was at its peak (lowest). Hourly earnings from wage labor is the sum of earnings in cash and in kind during the past year from wage employment, divided by total hours spent on wage employment. In Columns (4)-(8), the sample is resctricted to eligible women who were engaged in wage employment during the last 12 months before each survey wave. All monetary values are measured in Bangladeshi Takas, deflated to 2007 prices using the annual CPI index published by the Bank of Bangladesh. In 2007, 1USD=69TK.

Table 3: Effects on Men's Labor in Eligible Households

	Labor Supply of Men in Eligible Households		Labor Contracts of Men in Eligible Households					
					Volatility of Wage Earnings			
	Extensive Margin: Works for a wage [Yes=1]	Intensive Margin: Hours devoted to wage employment	Receives transfers or loans from employer [Yes=1]	Receives transfers or loans from employer, conditional on working for a wage [Yes=1]	Range of monthly wage earnings [TAKAs per Month]	Maximum monthly wage earning [TAKAs per Month]	Minimum monthly wage earning [TAKAs per Month]	Hourly earnings from wage-labor [TAKAs per Hour]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment effect after 2 years	-0.014	-64.466	-0.020*	-0.035*	165.741	75.672	-90.069	0.621
	(0.038)	(88.304)	(0.010)	(0.018)	(277.829)	(265.992)	(114.818)	(0.443)
Treatment effect after 4 years	-0.059	-83.732	-0.016	-0.031	544.618**	410.320	-134.298	1.018
	(0.064)	(123.973)	(0.013)	(0.027)	(258.685)	(246.581)	(103.812)	(0.614)
Mean of outcome variable in treated communities at baseline	0.631	1098.85	0.053	0.084	2178.35	2603.12	424.78	8.896
Two year impact = Four year impact [p-value]	0.492	0.836	0.732	0.886				0.466
Adjusted R-squared	0.082	0.082	0.023	0.029	0.067	0.075	0.126	0.346
Observations (Clusters)	11731 (40)	11731 (40)	11731 (40)	5792 (40)	5641 (40)	5641 (40)	5641 (40)	5645 (40)

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. The table reports ITT estimates based on a difference-in-difference specification estimated by OLS. The sample includes eligible poor households who were surveyed at baseline (2007), midline (2009) and endline (2011) surveys. The program effect after two (four) years is the coefficient on the interaction term between the treatment indicator and indicator for the midline (endline) survey wave. At the end of the table, the p-value for the hypothesis test that the two and four year program impacts are equal is reported. All specifications control for the level effect of the treatment, survey waves and subdistrict fixed effects. Standard errors are clustered at the BRAC branch office level. All variables are measured at an annual basis and refer to the labor characteristics of the male head of the household. The individual is defined to be employed in wage labor (the dummy equals one) if she was engaged in income generating activities where she was employed by others. Hours spent in wage employment are computed by multiplying the number of hours worked in a typical day by the number of days worked in a year for each wage labor activity and then summing across all wage labor activities. "Receives transfers or loans from employer" is a dummy variable equal to 1 if the respondent is working for an employer who is also reported as a source of food transfers or has given transfers or loans to the respondent during the past year. Range of monthly wage earnings is the difference between the maximum and minimum monthly earnings of the respondent from wage employment during the past 12 months. Maximum (minimum) monthly wage earnings is the amount of income that the respondent had from wage employment during the month (in the last year) when wage-labor income was at its peak (lowest). Hourly earnings from wage labor is the sum of earnings in cash and in kind during the past year from wage employment, divided by total hours spent on wage employment. In Columns (4)-(8), the sample is resctricted to male heads of eligible households who were engaged in wage employment during the last 12 months before each survey wave. All monetary values are measured in Bangladeshi Takas, deflated to 2007 prices using the annual CPI index published by the Bank of Bangladesh. In 2007, 1USD=69TK.

Table 4: Effect on Reciprocity of Food Transfers

	Household receives transfers or loans from any employer [Yes=1]	Reciprocity (fraction of borrowing partners who are also reported as lending partners)					Log (baseline) wealth of food transfer network	
		overall	with eligible poor	with ineligible poor	with middle class	with upper class	Borrowing	Lending
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment effect after 2 years	-0.044***	0.068**	0.035	0.083**	0.066*	0.115***	0.074	0.237**
	(0.015)	(0.033)	(0.048)	(0.040)	(0.034)	(0.039)	(0.062)	(0.115)
Treatment effect after 4 years	-0.033**	0.157***	0.115**	0.136***	0.158***	0.238***	-0.048	0.304**
	(0.014)	(0.039)	(0.054)	(0.046)	(0.040)	(0.054)	(0.081)	(0.140)
Mean of outcome variable in treated communities at baseline	0.101	0.366	0.645	0.538	0.360	0.107	10.469	8.884
Two year impact = Four year impact [p-value]	0.522	0.023	0.059	0.251	0.023	0.022	0.034	0.647
Adjusted R-squared	0.025	0.139	0.104	0.107	0.121	0.158	0.048	0.068
Observations (Clusters)	20196 (40)	18979 (40)	3911 (40)	6978 (40)	13860 (40)	4508 (40)	18523 (40)	12817 (40)

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. The table reports ITT estimates based on a difference-in-difference specification estimated by OLS. The sample includes eligible poor households surveyed at baseline (2007), midline (2009) and endline (2011) surveys. The program effect after two (four) years is the coefficient on the interaction term between the treatment indicator and indicator for the midline (endline) survey wave. At the end of the table, the p-value for the hypothesis test that the two and four year program impacts are equal is reported. All specifications control for the level effect of the treatment, survey waves, subdistrict fixed effects and an indicator variable equal to 1 if the primary female respondent was literate at baseline. Standard errors are clustered at the BRAC branch office level. The dependent variable in column (1) is a dummy variable equal to 1 if any of the employers of the primary female or the male household head are reported as a source of food transfers in times of need, or a source of loans/transfers during the past 12 months. Reciprocity of food transfer links is the fraction of households that the respondent reported as sources of food transfers who were also reported as destinations of food transfers given out. The dependent variables in columns (3) - (6) are the fraction of eligible poor, near-poor, middle class or rich class households (respectivey) who are reported sources of food transfers and also reported as destinations of food transfers given out by the respondent's household. The dependent variables in columns (7) and (8) are the natural logarithm of the average wealth (as measured at community census survey in 2007, prior to the baseline household survey) of households reported as food borrowing/lending partners of the respondent's household. All monetary values are measured in Bangladeshi Takas, deflated to 2007 prices using the annual CPI index published by the Bank of Bangladesh. In 2007, 1USD=69TK.

Table 5: Spillover Effects On Ineligible Women's Labor Contracts

	Receives transfers or loans from employer [Yes=1]	Receives transfers or loans from employer, conditional on working for a wage [Yes=1]	Range of monthly wage earnings [TAKAs per Month]	Maximum monthly wage earning [TAKAs per Month]	Minimum monthly wage earning [TAKAs per Month]	Hourly earnings from wage-labor [TAKAs per Hour]	Hourly earnings from wage-labor [TAKAs per Hour]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment x midline	-0.003	-0.040	146.712	191.188	44.476	0.670**	0.671**
	(0.004)	(0.026)	(158.062)	(164.878)	(42.589)	(0.325)	(0.331)
Treatment x endline	-0.002	-0.020	189.209	252.938*	63.729	0.753**	0.736**
	(0.004)	(0.023)	(133.155)	(142.906)	(52.053)	(0.335)	(0.330)
Treatment x midline x transfers at baseline							-0.254
							(0.541)
Treatment x endline x transfers at baseline							-0.090
							(0.502)
Treatment effect after 2 years for workers who werereceiving transfers at baseline							0.417
							(0.555)
Treatment effect after 4 years for workers who were receiving transfers at baseline							0.646
							(0.574)
Mean of outcome variable in treated communities at baseline	0.031	0.155	1174.19	1301.68	127.49	5.801	5.801
Two year impact = Four year impact [p-value]	0.758	0.405	0.752	0.645	0.641	0.836	
Adjusted R-squared	0.005	0.018	0.069	0.063	0.057	0.236	0.237
Observations (Clusters)	48891	11288	10930	10930	10930	10930	10930

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. The table reports ITT estimates based on a difference-in-difference specification estimated by weighted OLS. The sample includes ineligible households who were surveyed at baseline (2007), midline (2009) and endline (2011) surveys. The weights are the sampling weights calculated as the fraction of households in the four-year panel from each wealth class (lower, middle and upper) relative to the number of households from the relevant wealth class in the community census. The program effect after two (four) years is the coefficient on the interaction term between the treatment indicator and indicator for the midline (endline) survey wave. At the end of the table, the p-value for the hypothesis test that the two and four year program impacts are equal is reported. All specifications control for the level effect of the treatment, survey waves and subdistrict fixed effects. In addition, specification in column (7) controls for whether the individual received any transfers or loans from an employer at baseline and the interaction of this variable with treatment indicator and survey wave fixed effects. Standard errors are clustered at the BRAC branch office level. All variables are measured at an annual basis and refer to the labor characteristics of the primary female respondent. "Receives transfers or loans from employer" is a dummy variable equal to 1 if the respondent is working for an employer who is also reported as a source of food transfers or has given transfers or loans to the respondent during the past year. Range of monthly wage earnings is the difference between the maximum and minimum monthly earnings of the respondent from wage employment during the past 12 months. Maximum (minimum) monthly wage earnings is the amount of income that the respondent had from wage employment during the month (in the last year) when wage-labor income was at its peak (lowest). Hourly earnings from wage labor is the sum of earnings in cash and in kind during the past year from wage employment, divided by total hours spent on wage employment. Hours spent in wage employment are computed by multiplying the number of hours worked in a typical day by the number of days worked in a year for each wage labor activity and then summing across all wage labor activities. In Columns (2)-(7), the sample is resctricted to ineligible women who were engaged in wage employment during the last 12 months before each survey wave. All monetary values are measured in Bangladeshi Takas, deflated to 2007 prices using the annual CPI index published by the Bank of Bangladesh. In 2007, 1USD=69TK.

Table 6: Spillover Effects On Ineligible Men's Labor Contracts

	Receives transfers or loans from employer [Yes=1]	Receives transfers or loans from employer, conditional on working for a wage [Yes=1]	Range of monthly wage earnings [TAKAs per Month]	Maximum monthly wage earning [TAKAs per Month]	Minimum monthly wage earning [TAKAs per Month]	Hourly earnings from wage-labor [TAKAs per Hour]	Hourly earnings from wage-labor [TAKAs per Hour]
	(1)	(2)	(4)	(5)	(6)	(3)	(7)
Treatment x midline	0.001	0.002	-267.618	-326.450	-58.831	0.041	0.044
	(0.005)	(0.013)	(400.742)	(364.352)	(93.761)	(0.429)	(0.429)
Treatment x endline	0.003	0.015	-33.882	-186.802	-152.920	0.390	0.389
	(0.004)	(0.011)	(336.852)	(323.507)	(112.624)	(0.607)	(0.610)
Treatment x midline x transfers at baseline							0.135
							(0.491)
Treatment x endline x transfers at baseline							0.467
							(0.784)
Treatment effect after 2 years for workers who were receiving transfers at baseline							0.179
							(0.612)
Treatment effect after 4 years for workers who were receiving transfers at baseline							0.856
							(0.835)
Mean of outcome variable in treated communities at baseline	0.027	0.060	2403.58	2946.07	542.50	9.875	9.875
Two year impact = Four year impact [p-value]	0.603	0.292	0.393	0.525	0.401	0.518	
Adjusted R-squared	0.006	0.016	0.040	0.044	0.044	0.144	0.146
Observations (Clusters)	42539	15670	15223	15223	15223	15228	15228

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. The table reports ITT estimates based on a difference-in-difference specification estimated by weighted OLS. The sample includes ineligible households who were surveyed at baseline (2007), midline (2009) and endline (2011) surveys. The weights are the sampling weights calculated as the fraction of households in the four-year panel from each wealth class (lower, middle and upper) relative to the number of households from the relevant wealth class in the community census. The program effect after two (four) years is the coefficient on the interaction term between the treatment indicator and indicator for the midline (endline) survey wave. At the end of the table, the p-value for the hypothesis test that the two and four year program impacts are equal is reported. All specifications control for the level effect of the treatment, survey waves and subdistrict fixed effects. In addition, specification in column (7) controls for whether the individual received any transfers or loans from an employer at baseline and the interaction of this variable with treatment indicator and survey wave fixed effects. Standard errors are clustered at the BRAC branch office level. All variables are measured at an annual basis and refer to the labor characteristics of the male head of the household. "Receives transfers or loans from employer" is a dummy variable equal to 1 if the respondent is working for an employer who is also reported as a source of food transfers or has given transfers or loans to the respondent during the past year. Hourly earnings from wage labor is the sum of earnings in cash and in kind during the past year from wage employment, divided by total hours spent on wage employment. Hours spent in wage employment are computed by multiplying the number of hours worked in a typical day by the number of days worked in a year for each wage labor activity and then summing across all wage labor activities. Range of monthly wage earnings is the difference between the maximum and minimum monthly earnings of the respondent from wage employment during the past 12 months. Maximum (minimum) monthly wage earnings is the amount of income that the respondent had from wage employment during the month (in the last year) when wage-labor income was at its peak (lowest). In Columns (2)-(7), the sample is resctricted to ineligible men who were engaged in wage employment during the last 12 months before each survey wave. All monetary values are measured in Bangladeshi Takas, deflated to 2007 prices using the annual CPI index published by the Bank of Bangladesh. In 2007, 1USD=69TK.

Table 7: Mechanisms Behind Spillover Effects On Wages

	Women's wages [TAKAs per Hour]	Men's wages [TAKAs per Hour]
	(1)	(2)
Treatment x midline	0.560*	0.051
	(0.304)	(0.442)
Treatment x midline x connected to eligibles	0.524	-0.015
	(0.340)	(0.451)
Treatment x endline	0.704**	0.427
	(0.346)	(0.630)
Treatment x endline x connected to eligibles	0.263	-0.134
	(0.403)	(0.521)
Treatment effect after 2 years for those connected to eligibles at baseline	1.084**	0.036
	(0.494)	(0.558)
Treatment effect after 4 years for those connected to eligibles at baseline	0.967**	0.292
	(0.462)	(0.659)
Adjusted R-squared	0.236	0.144
Observations	10930	15231

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. The table reports ITT estimates based on a difference-in-difference specification estimated by weighted OLS. The sample includes *ineligible* households who were surveyed at baseline (2007), midline (2009) and endline (2011) surveys. The weights are the sampling weights calculated as the fraction of households in the four-year panel from each wealth class (lower, middle and upper) relative to the number of households from the relevant wealth class in the community census. The dependent variable in all regressions is the hourly wage of the respondent, calculated as the sum of earnings in cash and in kind during the past year from wage employment divided by total hours spent on wage employment. Hours spent in wage employment are computed by multiplying the number of hours worked in a typical day by the number of days worked in a year for each wage labor activity and then summing across all wage labor activities. In column (1) respondent is the primary female of the household, while in column (2) the respondent is the male head of the household. The program effect after two (four) years is the coefficient on the interaction term between the treatment indicator and indicator for the midline (endline) survey wave. At the end of the table, the p-value for the hypothesis test that the two and four year program impacts are equal is reported. All specifications control for the level effect of the treatment, survey waves and subdistrict fixed effects, whether the respondent's household had at baseline any network connection (family, informal insurance, employment or asset transaction) to at least one eligible poor household within their community and the interaction of this variable with treatment indicator and survey wave fixed effects. Standard errors are clustered at the BRAC branch office level. All monetary values are measured in Bangladeshi Takas, deflated to 2007 prices using the annual CPI index published by the Bank of Bangladesh. In 2007, 1USD=69TK.

ONLINE APPENDIX

A Predictions based on Bardhan (1983)

A.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 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[1]. 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 indexed 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

[1]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.

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 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.

A.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)[2].

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 workers hired by the landowner. The net profit of the employer in each peak-season period will be given by[3]:

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

The decision to hire tied workers 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 order condition for 3 yields:

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

[2]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 could assume 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.

[3]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.

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.

A.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, $\tilde{y}_k^i$. 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[4].

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

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

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 worker 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.

[4]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.

A.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 workers to be self-employed or casual workers. 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[5]:

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

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

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

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

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

[5]Note that $W(A) > u^{-1}(\widehat{y})$ as hypothesized.

This gives three equations in three unknowns: $\widehat{y}$, z^* and $W^*(A)$. It is the properties of these equations which are of interest. Figure A1 demonstrates the occupational choices in equilibrium in relation to the distribution of outside options $G(\tilde{y}_k^i)$. In equilibrium, it will be the workers with lowest expected outside options (below $\widehat{y}$) who choose to enter tied-labor arrangements. Workers whose outside options are higher than $\widehat{y}$ will either be casual workers (if the realized wage rate $W(A)$ exceeds their expected outside option $\tilde{y}_k^i$) or they will remain self-employed (if W(A) is below $\tilde{y}_k^i$).

A.5 Comparative Statics

The TUP program corresponds to an exogenous increase in the outside options of a group of workers at the bottom of the distribution $G(\tilde{y}_k^i)$. 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: First, 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. Second, 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.

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 A2 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* $\frac{d\widehat{y}}{d\lambda} \geq 0$.

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

$$\Omega_{3x3} \cdot \begin{bmatrix} dW \\ dz \\ d\widehat{y} \end{bmatrix} = \begin{bmatrix} 0 \\ NG_{\lambda}(u(W)) \\ \frac{\beta}{1-\beta^2}\left(F'\left(\left(\frac{NG(\widehat{y})}{x}\right)\frac{N}{x}G_{\lambda}\left(\widehat{y}\right)\widehat{y}\right)\right) \end{bmatrix} \cdot d\lambda \tag{16}$$

where

$$\Omega = \begin{bmatrix} -\frac{\beta}{1-\beta^2}\int\limits_{\frac{NG(\widehat{y})}{x}}^{\overline{A}} dF(A) & \frac{1}{1-\beta} & 0 \\ -NG'(u(W))u'(W) & 0 & 0 \\ \int\limits_{\frac{NG(\widehat{y})}{x}}^{\overline{A}} u'(W)dF(A) & \frac{u'(z)}{1-\beta} & -\left[\frac{1}{1-\beta^2}+\frac{\beta}{1-\beta^2}\left(F'\left(\frac{NG(\widehat{y})}{x}\right)\frac{NG'(\widehat{y})}{x}\widehat{y}+F\left(\frac{NG(\widehat{y})}{x}\right)\right)\right] \end{bmatrix} \tag{17}$$

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:

$$\Omega^{-1} = \begin{bmatrix} 0 & -\frac{1}{NG'(u(W))u'(W)} & 0 \\ 1-\beta & \frac{\beta(1-\beta)}{1-\beta^2}\int\limits_{\frac{NG(\widehat{y})}{x}}^{\overline{A}} dF(A) & \\ -\frac{u'(z)}{\theta} & -\frac{}{NG'(u(W))u'(W)} & 0 \\ & \mu & -\frac{1}{\theta} \end{bmatrix} \tag{18}$$

where

$$\theta = \left(\frac{1}{1-\beta^2}+\frac{\beta}{1-\beta^2}\left(F'\left(\frac{NG(\widehat{y})}{x}\right)\frac{NG(\widehat{y})}{x}\widehat{y}+F\left(\frac{NG(\widehat{y})}{x}\right)\right)\right) \tag{19}$$

and

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

This implies that:

$$\begin{pmatrix} dW \\ dz \\ d\widehat{y} \end{pmatrix} = \Omega^{-1} \cdot \begin{pmatrix} 0 \\ NG_{\lambda}(u(W)) \\ \frac{\beta}{1-\beta^2}\left(F'\left(\frac{NG(\widehat{y})}{x}\right)\frac{N}{x}G_{\lambda}(\widehat{y})\widehat{y}\right) \end{pmatrix} \cdot d\lambda \tag{21}$$

Hence

$$\frac{dW}{d\lambda} = -\frac{1}{NG'(u(W))u'(W)}NG_{\lambda}(u(W)) \tag{22}$$

$$\frac{dz}{d\lambda} = -\frac{\int_{\frac{NG(\widehat{y})}{x}}^{\overline{A}} dF(A)}{NG'(u(W))u'(W)} NG_\lambda(u(W)) \tag{23}$$

$$\frac{d\widehat{y}}{d\lambda} = \mu \cdot NG_\lambda(u(W)) + \frac{-\beta\left(F'\left(\frac{NG(\widehat{y})}{x}\right)\frac{N}{x}G_\lambda(\widehat{y})\widehat{y}\right)}{\left(1+\beta\left(F'\left(\frac{NG(\widehat{y})}{x}\right)\frac{NG(\widehat{y})}{x}\widehat{y} + F\left(\frac{NG(\widehat{y})}{x}\right)\right)\right)} \tag{24}$$

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 (24) will be non-negative. To evaluate the sign of the second term in (24), 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 (24) is also non-negative. This implies that $\frac{d\widehat{y}}{d\lambda} \geq 0$. ■

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 $u\left(W\left(A\right)\right) \leq \widetilde{y}$, 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 $u\left(W\left(A\right)\right) > \widetilde{y}$, 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 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.

Figure A1: Occupational Choices in Equilibrium

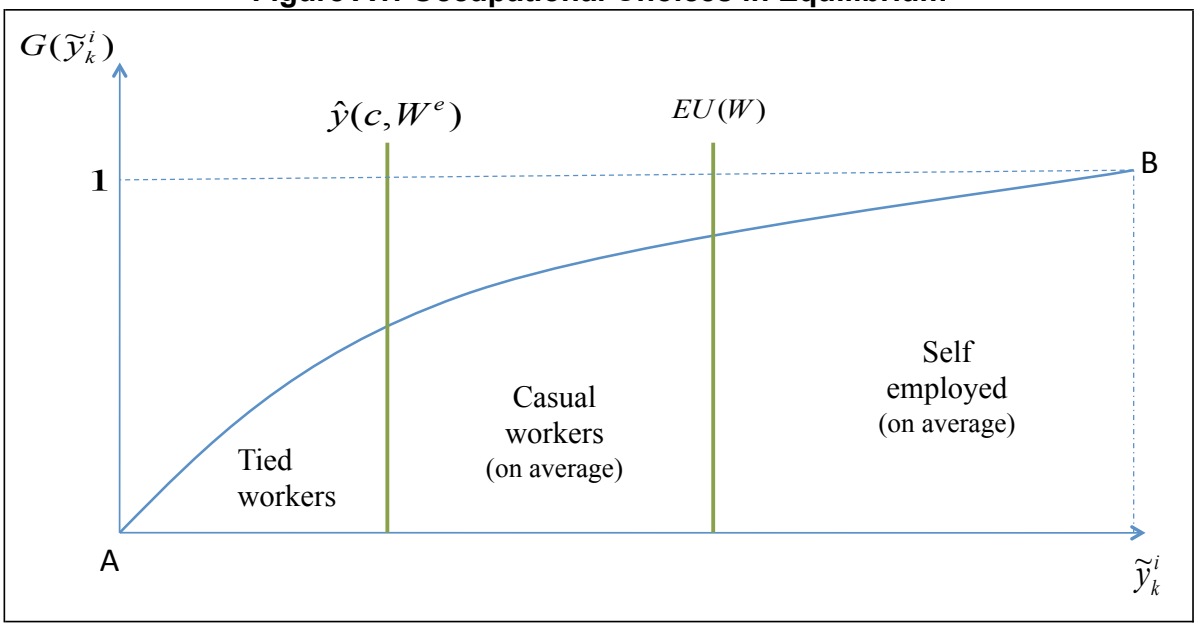


Figure A2: Effect of the TUP Program on the Distribution of Workers' Outside Options

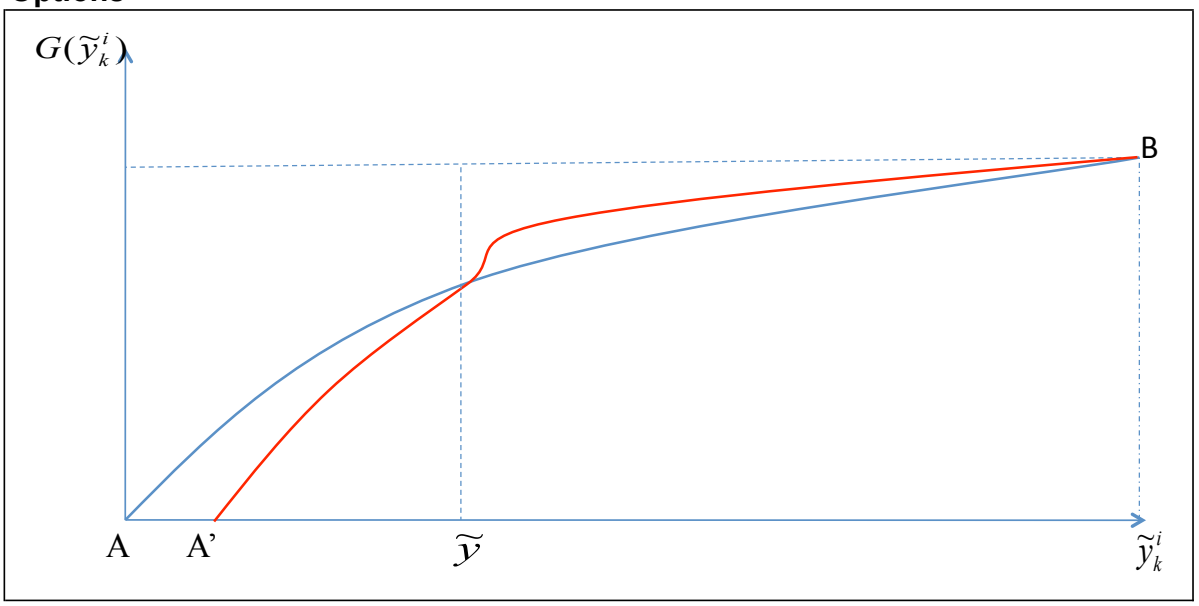


Table A1: Determinants of Non-Attrition

Dependent Variable =1 if Respondent is Surveyed in All Three Survey Waves

Panel A : Eligible Households

	Respondent: Primary female			Respondent: Head of Household		
	Treatment Assignment	Works for a wage [Yes=1]	Receives transfers or loans from employer [Yes=1]	Treatment Assignment	Works for a wage [Yes=1]	Receives transfers or loans from employer [Yes=1]
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment community [Yes=1]	0.014	0.018	0.012	0.019	-0.039	0.015
	(0.011)	(0.017)	(0.011)	(0.015)	(0.043)	(0.015)
Works for a wage [Yes=1]		0.001			0.324***	
		(0.017)			(0.041)	
Treatment x Works for a wage		-0.008			0.067	
		(0.021)			(0.060)	
Receives transfers or loans from employer [Yes=1]			-0.005			0.094**
			(0.023)			(0.039)
Treatment x Receives transfers or loans from employer			0.019			0.078
			(0.028)			(0.049)
Adjusted R-squared	0.003	0.003	0.003	0.036	0.193	0.041
Observations (Clusters)	7953 (40)	7953 (40)	7953 (40)	4608 (40)	4608 (40)	4608 (40)

Panel B: Ineligible Households

	Respondent: Primary female			Respondent: Head of Household		
	Treatment Assignment	Works for a wage [Yes=1]	Hourly earnings from wage-labor [TAKAs]	Treatment Assignment	Works for a wage [Yes=1]	Hourly earnings from wage-labor [TAKAs]
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment community [Yes=1]	-0.011	-0.014*	-0.013	-0.012	-0.025	0.005
	(0.007)	(0.007)	(0.019)	(0.016)	(0.033)	(0.007)
Works for a wage [Yes=1]		-0.026**			0.226***	
		(0.012)			(0.035)	
Treatment x Works for a wage		0.011			0.040	
		(0.018)			(0.051)	
Hourly earnings from wage-labor [TAKAs]			-0.002			0.000
			(0.002)			(0.000)
Treatment x Hourly wage			0.001			-0.000
			(0.003)			(0.000)
Adjusted R-squared	0.007	0.008	0.014	0.044	0.118	0.011
Observations (Clusters)	19012 (40)	19012 (40)	4004 (40)	16557 (40)	16557 (40)	7385 (40)

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. The dependent variable is a dummy variable equal to 1 if the respondent is surveyed in all three survey waves (baseline, midline, endline). Sample is restricted to baseline observations only. In Panel A, the sample includes all eligible households that were surveyed at baseline. In Panel B, the sample includes ineligible households that were surveyed at baseline. In columns (1)-(3), the respondent refers to the primary female in the household; in columns (4)-(6) the respondent refers to the head of household (who may be the same person as the primary female respondent or a different member). All specifications control for the level effect of the treatment and subdistrict fixed effects. Standard errors are clustered at the BRAC branch office level."Receives transfers or loans from employer" is a dummy variable equal to 1 if the respondent is working for an employer who is also reported as a source of food transfers or has given transfers or loans to the respondent during the past year. "Works for a wage" is a dummy variable equal to one if the respondent was engaged in income generating activities where s\he was employed by others. Hourly earnings from wage labor is the sum of earnings in cash and in kind during the past year from wage employment, divided by total hours spent on wage employment. Hours spent in wage employment are computed by multiplying the number of hours worked in a typical day by the number of days worked in a year for each wage labor activity and then summing across all wage labor activities. In 2007, 1USD=69TK.

Table A2: Correlates of Receiving Transfers from Employers at Baseline

	Women's labor contracts		Men's labor contracts	
	Hourly earnings from wage-labor [TAKAs per Hour]	Range of monthly wage earnings [TAKAs per Month]	Hourly earnings from wage-labor [TAKAs per Hour]	Range of monthly wage earnings [TAKAs per Month]
	(1)	(2)	(3)	(4)
Receives transfers or loans from employer [Yes=1]	-0.154*	-111.950***	-0.229**	-6.223
	(0.083)	(43.232)	(0.116)	(97.815)
Mean [std. dev.] of the dependent variable	5.694	1237.87	9.086	2293.22
	[5.694]	[1267.50]	[2.551]	[3009.50]
Adjusted R-squared	0.293	0.084	0.155	0.041
Observations	6179	6179	5966	5963

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. In 2007, 1USD=69TK. The table reports baseline correlations between contract type (tied or casual) and contract terms (wage and regularity). Sample includes baseline observations on eligible poor and near poor households. All regressions control for the following characteristics of the relevant individual [primary female in columns (1)-(2) and male household head in columns (3)-(4)] : whether the individual is literate, individual's age (in years) and age squared, total household wealth (in Takas). In addition, all regressions control for a dummy variable equal to 1 if the Body Mass Index (BMI) of the primary female is below 18.5. The BMIs of male head of the household respondents were often not collected, as such I control for primary female respondent's BMI in columns (3)-(4) as a proxy for the male head of household's nutritional status. Columns (1)-(2) refer to terms of labor contracts of primary female in the household and columns (3)-(4) refer to terms of labor contracts of the male household head. Wage per day (in Takas) which is the sum of earnings in cash and in kind on a typical work day from wage employment. Range of monthly wage earnings is the difference between the maximum and minimum monthly earnings of the respondent from wage employment during the past 12 months. Maximum (minimum) monthly wage earnings is the amount of income that the respondent had from wage employment during the month (in the last year) when wage-labor income was at its peak (lowest). "Receives transfers or loans from employer" is a dummy variable equal to 1 if the respondent is working for an employer who is also reported as a source of food transfers or has given transfers or loans to the respondent during the past year. In 2007, 1USD=69TK.

Table A3: Mechanisms Behind the Effects on Labor-tying -- Outside Option vs Time Constraints

			Volatility of Wage Earnings		
	Receives transfers or loans from employer [Yes=1]	Receives transfers or loans from employer, conditional on working for a wage [Yes=1]	Range of monthly wage earnings [TAKAs per Month]	Maximum monthly wage earning [TAKAs per Month]	Minimum monthly wage earning [TAKAs per Month]
	(1)	(2)	(3)	(4)	(5)
Treatment x midline	-0.029	-0.030	115.062	100.017	-15.045
	(0.023)	(0.035)	(243.972)	(255.627)	(53.254)
Treatment x midline x no of hh members at baseline	0.009	0.033	86.306	146.533	60.227
	(0.017)	(0.024)	(217.879)	(218.935)	(58.691)
Treatment x endline	-0.002	-0.010	19.855	24.990	5.135
	(0.005)	(0.008)	(52.363)	(50.530)	(12.080)
Treatment x endline x no of hh members at baseline	-0.011**	-0.023***	56.219	45.803	-10.416
	(0.004)	(0.008)	(56.885)	(51.691)	(12.734)
Adjusted R-squared	0.023	0.025	0.054	0.050	0.077
Observations	20196	10686	10436	10436	10436

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. The table reports ITT estimates based on a difference-in-difference specification estimated by OLS. The sample includes eligible households who were surveyed at baseline (2007), midline (2009) and endline (2011) surveys. The program effect after two (four) years is the coefficient on the interaction term between the treatment indicator and indicator for the midline (endline) survey wave. At the end of the table, the p-value for the hypothesis test that the two and four year program impacts are equal is reported. All specifications control for the level effect of the treatment, survey waves and subdistrict fixed effects, number of household members at baseline and the interaction of this variable with treatment indicator and survey wave fixed effects. Standard errors are clustered at the BRAC branch office level. All variables are measured at an annual basis and refer to the labor characteristics of the primary female respondent. "Receives transfers or loans from employer" is a dummy variable equal to 1 if the respondent is working for an employer who is also reported as a source of food transfers or has given transfers or loans to the respondent during the past year. Range of monthly wage earnings is the difference between the maximum and minimum monthly earnings of the respondent from wage employment during the past 12 months. Maximum (minimum) monthly wage earnings is the amount of income that the respondent had from wage employment during the month (in the last year) when wage-labor income was at its peak (lowest). In Columns (2)-(5), the sample is resctricted to eligible women who were engaged in wage employment during the last 12 months before each survey wave. All monetary values are measured in Bangladeshi Takas, deflated to 2007 prices using the annual CPI index published by the Bank of Bangladesh. In 2007, 1USD=69TK.

Alternative Explanations

Change in the location of labor-tying

One caveat of the analysis was the limitation of the network map to *within-village* employment and transfer links. In particular, if the treatment (i.e. offer to participate in the TUP program) increased the likelihood of having an employer outside of the community (i.e. outside of the network map) and at the same time shifted labor-tying to these new employers outside the community, then the negative effect on transfers/loans received from employers could be picking up this shift in labor-tying from within-community to outside. If this were the case, one would expect to see an increase in the likelihood that eligible women (and men in their households) work for an employer outside the village *and* an increase in their likelihood to borrow food from someone outside their village. Table A4 provides the results of testing for (i) the effect of the program on the probability of having an employer outside the village (ii) the effect on probability of borrowing food from someone outside the village. Column (1) of the table shows that the program did not have a significant effect on the likelihood that eligible women were working for an employer outside their communities. If anything, the point esitmate at endline is negative but insignificant. Similarly, column (2) shows that the program had no significant impact on the likelihood that male repsondents in eligible households were working for employers outside the village (once again, the point estimates are negative and imprecisely estimated). Column (3) of the table shows that, by the endline survey, the program had a negative impact of 9.7 ppt on the likelihood that eligible households were receiving transfers from outside their village. As such, the impact on labor-tying in eligible poor's contracts is not likely to be driven by the change in the location of labor-tying from within village to outside village employment links.

Fall in overall transfers (not just from employers)

Table A4 also shows the impact of the program on eligible poor's likelihood to receive transfers or loans from other households. Column (4) of the table shows that there was no significant impact on the probability that eligible poor households would report borrowing food from other households. The point estimate is 0.012 (0.003) at endline (midline) survey and imprecisely estimated. This also rules out an alternative explanation based on the program lowering not just labor-tying but ftransfers in general.

Table A4: Effects on Likelihood of Having Employers or Transfer Sources Outside the Network Map

	Main female respondent works for anyone outside the community [Yes=1]	Male hh head works for anyone outside the community [Yes=1]	Household receives transfers or loans from anyone outside the community [Yes=1]	Household receives transfer or loans from anyone [Yes=1]
	(1)	(2)	(3)	(4)
Treatment effect after 2 years	0.010	-0.016	0.047	0.003
	(0.023)	(0.039)	(0.040)	(0.017)
Treatment effect after 4 years	-0.058	-0.036	-0.097**	0.012
	(0.040)	(0.061)	(0.047)	(0.016)
Mean of outcome variable in treated communities at baseline	0.241	0.390	0.058	0.926
Two year impact = Four year impact [p-value]	0.094	0.756	0.062	0.999
Adjusted R-squared	0.044	0.042	0.133	0.023
Observations (Clusters)	20196	11751	20196	20196

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. The table reports ITT estimates based on a difference-in-difference specification estimated by OLS. The sample includes eligible poor households surveyed at baseline (2007), midline (2009) and endline (2011) surveys. The program effect after two (four) years is the coefficient on the interaction term between the treatment indicator and the indicator for the midline (endline) survey wave. At the end of the table, the p-value for the hypothesis test that the two and four year program impacts are equal is reported. All specifications control for the level effect of the treatment, survey waves, subdistrict fixed effects and an indicator variable equal to 1 if the primary female respondent was literate at baseline. Standard errors are clustered at the community level. The dependent variable in column (1) is a dummy variable equal to 1 if the primary female respondent reported working for someone outside the community. The dependent variable in column (2) is a dummy variable equal to 1 if the male head of household reported working for someone outside the community. The dependent variable in column (3) is a dummy variable equal to 1 if any household outside the community was reported as a source of food transfers. The dependent variable in column (4) is a dummy variable equal to 1 if the household received any food transfers from other households.