Effects of Emigration on Rural Labor Markets

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Abstract

Rural to urban migration is an integral part of structural transformation and the development process, but there is little evidence on how out-migration transforms rural labor markets. Emigration could benefit landless village residents by reducing labor competition, or conversely, reduce productivity if skilled workers leave. We offer to subsidize transport costs for 5792 potential seasonal migrants in Bangladesh, randomly varying saturation of offers across 133 villages. The transport subsidies increase beneficiaries' income due to better employment opportunities in the city, and also generate the following spillovers: (a) A higher density of offers increases the individual take-up rate, and induces those connected to offered recipients to also migrate. The village emigration rate increases from 35% to 65%. (b) Statistically weak evidence that this increases the male agricultural wage rate in the village by 4.5-6.6%, and the available work hours in the village by 11-14%, which combine to increase income earned in the village. (c) There is no intra-household substitution in labor supply, but migrants earn more during weeks when they return home, but many of their village co-residents are still away. (d) The wage bill for agricultural employers increases, which reduces their profit, with no significant change in yield. (e) Food prices increase by 2.7% on net, driven by an increase in the price of (fish) protein, and offset by (f) a decrease in the price of non-tradable goods like prepared food and tea. Seasonal migration subsidies not only generate large direct benefits, but also indirect spillover benefits by creating slack in the village-of-origin labor market during the lean season. Offering migration subsidies to some households indirectly benefits others mostly by making it easier for them to also migrate.

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1. Introduction

A shift in labor from rural to urban areas has been an integral part of the process of economic development, and central to theories of long-run growth and structural transformation (Lewis 1954, Harris and Todaro 1970). Migration marked American agricultural development in the 19th century, Chinese development in the late 20th century, and has been a feature of the growth path of virtually every developing country (Taylor and Martin 2001).[1] Understanding the causes and consequences of mobility – both for the migrant, and for the broader rural society – are therefore central to understanding development.

A modern literature links migration to development by carefully documenting that workers are more productive in cities, both within developed (Glaeser and Mare 2001) and developing (Gollin et al. 2014) economies.[2] The accompanying empirical literature has largely focused on the benefits of migration to the migrant and his immediate family (e.g. McKenzie et al. 2010, Garlick et al. 2016), but not the spillover effects on the broader rural economy that are surely central to the links between migration and development. This study attempts to fill that gap by conducting a field experiment in which we randomly vary the fraction of landless households in Bangladeshi villages that are induced to out-migrate temporarily, to generate labor supply shocks of varying magnitudes, and use those to study spillover effects on the rural economy.

The precursor to this paper, Bryan et al. (2014), encourages a sample of 1,292 landless households in rural Bangladesh to migrate during the 2008 lean season using conditional transfers to cover the roundtrip travel cost to nearby cities, and shows that migration significantly improves consumption in induced households. That simple research design can only evaluate the direct effects of migration opportunities on beneficiary households, and does not answer questions about spillover effects on non-beneficiaries. We expand on that design in several ways during the 2014 lean season to study general equilibrium effects on the rural labor market, and in the process, provide a more comprehensive evaluation of a program to encourage migration.[3]

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[1] Long (1991) notes that over 6% of the US population migrates internally within a year, and about 20% of the population of US and Canada move over a 5-year interval. Long-run panel data from India and Bangladesh show that 23 percent of men left their village after 17–20 years (Foster and Rosenzweig 2008).

[2] This is likely due to the benefits of agglomeration (Combes et al. 2010). There is also evidence that cities speed up human capital accumulation, producing growth (and not just level) effects in productivity (Glaeser and Resseger 2010).

[3] We recognize that a complete examination of the welfare and equilibrium effects of migration will need to encompass spillover effects on the urban destinations as well as the origin, but this is beyond the scale and scope of our experimental design, and we therefore reserve this question for future research. Understanding the effects on the origin villages is a valuable piece of the puzzle in and of itself, considering the ambiguous predictions of economic theory.

First, in addition to randomly assigning migration subsidies to an expanded sample of 5,792 poor landless households across 133 villages, our design also randomly varies the *proportion* of the eligible population in the village receiving such offers, because that market-level variation is necessary to track general equilibrium effects on wages and prices. Second, we collect data from both households that receive the randomized offers as well as households that do not, to track spillover effects on the migration and labor supply choices of non-beneficiaries. Third, we collect high-frequency data on earnings and hours worked by week, by location, and by individual worker, to create a richer description of the effects of migration including intra-household adjustments in labor supply. Fourth, we collect data from employers in the village to study effects on market wages, labor costs and profits. Fifth, we collect price data from local shopkeepers to study equilibrium effects on food prices.

We find that temporary emigration generates several different categories of spillovers. First, migration decisions are strategic complements: a larger number of simultaneous migration subsidy offers in a village increases each household's propensity to migrate, and induces others connected to them to migrate as well. Second, those who migrate earn much more. Much of the spillover benefits to non-beneficiaries therefore stem from their own increased propensity to migrate when their neighbors receive subsidies. Third, when larger numbers of people are induced to migrate away, there are some tentative signs of positive spillovers on the village economy: Both the equilibrium agricultural wage rate at home and the available work hours for those who stay in the village increase.[4] We use individual-specific data to explore whether departure of the migrant induces other household members to supply more labor in the village (Rosenzweig 1980), but find that the increase in home-income is mostly due to primary workers earning more when they return home from the city during weeks in which many of their village co-residents are away. Fourth, the increased agricultural wage rate increases the wage bill for employers and reduces their profit. Fifth, food prices in the village increase slightly on net, driven by an increase in the price of fish (the main source of protein), no change in the prices of main staples (like rice and wheat), and offset by a decrease in the price of prepared food and tea at the village tea shops.[5]

[4] These are estimated using variation in emigration rates across 133 villages, and estimation at this level is not very statistically precise. We see the wage result with 90% confidence in a log wage specification.

[5] Bryan et al. (2014) documented an increase in protein consumption in migrant households. It appears that large-scale male migrant departures led to a negative demand shock for tea and snacks in village tea shops.

While we document mostly positive spillover benefits to non-beneficiaries, in theory, migration subsidies could have produced negative spillovers on the villages of origin. It could have undermined agglomeration in the village (Ciccone and Hall 1996; Greenstone et al. 2010). Or if skilled workers disproportionately depart, it could have made those who stay back less productive. Many scholars have expressed related concerns, that migration "may deprive source regions of critically needed human capital," (Greenwood 1997), or "increase rural poverty and income inequality," (Connell 1981). On the positive side, remittances sent by migrants could have enhanced productivity (Rempell and Lobdell 1978), but in our sample, most of the extra income is consumed, not saved. Recent review articles (e.g. Lucas 1997, Foster and Rosenzweig 2008) lament the lack of rigorous evidence on spillovers from migration.[6]

Our results carry important implications for development theory and policy. First, change in both work availability and the agricultural wage rate that we document implies that rural labor supply is not as elastic as labor surplus models (e.g. Lewis 1954) presumed. This evidence adds to the literature on the functioning of labor markets in developing countries (Rosenzweig 1988, Benjamin 1992, Jayachandran 2006, Kaur 2015). Second, our results should encourage policymakers to re-think the various restrictions to internal mobility they have instituted under the guise of rural development policy (Oberai 1983). Anti-migration bias remains rampant in policy circles, and many governments, including China, Indonesia and South Africa, have historically reacted to migration as if "it were an invasion to repel" (Au and Henderson 2006a,b; Simmons 1981). The direct and indirect benefits we document (both for migrants and non-migrants competing in the same labor markets) suggest that this mode of thinking, and the associated restrictions imposed on migrants' transport, settlement and employment by policymakers, may be misguided. Concerns about emigration increasing rural poverty and inequality appear to be unfounded, at least in our context. Third, our results are informative about the choice of whether to pursue rural development (such as rural food-for-work programs or India's NREGA employment scheme) or to invest in infrastructure that improves connectivity between rural and urban areas.

This paper contributes more broadly to the burgeoning economics literature on program evaluation by developing a framework that goes beyond estimation of direct effects on the treated

[6] Pritchett (2006) shows using census data that agricultural, coal mining and cotton farming areas of the United States lost 27-37% of their populations to emigration between 1930 and 1990, but the population exodus was not accompanied by any large decrease in absolute or relative income. Ashraf et al. (2015) describes remittance behavior of Salvadoran migrants to the U.S. more rigorously, but do not attempt to quantify changes on the origin economy.

population. Comprehensive evaluation requires consideration of general-equilibrium changes, especially if we are interested in assessing possible effects of programs when they are scaled up (Heckman, 1992; Rodrik, 2008; Acemoglu, 2010). For example, providing skills training to large numbers of beneficiaries (Banerjee et al. 2007) may change skilled wages, or providing livestock assets on a large scale (Banerjee et al. 2015) may affect livestock prices. Randomized controlled trials examining aggregate effects of equilibrium price changes induced by programs implemented on a large scale are still rare[7], and our results suggest that these considerations might be important. Prior literature on migration has explored indirect effects through a risk sharing channel (Morten 2015, Munshi and Rosenzweig 2016, Meghir et al. 2016), but no study estimates equilibrium effects on the village economy. Cunha et al (2017) studies village price effects of a transfer program.

We describe the problem of seasonality and earlier research on seasonal migration in the next section, and our experiment and data in section 3. Section 4 presents a model of the village labor market with endogenous migration to organize our empirical results on migration, labor supply, earnings, wages and prices. We present empirical results in Section 5. We combine estimates to calculate the aggregate real effects of our intervention on the village economy in Section 6. Section 7 concludes.

2. Context

2.1 Background on Seasonality and Seasonal Migration

Our intervention is designed to enable temporary, seasonal migration. While social scientists and policymakers have noted the pervasiveness of rural-urban migration in both developed and developing societies, the facts that (a) most of this migration is internal rather than international,[8] and (b) much of the internal rural-urban movement is seasonal and circular in nature, are less well known. The rural-urban wage gap varies within the year due to crop cycles, and seasonal migration is one of the primary methods used by Indians (Banerjee and Duflo 2007) and Bangladeshis (Bryan et al. 2014) to diversify income and cope with seasonality. Such seasonal fluctuations in rural labor

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[7] Exceptions are Andrabi et al (2017), Mobarak and Rosenzweig (2016) and Muralidharan et al. (2017). It is more common for RCTs to track non-market spillovers on the non-treated, including financial transfers (Angelucci and DeGiorgi 2009), and social learning (Oster and Thornton 2012, Miller and Mobarak 2015). Crepon et al. 2012 and Muralidharan and Sundararaman (2013) study aggregate effects in relevant markets, but do not estimate price or (teacher) wage effects.

[8] There were 240 times as many internal migrants in China in 2001 as there were international migrants (Ping 2003), and 4.3 million people migrated internally in the 5 years leading up to the 1999 Vietnam census compared to only 300,000 international migrants (Ahn et al. 2003).

productivity have been documented in Ethiopia (Dercon and Krishnan 2000), Thailand (Paxson 1993), Indonesia (Basu and Wong 2015), Malawi (Brune et al. 2016) and Ghana (Banerjee et al. 2015). Seasonal migration also appears to be more responsive to policy interventions and to changes in local labor market conditions than permanent migration (Imbert and Papp 2015).

Globally, approximately 805 million people are food insecure (FAO 2016), of which about 600 million are rural residents. Estimated conservatively, half of these people—300 million of the world's rural poor—suffer from seasonal hunger (Devereux et al. 2009). In predominantly agrarian economies, seasonal deprivation often occurs between planting and harvest, while farmers have to wait for the crop to grow. Labor demand and wages are low during this period, and the prices of staples like rice tend to increase. These two facts combine to produce a dire situation in the Rangpur region of Northern Bangladesh, where rice consumption drops dramatically during the lean season.[9] This is an annually repeating phenomenon known as "monga" in Bangladesh, and by other names in other agrarian societies around the world ("hungry season" in southern Africa (Beegle et al. 2016), and "musim paceklik" in eastern Indonesia (Basu and Wong 2012)). The landless poor supplying agricultural labor on others' farms are especially affected when demand for agricultural labor falls. They constitute around 56% of the population in our sample area, and are the target of the seasonal migration encouragement intervention that we design. Our sampling frame is representative of this landless population in the Rangpur region of Northern Bangladesh. According to the Bangladesh Bureau of Statistics, there are roughly 15.8 million such inhabitants in Rangpur (BBS, 2011).

According to anthropological accounts, nearby urban and peri-urban areas do not face the same seasonal downturns, and these locations offer low-skilled employment opportunities during that same period (Zug, 2006). This contrast suggests a seasonal labor misallocation, or a spatial mismatch between the location of jobs and the location of people during that particular season.

Inspired by these observations, Bryan et al. (2014) conduct a randomized controlled trial to encourage landless households from the Rangpur region facing seasonal deprivation to migrate during the Monga period to nearby cities to find work. They document positive effects of migration on consumption, and then explore why these households were not already migrating. A conditional transfer of about \$8.50-\$11 (equivalent to the round-trip travel cost by bus) increases the seasonal migration rate in 2008 by 22%, increases consumption amongst the migrant's family members by

[9] Figure A.1 uses nationally representative Household Income and Expenditure Survey (HIES) data collected by the Bangladesh Bureau of Statistics to illustrate these facts. Figure A.2 shows the drop in labor hours and earning capacity in the agricultural sector during the pre-harvest lean season using a different data source (Khandker and Mahmud 2012).

757 calories per person per day in 2008 on average, and also induces 9.2% of the treated households to re-migrate the following year.

Bryan et al. (2014) attempt to explain these observations using a model in which people living very close to the margin of subsistence are unwilling to take on the risk of paying the cost of migration and sending a member away. Even a small chance that the costly migration fails to generate income could be catastrophic if the household faces a risk of falling below subsistence. Thus, uninsured risk creates a poverty trap in which the extreme poor fail to take advantage of migration opportunities that turn out to be profitable on average. A conditional transfer can address that constraint and create efficiency gains.

2.2 Studying Spillover Effects of Seasonal Migration

Bryan et al. (2014) only focused on households that received migration subsidies, not the spillover effects on non-beneficiaries, or any general equilibrium changes associated with increased scale of emigration. Consideration of general equilibrium effects requires a fundamentally different, and more complicated, data collection and experimental strategy, which we employ in this study.

To study market-level effects, the scale of our experiment is five times as large, and we further randomize the proportion of the village population induced to migrate. We also employ a richer data collection strategy: (a) Track both households that receive these offers and eligible households supplying labor on others' farms that do not, (b) a labor survey of every member of the household, (c) a survey of landowning employers and business owners who hire landless workers and (d) a survey of grocers to track food prices. These market-level considerations are policy relevant, because implementers and funding agencies are advocating for and deploying seasonal migration subsidies on a large scale as a social policy to counter seasonal poverty (Evidence Action 2016). For instance, Evidence Action's "No Lean Season" program targets 200,000 households in rural Bangladesh in 2017 to induce 140,000 moves, and was piloted at a smaller scale in Indonesia. Decisions on scale up should be guided by evaluations of both direct and indirect effects. China promotes some temporary migration programs connecting rural workers to urban factory jobs (Demirtepe and Bozbey 2012).

3. Experiment and Data

The next two sub-sections set out the details of the experiment and the data collection. Figures 1 and 2 provide a visual account of the main features of the experiment and the type and timing of data collection.

3.1 Intervention

The basic form of our intervention was the offer of a cash grant worth 1,000 Taka ($13.00 USD) to rural households in northern Bangladesh to cover the round-trip cost of travel to nearby cities where there are job opportunities during the lean season. This was a conditional transfer, where the subsidy is conditional on one person from the household agreeing to out-migrate during the lean season. As offers were made, we let households know that they may have a better chance of finding work outside of their village, but we did not offer to make any connections to employers. No requirement was imposed on who within the household had to migrate, or what city they had to go to. As in Bryan et al. (2014), migration was carefully and strictly monitored by project staff to ensure adherence to the conditionality.

3.1.1 Sampling

The experiment was conducted in 133 randomly selected villages in Kurigram and Lalmonirhat districts of Rangpur. We first conducted village censuses to identify all households that would be "eligible" to receive this intervention in each of these villages. A household was deemed eligible if (1) it owned less than 0.5 acres of land, and (2) it reported back in 2008 that a member had experienced hunger (i.e., skipped meals) during the 2007 monga season. We focused on landownership because land is the most important component of wealth in rural Bangladesh, and it is easily measurable and verifiable. We used the second question on skipping meals to avoid professional, non-agricultural households (who may not own much land, but who are comparatively well off). Our census data suggest that about 57% of households in these villages were eligible to receive the intervention after applying these two criteria.

3.1.2 Random Assignment

We randomly assigned the 133 villages into three groups:

(a) Low Intensity – 48 villages where we targeted migration subsidies to roughly 14% of the eligible (landless, poor) population. This is comparable to the Bryan et al. (2014) treatment.

(b) High Intensity – 47 villages where we targeted roughly 70% of the eligible population with migration subsidy offers.

(c) Control – 38 randomly selected villages where nobody was offered a migration subsidy.

The high vs. low intensity design was chosen to generate significant variation in the size of the emigration shock, but the precise target (14% vs 70%) varied a little across villages within treatment arms.[10] This is because our village population estimates were dated (from 2008) for most (100) villages, and imprecise in the 33 other villages, which made it difficult for us to precisely estimate the ratio (offers/eligible population) in each village.

The sample of 133 villages included the 100 villages that were part of the earlier Bryan et al. (2014) experiment, but the majority of the households in our sample are new, and were not included in the earlier experiment. We show in Appendix Tables A2-A5 that participation in the earlier rounds of the experiment has no significant effect on migration decisions this year, and therefore does not materially affect the main results reported in this paper. In other words, there are no detectable long-term effects of the original 2008, 2011 and 2013 experiments on the subset of households in our sample that happens to overlap with the 2008-2013 experiments. Controlling for village level random assignment in the earlier rounds does not affect our results either. The fact that that the effects of 2008 or 2011 interventions do not persist beyond a 5-7 year horizon is interesting in and of itself, but we show these results here mainly to clarify that we interpret the downstream effects of migration on income, labor supply and other outcomes that we show below to be the result of "new" migration from the 2014 treatments, and not related to the longer-run effects of earlier rounds of treatment.

Landless households are engaged in both agricultural and non-agricultural work. We had provided experimental instructions to target non-agricultural households first in some (randomly chosen) villages, and our randomization of low vs high intensity was stratified and perfectly balanced by this instruction. During implementation we learned that in reality most households supply labor to some form of agriculture. We show in Tables A2-A5 that the stratification had no effect on migration decisions, nor does it affect our estimates of the effect of treatment intensity on migration or income outcomes.

[10] In our project planning documents and in previous drafts of this paper, we labeled our "High Intensity villages" as "50% villages" (and "Low Intensity" as "10% villages"), because we expected that offering subsidies to 70% (14%) of eligible households would result in a take-up rate of roughly 50% (10%). However, this was used merely as shorthand for our expectation, and therefore the "50%" and "10%" terminology has now been replaced with "High Intensity" and "Low Intensity" for clarity.

There were a total of 883 subsidy offers made in the 48 low-intensity villages and 4,881 subsidy offers made in the 47 high-intensity villages. The total number of households resident in these 133 villages was 36,808.

3.1.3 Timing, Protocol and Logistics

We disbursed grants during the latter part of the monga season, in early November, 2014. Figure 2 provides a timeline of project activities. Ideally, seasonal migration subsidy offers should be made in September after the rice planting work is done. Despite this delay, we observe high overall take-up and migration during the late Monga, as well as some post-harvest migration after January. We also report results on re-migration a year later, covering the full 2015-16 migration season.

All of the implementation activities – the offers and marketing, grant disbursement, and monitoring to ensure adherence to the conditionality, were conducted by RDRS, a local NGO with 40 years of engagement in Rangpur, and substantial presence in the region. RDRS runs a microfinance program among other poverty alleviation activities, and this expertise was useful to handle the disbursement of grants, and to ensure recovery of funds in cases of non-compliance with the condition associated with this grant.

Innovations for Poverty Action in Bangladesh (IPA-B) coordinated all research activities and was responsible for testing and fielding surveys, collecting, cleaning and maintaining data. They also monitored RDRS' implementation activities to ensure that they were conducted in accordance with the research protocol.

After the research team conducted the sampling and randomization, they provided RDRS staff with a list of eligible households in the village and their treatment assignments, and RDRS staff were deployed to the village to implement the intervention. Staff members approached each specific household on their list and first verified that they satisfied the eligibility criteria. Then the household was offered the grant to migrate, and the conditionality was stated explicitly. The head of the household was told that if it accepts the grant, one member would have to use it toward migration travel expenses, and that this would be monitored. Households were also informed that nearby areas may offer better chances of employment than their home village.

Once the conditions of the offer were explained clearly, the household was provided guidance on how to collect the grant funds from their local RDRS office. The RDRS staff member collected identification information from the household. If the beneficiary visited the RDRS office to collect the grant, an officer verified their identity before disbursing funds. The grant amount

(1,000 Taka) was large enough to cover the cost of a round-trip bus ticket to nearby popular urban destinations, with some money left over for a few days of board and lodging.[11]

RDRS carefully monitored adherence to the conditionality. After disbursement of funds, an RDRS officer visited each household to check whether someone had migrated or not. If no one had migrated at the time, the officer reminded the head of household that the grant he had received was conditional on migration and that if no household member were to migrate he would be required to return the funds. The officer made two more visits to the households that had failed to send a migrant, and requested that funds be returned if migration still had not taken place.

3.1.4 Note on a Failed Experiment in 2013

While the current study refers to the experiment begun in 2014, a similar experiment was attempted a year earlier in 2013. This was unfortunately a failed experiment, in that an unprecedented wave of political strikes ("hartals") and strike-induced violence in Bangladesh in late 2013 – precisely during the months when rural households typically migrate – led to both implementation difficulties for us, and also made it costly and risky for our beneficiaries to migrate. Hartals are used to shut down roads, buses, railways and all other forms of public transportation, as a way for the opposition party to deter economic activity. This naturally creates safety risks associated with movement. Ahsan and Iqbal (2016) code all occurrences of hartals and violence between 2005 and 2013 using newspaper reports, in order to document the effects of strikes on Bangladeshi garment exports. Their data show that the number of strikes in 2013 was comparable to the *combined total* of the previous 8-year period. They note that hartal-related deaths in 2013 likely exceeded the combined death toll from *all* previous hartals over the entire history of Bangladesh.

While the violence and disruption are likely the main deterrents to migration (because it creates uncertainty about the possibility of returning to the village), Ahsan and Iqbal (2016) also directly calculate that the cost of transportation rises as much as 69% on hartal days. The strikes and the hartal-induced violence were particularly concentrated near the end of 2013, coinciding with the period immediately after migration subsidies were disbursed, which, according to results from other rounds of study, is one of the most popular periods for seasonal migration. We were unable to enforce the migration conditionality during such a difficult period. We re-drew a new household

[11] We considered the possibility of providing bus tickets to migrants, but the logistics of contracting with multiple transport companies, and finding flexible means to match transporters to migrants were too daunting. Previous experience also suggested that it was possible to get beneficiaries to adhere to the migration condition, so we settled on cash transfers.

sample in 2014 to conduct the new experiment that is reported on in this paper. As Appendix A2-A5 show, the 2013 failed experimental attempt had no detectable effect on households' responses to the 2014 interventions reported in this paper. For these reasons, we consider the 2013 RCT to be a separate, failed experiment on a different sample, conducted during an extremely unusual year, and focus only on the 2014 data in this paper.

3.2 Data Collection

We conducted four separate types of surveys in 2014-15 to capture effects on labor market choices, other household impacts, effects on employers, and effects on food prices. We conducted two additional surveys a year later (after the lean season in the following year) to capture longer-term persistent effects on households and employers in 2015-16, i.e. two years after the initial intervention (November 2014 to September 2016). Figure 1 depicts sample sizes by experimental cell, Figure 2 lays out the timeline of data collection and intervention activities relative to the agricultural season.

3.2.1 High Frequency Labor Market Survey of Households

After the travel grants were disbursed in November 2014, we started surveying 2,294 households in both treatment and control villages about their wage and employment conditions. The survey was administered once every 10 days for six rounds starting on December 22, 2014. We refer to this as "High Frequency Origin Surveys". The survey asked respondents about labor market outcomes (income, time spent working, location, industry) and a brief set of questions on consumption (essential food and non-food items) and migrant remittances.

We focus on income and labor market outcomes given our interest in general equilibrium effects, in contrast to Bryan et al. (2014), who largely focused on consumption to evaluate the direct effects of inducing migration. Income is generally thought to be more difficult to measure well in rural, agrarian areas of low income countries due to seasonal variation, multiplicity of sources of income, weekly variation in activities over the course of the agricultural cycle, self-employment and family employment (Deaton and Muellbauer 1980). This is why we engage in a very expensive method of surveying, visiting households six times on an almost weekly basis and asking about income-generation activities of all household members over only the previous week to minimize recall bias. We also conduct the surveys during a narrow two-month window during which seasonal and employment variation is minimized. The surveys focus on landless households that have minimal self-employment or unpaid family employment on their own farm. This provides us with labor supply choices of all working individuals within each household, the location where they work (inside the village or at migration destinations), and how much they earn on a daily basis.

This method of surveying produces some ancillary benefits. First, it allows us to track high-frequency movements back and forth between the village and the city. Many migrants travel for only 2-4 weeks at a time and engage in multiple trips during the season. We observe 1.6 trips per migrant on average in our data. Second, the technique also allows us to track intra-household substitutions in labor supply, because we collect data at the individual level. Third, it allows us to cross-validate the direct (income) effects of migration that we estimate, with the consumption outcomes Bryan et al. (2014) collected using a completely different surveying method six years prior, but administered on a similar population chosen using the same sampling frame. The magnitudes of income and consumption effects need to be coherent. Fourth, we can also validate our income estimates from the high-frequency survey using income measures collected in the endline household survey we ourselves conducted a few months later.

The high-frequency surveys were administered to 709 households that did not receive migration offers in treatment villages, in addition to 865 households that did (plus 720 households in control villages). Our goal was to track whether offers to a certain sub-group of households lead others to migrate, and track any spillover income and employment effects on those households either at home or at the destination.

3.2.2 Food Price Data: High Frequency Survey of Shopkeepers

We paired the brief consumption module in the high-frequency survey described above with repeated surveys of 399 shopkeepers (i.e. grocery store owners), or three in each of the 133 villages in our sample. These were administered simultaneously with the consumption module to collect prices for the same food items that the consumption module asked households about. We collected data on the prices of major food items, including rice, wheat, pulses, edible oil, meat, fish, eggs, milk, salt and sugar. These data allow us to explore whether encouraging migration at large scale in a village (and the extra income that generates) leads to price effects on food markets. It also allows us to convert the food consumption effects into monetary values.

3.2.3 Endline Survey

Next, we conducted a detailed endline survey of 3,600 households during April 2015, before the start of the next rice-planting season. Figure 1 displays the sample breakdown across treatment arms and across types of households (those who were offered grants and those who were not). This endline survey collected information on a broader set of questions on migration and other socioeconomic outcomes that were not sensible or possible to ask repeatedly on a weekly basis, as in the high frequency survey. Core modules focused on collecting detailed information on the migration experience, including number of members who migrated, timing of migration events and destinations. The survey also delved into income generated by households (especially from migration), behavior and attitude changes, risk coping, credit and savings.

3.2.4 Employer Survey

To measure impacts on the demand side of the labor market, we asked 1,099 employers across all villages about the wages they paid for employees around (and after) the time that we disbursed migration grants. We also asked employers to provide qualitative assessments of the ease of finding and hiring workers during that period. We collected data on wages for multiple activities in both agricultural and non-agricultural sectors, separately for males and females hired. Almost all seasonal migrants are male. Unlike the high-frequency wage survey, the employer survey was retrospective, and asked employers to recall wage and employment conditions for every two-week period starting mid-October through the end of December 2014. We are confident of high quality recall because (a) our survey referred to wages paid for specific agricultural activities (e.g. for planting or for harvest), (b) employers tend to maintain records for their businesses, and (c) survey staff were trained to prompt employers with cues on types and timing of events (e.g. associating the timing of a given employment activity with a significant cultural or religious event).

3.2.5 Follow-up Surveys 2016

To study the longer-term behavior of households, we conducted a follow-up survey in August 2016 enquiring about a number of items over the time period beginning mid-August 2015 through mid-August 2016. This survey included questions on migration – specifically, timing and number of episodes, income from migration and questions about resource sharing by migrants – and the household's experience of hunger over the previous year. This was administered to the original endline sample from the 2014-2015 round of study and we were able to effectively re-interview 94% of the sample (3,382 of the original 3,600 households). This 6% attrition was not systematically different in treatment versus control villages. The migration subsidy program was not implemented again during the 2015 monga season, so this survey captures any longer-run changes from the intervention carried out during the prior lean season.

The second component of our August 2016 longer-term follow-up survey work targeted the demand side of the labor market, i.e. employers. We administered a labor demand and wage survey to agricultural employers to better understand the impacts of emigration on their enterprise and decisions. The employer labor demand and wage survey was administered to 649 employers across all 133 villages.

4. Framework

4.1 Offer Intensity and Migration

Our theory characterizes the response of rural labor markets to labor supply shocks (migration). We define a village as the local labor market with two types of households:

a. Landless households that supply labor
b. Landed farmers that hire labor

Our intervention targeted landless households by design. In any given village, a proportion x of landless households was offered a travel grant, $B > 0$. x was experimentally varied. A landless household decides to send a migrant if the net benefits of migration are greater than wage income from the village labor market,

$$w^m + B - F_I - F_S(x) \geq w(x) \qquad (4.1.1)$$

Where, w^m is wage at migration destination, $B > 0$ for those who receive the conditional migration subsidy offer (and *B*=0 for those who do not), $F_I \sim G(.)$ is the individual specific cost of migration, F_S is the cost of migration that can be shared with other migrants (hence a function of x) and w is the village wage which can be affected by the number of departures (hence also a function of x). w^m is not a function of x, because our experimental sample of 5,764 offers was not large enough to affect equilibrium wages in any of the destination cities.

For the proportion x of households that receive the grant *B>0*, the probability of migration can be expressed as,

$$\Pr\big(F_I \leq w^m + B - F_S(x) - w(x)\big) = G(w^m + B - F_S(x) - w(x)) \qquad (4.1.2)$$

For the remaining $(1 - x)$ unincentivized households the probability of migration is,

$$\Pr\big(F_I \leq w^m - F_S(x) - w(x)\big) = G(w^m - F_S(x) - w(x)) \qquad (4.1.3)$$

This yields an aggregate migration rate in a village, $M(x)$,

$$\mathrm{M}(x) = x \cdot \mathrm{G}(w^m + B - F_S(x) - w(x)) + (1 - x) \cdot \mathrm{G}(w^m - F_S(x) - w(x)) \qquad (4.1.4)$$

First derivative of the above expression yields the change in migration rate as a function of our field experiment:

$$M'(x) = [\mathrm{G}(w^m + B) - \mathrm{G}(w^m)] + \left(-\frac{\partial F_S}{\partial x} - \frac{\partial w}{\partial x}\right) Z \quad (4.1.5)$$ [12]

For any $B > 0$, the first term on the right-hand side is positive and denotes the proportion of the population that is not infra-marginal (households that are induced to migrate by the transfer B). This is the first order effect of providing subsidies to more people on the migration rate. The first part of the second term, $\frac{\partial F_S}{\partial x} < 0$, denotes how the shared cost of migration decreases as more people from the village are offered travel grants simultaneously, which in turn boosts the migration rate when more offers are made. The second part of the second term, $\frac{\partial w}{\partial x}$, may lower the subsidy take-up rate of individuals due to the general equilibrium wage effect in the village. The sign of the second term, $\left(-\frac{\partial F_S}{\partial x} - \frac{\partial w}{\partial x}\right)$, depends on whether having more migrants from the village reduces the cost of travel (by permitting sharing) by more than the benefits of staying back at home (to take advantage of the fact that lean season wages will not fall by as much when many other people in the village emigrate). The relative sizes of these two factors are testable in our setting: We can compare how each individual receiving a migration subsidy (B) in the low- versus high-intensity village responds to the offer. The response to the exact same offer of B will be stronger in the high intensity village if $\left(-\frac{\partial F_S}{\partial x}\right)$ is larger in magnitude than $\left(\frac{\partial w}{\partial x}\right)$.

4.2 Effects of the Experiment on the Equilibrium Wage Rate and Food Prices

Appendix 1 models the labor supply and demand sides to show how this experiment can affect the wage rate and labor supply in the village. Since $M'(x) > 0$ (more people leave when more subsidies are assigned), the village wage rate rises in equilibrium with higher intensity of treatment: $\frac{\partial w}{\partial x} > 0.$ This leads to an attendant rise in labor supply among those remaining in the village. The appendix model makes clear that this simple logic assumes that employers don't react to the treatment in the short-run by changing their production function and (for example) substituting capital for labor. This is probably a reasonable assumption in the short run, and we collected data from employers to check whether it is true.

[12] $Z = xg\big(w^m + B - F_S(x) - w(x)\big) + (1 - x)g\big(w^m - F_S(x) - w(x)\big)$ is a positive number.

The departure of migrants may lower the demand for food in the village, whereas the extra income they bring back could increase the demand for food. If food markets are not well integrated across space, then food prices could either decrease or increase as a result. With integrated markets, supply would adjust, and there should not be any detectable change in food price.

4.3 Model Implications

The model highlights the following channels of spillovers:

1. The propensity to migrate may differ depending on how many others are simultaneously moving. Bryan et al. (2014) argue that risk aversion is a deterrent to seasonal migration, which could make migration decisions strategic complements if traveling together mitigates risk. On the other hand, general equilibrium wage effects in villages of origin would make migration decisions strategic substitutes.
2. Wages and labor supply in villages of origin increase with a larger labor supply shock, assuming that the village is a closed labor market, and village employers cannot change their production technology in the short run.
3. If food markets are not well integrated, local food prices may change with the movement of people or the extra income. However, if markets are well integrated food prices will not rise.

5. Empirical Results

5.1 Migration

We first examine the effects of subsidies on migration decisions. We report intent-to-treat (ITT) estimates that compare the five categories of people that our experiment creates (see Figure 1): (a) Landless laborers who receive subsidy offers in high-intensity treatment villages, (b) Landless laborers residing in the same villages who do not receive an offer, (c) Those receiving offers in low-intensity treatment villages and (d) Those in low-intensity treatment villages who do not, and finally (e) residents of control villages (none of whom receive offers). The ITT regression therefore reports four coefficients of interest corresponding to groups (a)-(d), with the control villages serving as the omitted category:

$$M_{ivj} = \alpha + \beta_1 Offer\_HI_{ivj} + \beta_2 NonOffer\_HI_{ivj} + \beta_3 Offer\_LI_{ivj} + \beta_4 NonOffer\_LI_{ivj} + \varphi_j + \varepsilon_{ivj} \quad (5.1.1)$$

The main outcome variable in Table 1, column 1 (denoted M_{ivj}) is a binary variable that indicates that household i in village v in sub-district j sent a seasonal migrant between September 2014 and March 2015. This regression relies only on the random variation generated by the experiment.[13] Standard errors are clustered at the level of randomization (village level) throughout this paper. We control for sub-district fixed effects φ_j to be consistent with Bryan et al. (2014), but results remain similar when omitting those controls. The comparison between groups (a) and (c) (coefficients β_1 and β_3) tells us whether two households react differently to the exact same subsidy if the number of *other* village residents receiving subsidies at the same time differs. Similarly, the comparison between (b) and (d) (coefficients β_2 and β_4) indicates whether two households facing the same full cost of migration have different propensities to migrate depending on how many other village residents are likely to migrate simultaneously. These two comparisons represent our main test of whether migration decisions are strategic complements, or a test of whether cost or risk-sharing considerations at the destination dominates general equilibrium wage considerations in the village of origin. Table 1 reports p-values for these tests in the bottom two rows.

About one-third of the households in control villages sent a seasonal migrant (34.2%), which is the same as what Bryan et al. (2014) and Khandker and Mahmud (2012) find in multiple years using other datasets. Households offered a grant in the low-intensity group were 24.8 percentage points more likely to migrate than a household in the control group, where no grant offers were made.[14] In contrast, households offered a grant in the high-intensity group had a 39.8 percentage point higher propensity to migrate compared to the control group. This large difference in their reactions to the exact same offer is statistically significant (p-value<0.001). This suggests that migration decisions are strategic complements: A household is significantly more likely to take up the subsidy offer and migrate if a larger number of other village residents have the opportunity to travel simultaneously. In terms of the model, the $\left(-\frac{\partial F_S}{\partial x}\right)$ term dominates $\left(\frac{\partial w}{\partial x}\right)$.

[13] Appendix Table A1 conducts randomization balance tests. Villages assigned to Control, Low Intensity and High Intensity treatments are well balanced along observables at baseline. Controlling for any individual imbalanced variable does not affect any of our main results. Baseline data were collected separately in two sets of villages, and we check for balance within each sub-sample.

[14] Bryan et al. (2014)'s simpler design only reports results comparable to our low-intensity treatment. In low-intensity villages, we make offers of grants to 18 households per village on average, which is comparable to their 19 offers per village in Bryan et al. (2014). They report a 24 percentage point increase in migration rates, comparable to the 24.8 percentage points here.

This positive spillover even extends to those not directly receiving migration offers. Households that did not receive an offer, but were located in a high-intensity village had a 9.7 percentage point greater propensity to migrate than households in control villages. Such spillovers are not apparent in the low-intensity treatment.

In columns 2 and 3, we change the dependent variable from "any migrant in a household" to "Number of unique migrants sent by a household" and "total number of migration episodes generated by a household". The effect size for "number of migrants" is very similar to that of "any migrant", which indicates that the treatment mostly had an extensive margin effect (inducing non-migrant households to send a migrant), and not much of an effect on the intensive margin (inducing migrant households to send an additional migrant). We offered to subsidize only one trip per household. The effect size for migration episodes is 1.6 times as large as the 'any migrant' effect, which indicates that the induced migrants migrate for multiple episodes during the season.

The fourth column uses data from the follow-up survey conducted two years after the intervention, which enquires about longer-run migration decisions during the next lean season (2015-16) for which migration incentives were no longer provided. We see persistent effects: households that received subsidy offers a year before along with many of their village co-residents (in the high-intensity treatment) were 29 percentage points more likely to re-migrate a year later. Households that received offers the previous year along with few of their village co-residents (i.e., low-intensity treatment) were 19 percentage points more likely to re-migrate relative to the control group. The reactions in the two types of villages are significantly different from each other (p-value<0.01). In both cases, the effect size is about 75% as large as the original migration effect in 2014-15, which suggests that about 75% of the offered migrant households chose to re-migrate on their own volition the following year. Unlucky households not offered grants in the high-intensity treatment villages the year before demonstrated very strong persistence: they were 13 percentage points more likely to re-migrate in 2015-16 relative to the control group.

5.1.1 Sources of Complementarity in the Migration Decisions

We use two additional pieces of data to explore the source of these spillovers. First, prior to treatment assignment, we collected data on social network relationships between sample households, starting with a simple question on whether they know the other people in the sample.[16] In column 1

[16] These data were collected for a subset of our respondents for an earlier study, and we were able to match 998 households from our endline sample to this earlier dataset with network information. Respondents to this module were asked whether and to what extent they knew each of 18 (randomly selected) other households from within their village.

of Table 2, we employ this question to examine migration decisions, while separating each treatment group into households that know another sample household that received the randomized offer, and those that do not. This creates a total of 8 cells: [High/Low Intensity]x[Not Offered/ Offered]x [Knows/Does not Know]. We control for the fraction of people that the respondent knows, in order to hold constant how social the respondent is, and use only variation in whether they happen to know another person who got lucky and received a migration subsidy. We find that if the household received an offer itself, then its migration propensity was equally high regardless of whether it knew another household that received an offer. However, if the household got unlucky and did not get an offer, then it only migrated if it knew another household that received an offer. In other words, the spillover effect is only present if the household knows others with offers.

The second column uses richer variation from the social network relationship data, where we asked each household to identify the subset of other households it could rely on in times of need. The spillover effect on the household's migration is much stronger if another household with which it has a strong connection ("can be relied on") gets lucky and receives an offer. These results are suggestive of a conscious, coordinated process by which the complementarity in migration decisions arises. If households coordinate more with those it can rely on, then risk may indeed be an important impediment to migration.

In Table 3, we run a regression that characterizes how our low versus high intensity treatments affected the propensity to share accommodation costs at the destination or travel with others. 90% of migrants report traveling with companions, and providing migration subsidies to many villagers simultaneously (in the high-intensity treatment) significantly increases the number of travel companions, as we would expect from increasing the number of migrants. It has a smaller effect on the number of migrants sharing housing, which again suggests that risk mitigation rather than sharing costs was the primary motivator.[17] In this context, accommodation is typically not provided by employers.

We show evidence here of merely two of the mechanisms by which migration decisions across households may be inter-linked. There may be other ways in which household decisions are inter-connected (e.g. through gifts and transfers), and this could be further explored.

[17] In qualitative interviews, one group of migrants demonstrated real organization to their travel, having designated one member of the group general caretaker. This caretaker was responsible for some key logistical tasks (such as procuring and cooking food for the group) and as such was not expected to work at the destination. Rather, the group of migrants paid him a salary for his role.

5.1.2 Village Level Migration Rates

These individual level reactions to the migration subsidy offers across "offered" and "non-offered" households aggregate up to a village level migration response, which essentially serves as a "first stage" for our general equilibrium analysis. The first stage is likely to be strong, not only because we made many more offers in the high-intensity treatment, but also because each household in the high-intensity villages reacted more strongly to the offers.

Table 4 reports the results of the following village-level regression (133 observations) to examine the proportion of the village population (M_{vj}) induced to move by each of our treatments:

$$M_{vj} = \alpha + \delta_1 Low\_I_{vj} + \delta_2 High\_I_{vj} + \varphi_j + \varepsilon_{vj} \tag{5.1.2}$$

35% of eligible (landless poor) households send a seasonal migrant in control villages, and the high-intensity treatment increases the migration rate in those villages by a further 30 percentage points to 65%. This represents a sizable labor supply shock. In contrast, the migration rate among eligibles in the low-intensity villages increases by only 7 percentage points to 42%.[18] We focus on the population eligible for our subsidies (the landless who face seasonal hunger) for these calculations, because that group represents the relevant labor market of workers who supply labor on other people's farms, or work as day laborers as seasonal migrants. The second column shows effects relative to the total village population: the high-intensity treatment induces an extra 11.5% of the population to emigrate from a base of 21% in control villages, which represents a 50% increase in the emigration rate.

Since a full village listing was conducted in only 117 of the 133 sample villages, our data on the denominators for the other 16 villages (the total and landless population) used in these regressions are based on a knowledgeable villager's estimates, and therefore imperfect. Columns 3 and 4 restrict the sample to the 117 villages with high-quality information, and the results remain strong (the high-intensity treatment induces an extra 28% of the landless population, or 15% of the total population to emigrate). To be careful, our subsequent analysis reports the important second stage estimates for both the full (133 village) and partial (117 village) samples.

[18] These village level emigration rates are consistent with the individual level responses reported in Table 1. 70% of eligible households were offered subsidies in the high intensity villages, and migration propensity estimates for offered (0.398) and non-offered households (0.097) from table 1 suggests that an extra 30% of the eligible population of the high intensity villages migrated, which is almost identical to the 30.4% estimate in table 4. The estimate for the low intensity village also matches closely.

Finally, we examine the 2015-16 re-migration rate at the village level in column 5, a year after the incentives were removed. The high-intensity treatment continues to induce an extra 20% of the landless population to re-migrate. The corresponding figure for the low-intensity treatment is 5.6%.

5.1.3 Summary of Effects on Migration

To summarize, people react more positively to the exact same migration subsidy offer when a larger number of their village co-residents receive the offer at the same time. This enhanced propensity to migrate even extends to other village residents who did not receive an offer themselves. Our data suggest that people coordinate their migration plans: these spillovers are stronger amongst people who know, or can rely on, an offer recipient. The high intensity offers increased the number of migrants sharing housing by a little and the number of travel companions by a lot. All of these factors combined to produce a much stronger labor supply shock to the village labor market under the high-intensity treatment, which we exploit to study the downstream effects on income, wages and employment conditions.[19]

5.2 Income, Labor Supply and Wages

Our endline survey collected data on all sources of income (including migration income) over a 5-month recall period covering the entire main Aman rice growing season (including the pre-harvest lean period). Each round of the high frequency labor surveys focused only on labor income over a 7-10 day recall period, and conducted a deeper dive into hours of labor supplied and daily earnings inside and outside the village, broken down by all working household members. The labor surveys were repeated 6 times, covering 8 weeks total. We now report the effects of our treatment on income, starting with all sources of household income gathered from the endline survey.

Table 5 reports ITT estimates. Those offered grants in either high or low intensity villages experience large and significant gains in migration income. The migration rates in these two cells were different (Table 1), so the larger income estimate for the high-intensity villages (4,815 compared to 3,516) does not necessarily imply that migrants from those villages were more successful. Columns (2) and (3) show that inducing migration amongst these households reduced their non-migration income for the 5-month recall period. Columns (4) and (5) show that even after

[19] Our research design assumes that villages are closed labor markets. A concern about this research design would be the possibility that our treatments induce travel from control and low-intensity into high-intensity villages. Villages in our sample are not very close to each other, and we use GPS coordinates of all treatment villages to verify that out-migration rates are no different from villages that are physically close to other treatment villages. A regression of migration rate on distance to the nearest treatment village (in km) has a coefficient of +0.004 and standard error of 0.03.

accounting for that displacement, net income increased significantly (by about 9-19% of the control group mean) in the ITT when households were offered a migration transfer. Total labor income increases for those who were not offered grants (column 5), and about three-quarters of that effects stems from these groups' increased migration propensity when their friends receive offers to travel.

Column 6 shows that this extra income led to virtually no change in savings. This is consistent with results from earlier rounds of investigation (Bryan et al. 2014) that document that this is a sample of extremely poor people who consume any extra income earned during the lean season. Finally, column 7 shows that the re-migration a year after the interventions led to persistent gains in income. The larger gains observed a year later is also consistent with results in Bryan et al. (2014), and could be partly due to selection on successful migrants who choose to re-migrate.

5.2.1 Differences between High and Low Intensity Villages

ITT estimates report average effects combining migrants and non-migrants, and are therefore not useful for determining whether the migration experience of those traveling from high-intensity village is more successful than that of migrants from low-intensity villages. The high-intensity treatment may allow more friends to travel together and create other benefits, but on the other hand, it may draw in more marginal migrants who do not possess as strong a comparative advantage in the city. To explore these tradeoffs, we run an IV specification, where the decision to migrate from each type of village is instrumented with assignment to treatment:

$$Y_{ivj} = \alpha + \gamma_1 Migrant\_LI_{ivj} + \gamma_2 Migrant\_HI_{ivj} + \varphi_j + \varepsilon_{ivj} \qquad (5.2.1)$$

$Migrant\_LI_{ivj}$ ($Migrant\_HI_{ivj}$) is an indicator for any member of the household migrating at any point during the lean season from a low (high) intensity village, and Y_{ivj} is migration income. We instrument the migration decisions with the randomized treatment variables, {Offer_HI, NonOffer_HI, Offer_LI, NonOffer_LI} from equation (5.1.1), or simply the village level treatments {Low_I, High_I} from equation (5.1.2). The bottom rows of Table 6 show that the first-stage fits are quite strong, regardless of which set of instruments is used (F-stat on excluded instruments of 305 or 165).

Column 1 of Table 6 shows that migrants traveling from high-intensity villages earn an extra 6,173 Taka (US$80 at BDT 77 = $1) at the destination, while migrants from the low-intensity villages earn an extra 4,672 Taka ($61), both large relative to the 1,000 Taka ($13) conditional grant to subsidize migrants' travel costs. The difference between the two numbers is not statistically significant (p=0.18). The point estimate indicates that additional benefits associated with traveling with more companions is larger than the negative selection effects, but the difference is not significant on net.

Table 7 uses the high frequency labor surveys to delve into how the experiences of migrants from the control, low-intensity and high-intensity villages differ. The primary workers in households from high-intensity villages who migrate have an easier time finding work: they are employed 5.9 (out of 7) days during the weeks when they migrate, compared to 5.7 days for workers from low-intensity villages (p-value of difference = 0.46) and 5.3 days (p-value = 0.08) for control village migrants (column 1). High-intensity village migrants are entirely unemployed in the city 1.6% of the time, compared to 4.6% for low-intensity (p-value=0.18) and 6.5% (p-value = 0.06) for control village migrants (column 12). Consequently, migrants from the high-intensity villages earn more.

Table 7 also makes clear how labor market conditions between the villages of origin and migration destinations differ. In contrast to the 1.6-6.5% unemployment rate in the city, primary workers in our sample remain unemployed about 50% of the time during the weeks when they are back in their village of origin.[20] The situation is better in high-intensity villages (39% unemployment, compared to 53% in low-intensity and 58% in control villages in column 14), which likely has to do with many competing workers being away that week, which is a topic that we'll return to when discussing general equilibrium effects. On average across the entire sample, people are able to work 3.9-4.3 days per week in the village, compared to the 5.3-5.9 days per week in the city. The income differences we observe are therefore at least partly due to differences in work availability.

5.2.2 Further Insights on Income Sources from High Frequency Labor Surveys

The high frequency surveys asked about the details of labor market participation for all household members over a weekly recall, which allows us to provide richer detail on income earned by the household at migration destinations, income earned in the village, as well as hours worked and daily income separately for each location. While the endline survey allows us to capture income aggregated over 5 months, with the high-frequency we can delve into exactly what was happening in the village labor markets week-to-week during the exact period that many migrants were moving back and forth between the village and the city. Table 8 provides ITT estimates. For completeness, Panel A shows full sample results and Panel B omits the 16 villages where we do not have an exact measure of village population (which will affect the precision of IV estimates in subsequent tables).

[20] This compares migrants to those who choose to stay in the village, and is therefore partly driven by selection.

Offering a migration subsidy increases income by 1,263-1,401 Taka during that eight week period[21], and income earned at destinations (1,000-1,100 Taka) accounts for roughly 80% of the overall gain. The treatment increased both the number of work days (+4.8, of which +4.4 is work away from the village), and income earned per day, and these two channels combine to produce the overall income gain. The ITT estimates for the low-intensity villages are smaller, probably because the gains we observe clearly come from households that access labor markets outside the village, and the migration take-up rate is a lot lower in the low-intensity villages.

There are two other noteworthy results in Table 8 that require further investigation. First, offering a subsidy to migrate increases income earned in the home village (by ~200 Taka, but statistically insignificant change) during this focused period covered by the high-frequent labor sureveys. It also increases households' work days at home by about a day compared to the control group. This implies that increases in income earned outside the village during the 8 week heavy migration period did not simply displace income that would have otherwise been earned in the village. Further, the direction of these effects is somewhat surprising, even though it is not statistically significant, because we induced the main income earner to leave home and go work outside the village. That the household continues to earn more at home even when the primary worker is not present (while also earning more at migration destinations) may indicate some changes in the village labor market, or intra-household changes in labor supply, and we need to understand this better.

Second, households residing in the high intensity villages but who were never treated or directly contacted in any way by our implementation partners, experience an increase in their income (419 Taka in the full sample and 619 Taka (p-val<0.1) in the 117 village sample). This is a summary measure of the general equilibrium effect that is inclusive of all forms of spillovers, which may include: (a) increasing the migration propensity of the non-offered, (b) changing their outcomes at

[21] We have provided estimates of the effect of our treatment on income from two separate sources: the endline and the high frequency survey. The effect sizes are quite comparable. The ITT estimate for migration income gain from offers in high intensity villages is 4,815 taka (over 22 week recall period or about 218 taka per week), and 3,231 Taka (147 per week) for all labor market income. The corresponding estimates from the high frequency survey is 1,263 taka for labor market income (over 6 weekly recall periods or about 210 taka per week), and 1,049 Taka (175 per week) earned away from the village. There is also quantitative consistency across years and studies: Our LATE estimates of extra income accruing to migrant families are large enough to produce the consumption and calorie gains reported in Bryan et al. (2014).

destination, or (c) changing labor market conditions at the origin. We will try to shed light on each of these potential channels.[22]

5.2.3 Why did Income Earned in the Village Increase?

We first establish the magnitude of the increase in income by running a LATE version of the ITT estimates presented in the previous table. In this smaller high frequency survey sample, we have first-stage precision only when instrumenting the household's decision to send a migrant with assignment to the high-intensity treatment. Table 9 shows that induced migrants earn an extra 9,135 Taka ($119), and again, about 80% is derived from income earned outside the village, while the other 20% is attributable to an increase in home income. Migration allowed the household to work an extra 34 person-days, most of it outside the village. This reflects the stark difference in unemployment rates between the village and the city during the lean season: Recall that Table 7 shows that it's much easier to find work in the city. The last column provides the first clue as to why income earned *inside the village* increases for migrant households. Daily income at home, which is calculated as income per day of work, increases for those induced to migrate. This suggests that wages and employment conditions changed in high- intensity villages.

Table 10 reports the equilibrium effects of our village-level treatments on wages paid, using data from our survey of employers. In this regression, we use village-level assignment to high-intensity treatment to instrument for the fraction of households induced to send a seasonal migrant (see Table 4), and then study the effects on wages reported by employers in a second stage:

$$W_{evj} = \alpha + \mu \cdot Migration\_Rate_{vj} + \varphi_j + \varepsilon_{evj} \qquad (5.2.3)$$

This is an employer-level regression, where W can be either agricultural or non-agricultural wages reported by employers. We have a relatively small number of employers reporting wages in each category (385 or 276), which is why we focus only high-intensity villages where we have a much stronger first stage. The smaller increase in emigration rate in the low-intensity villages creates a weak instrument in the employer sample. We collected data on wages by gender, but focus on the wage rate for males because very few employers reported hiring females, and almost all migrants were male. The village-level "Migration_Rate" is defined as (Households with Migrants/Landless

[22] To do so, we will need to make use of the details in the high-frequency data on who within the household leaves and who stays at home during the critical migration period, and where and what do they earn. The fact that our treatment changed the migration propensity of the untreated (as documented in Table 1) implies that "non-offered" households cannot act as a clean sample that helps us experimentally isolate labor market conditions in the village.

households). We show results for both the full sample, and the subsample omitting the 17 villages without precise data on the landless population in the village.

More out-migration causes the agricultural wage rate in the village to increase, but has no detectable effect on the non-agricultural wage rate. For every extra 10% of the landless population that emigrates, wages increase by 2.2% ($p<0.05$; column 3) in the 117 village sample, or 1.5% in the full sample ($p<0.1$). Our high-intensity treatment induced an extra 30% of the population to emigrate on average. Agricultural wages are therefore predicted to increase by about 4.5-6.6%.

We must acknowledge the fact that these wage effects, estimated using variation in the emigration rate across 133 villages, are only marginally statistically significant. We re-compute the statistical precision of these estimates using randomization inference, which does not rely on asymptotic normality assumptions. As Imbens and Rosenbaum (2005) show, when data is uninformative and 2SLS gives overly precise estimates, randomization inference will yield correctly wide confidence intervals. Figures 3 and 4 show that our randomization inference yields similar but slightly less precise estimates of the effect of migration on log agricultural wages. Using the 117 village sample, the p-value testing the null of no effect of emigration on log wages is 0.071. Using the full sample, the p-value is 0.104.

Overall, there are consistent indications in different datasets and variables that village wages increase when the emigration rate is higher – (a) when employers are asked about wages paid, (b) when we compute "daily income" earned at home (Table 13, column 3 and Table 9, column 8) using the high-frequency labor surveys, and (c) when we ask employers about their costs and revenues (presented below in Section 5.3). However, the statistical precision for each of these tests is weak, with p-values in the range of (0.05, 0.1). Also in terms of magnitude, only a sixth of the observed increase in "income earned in origin village" amongst migrant households (1,787 Taka in Table 9, which is a 40% increase relative to the control group) can be attributed to the increase in the wage rate. There must have also been an increase in hours worked at home, which we investigate next.

5.2.4 Household Labor Supply at Origin

To explain the increase in labor supply and income at home in migrant households, the first natural candidate to consider is an increase in labor supply amongst other household members when the migrant is away. With many primary earners migrating away from the high-intensity villages, the spouses and adult children of the primary working age males may have an easier time finding work, and choose to expand their labor supply in response to the increased agricultural wages.

Our survey instrument collected labor supply and income separately for every working member of all households, and it also clearly identified the one primary earner for each household (who is often the migrant). Table 11 examines intra-household shifts in labor supply by studying income, work days and daily earnings for all household members *other than* the primary worker. Surprisingly, there is no increase in work days or income in this sub-population across treatment arms. The point estimates are quite small (e.g. days worked changes by -0.17 days), and not statistically distinguishable from zero. Evidently, migration does not induce any intra-household substitution in labor, and other members are not responsible for the increase in income inside the village that we have documented.

So the remaining possibility is that the primary worker boosts participation in the labor markets within high-intensity villages. Since this person is also induced to migrate by our treatment, the story would have to be that the primary earner goes back-and-forth between origin and destination, and, during periods when a larger number of competing workers are away during any given week in high-intensity villages, he supplies labor to the origin labor market during the week when he is back, taking advantage of slack (and the higher agricultural wages) there.

Before conducting a formal test of this mechanism, Table 12 presents a series of descriptive statistics which show that a number of the conditions that are necessary for these gains to occur are actually present in the data. First, panel A shows that the primary worker is not only contributing the bulk of income earned away from the village (as a migrant), but he is also responsible for the majority of income earned inside the village. Across all treatment arms, he contributes about 80% of household income and days worked at home. Panel B sheds light on how this is possible: the high-frequency re-visits show us that the primary worker migrates over multiple episodes (averaging 1.55 episodes per migrant) and therefore moves back-and-forth between the village and the city. On average, he spends a third of his time away from the village.

Finally, primary workers in high-intensity villages can only take advantage of periods of slack in the village labor market if not all migrants leave the village and return to the village simultaneously. To explore whether this is the case, panel C uses the high -frequency data to calculate the probability that a randomly -sampled “ever-migrant” (i.e. someone who has migrated during the period of enquiry) is at home during a week that another randomly -sampled ever-migrant *from his village* is away migrating. We see that this probability is quite high—about 75%—across all treatment arms. This implies that not all migrants from a village travel and return together at the same time: some might travel for weeks 1-3, others weeks 4-6, and yet others traveling back-and-forth (e.g. week 2 plus 5-6). Whenever a migrant returns home to a high-intensity village, many of his competitors are away (owing to the high intensity of the treatment), and this could make it easier for him to find work and take advantage of the improved agricultural wages.

Some statistics presented earlier in Table 7 already indicate that this is a plausible explanation. We had used the individual-level and weekly variation afforded by our high frequency surveys to compare the labor market performance of primary workers during the weeks they spend back at the village of origin. The sub-sample of ever-migrants find work 3.5 days of the week when they return to control villages, 3.6 in low-intensity, and 4.3 days in high-intensity treatment villages (Table 7, column 3). The difference between high-intensity and control villages is statistically significant (p-value<0.01, with errors clustered by village). In the full sample of households (col. 2), there are 3.9 days of work in control villages, 4.1 in low-intensity and 4.3 in high-intensity (p-val=0.02). Primary workers in high-intensity villages are also less likely to be entirely unemployed during weeks when they are home (Table 7, col. 14: 38% versus 53% in low-intensity and 58% in control; p-value=0.09). They also report greater weekly earnings at home (cols. 8 or 9: 825 Taka versus 680 Taka in low-intensity and 615 in control; p-value<0.001).

The data therefore provide some clear indication of greater labor market slack in the villages of origin, as the saturation of the migration subsidy treatment (and correspondingly, the emigration rate from those villages) was increased. We test this hypothesis in Table 13 using the individual-level and weekly variation recorded in the six rounds of the high frequency labor market surveys. During the weeks when they are at home, primary workers earn 60 Taka more in low-intensity villages than in control, and primary workers in high-intensity villages earn 88 Taka more. This test uses the sample of all primary workers, not only those who chose to migrate. These benefits are accruing to all residents of high-intensity villages.

However, column 10 in Table 7 shows that this result is a little complicated to interpret. Naturally, primary workers in high-intensity villages spend more weeks away from the village (1.4) than workers in control villages (0.8), because our treatment induced migration. Cross-village comparisons of "income earned in the village" are therefore complicated, because migrants may choose to come back to the high-intensity village during the select weeks when it is most profitable to do so. That may lead to some "selection on timing" that drives the home-income difference in Table 13.

To address this concern, we identify the "best week" to be at home in each of the villages, defined as the week when the household earned the highest income in their home village. We then conduct a "best week to best week" comparison across control, low-intensity and high-intensity villages in Table 14. This now uses only one-sixth of our data (because we only pick one week – the best week - of data per household out of six), and the precision of our estimates therefore suffers. Even in this sub-sample, residents of high-intensity villages earn significantly more (74 Taka, $p<0.10$) during a week when they are at home compared to residents of control villages.

5.2.5 Summary of Income, Wage and Labor Supply Effects in the Village of Origin

Migration subsidies significantly increased income in both the low-intensity and high-intensity treatment villages. About 80% of the gain was due to income earned away from the village, where it is much easier to find work during the lean season. When many workers migrated from the high-intensity villages, both the rural agricultural wage rate and the amount of work available per worker increased in those villages. This improved home-earning for those who chose to stay behind, as well as for migrant families during the weeks when the migrant returned home to take advantage of the improved labor market conditions. The high intensity treatment produced substantial spillover benefits for those supplying labor to the village labor market, but we can only state this confidently when using data from the full sample (including treated households and migrants during weeks when they return home). Any inference from the partial sample of non-offered households is statistically weaker, both because this is a smaller sample and because many of them also chose to migrate.

5.3 Effects on Employers

The higher agricultural wages we documented have to be paid by someone, so we collected data from both agricultural and non-agricultural employers of those laborers. This sample represents the landed households who were ineligible for our migration interventions targeted to the landless. In 2016, we asked employers questions about their agricultural yields, revenues, costs, and profits for both the season during which the migration subsidy program was in effect as well as the season thereafter. While we cannot rule out the possibility of recall bias (see section 3.2.4 above for a description of the data collection that should mitigate this concern), we have no reason to believe that this bias will differ systematically between control and treatment employers. We use a very similar IV specification to the one we used to study the equilibrium effects on the wage rate (Table 10, equation 5.2.3) to analyze the effects of the village emigration rate (instrumented by the treatment intensities) on employer revenues, costs and profits.

Consistent with the increase in the agricultural wage rate, agricultural employers report an increase in the wage bill and also in non-wage costs in the first two columns of Table 15, which combine to increase total costs in column 3. The variance in costs across employers is high, and these effects are not statistically significant. In column 4 we control for employer reported costs in 2013 (reported retrospectively), and increase in costs becomes statistically significant ($p<0.05$). The next two columns show that revenues decrease, but not significantly. Unlike the cost variable, the effect on revenues gets smaller and stays insignificant when the dependent variable is defined as change in revenues pre- to post- intervention. There were no changes in farming practices or technology apparent in the survey.

The combination of the increase in costs and the slight decrease in revenues leads to a significant decrease in employer profits, as reported in column 7. Column 8 undermines some confidence in this profit result, because the result does not stay significant when we difference out pre-intervention profits from the dependent variable. This suggests some "imbalance" in employers' retrospective recollection of profits two years prior (in 2013), in that employers in high-intensity villages report lower profits in 2013, a season before our intervention.

We also collected data on yields for the main (Aman) rice growing season that coincides with the timing of our intervention. Results are reported in Appendix Table A10. There are no statistically significant or systematic effects on agricultural yields. The coefficients are positive for yields, but mixed for the logged specification, or when normalized by land size. In summary, there appears to be a (negative) pecuniary externality on employers. The large number of laborers leaving the high-intensity villages during the lean season creates upward pressure on agricultural wages, which lowers employer profits. Employers don't appear to adjust production technology during the two seasons post disbursement of migration subsidies.

5.4 Effects on Food Prices

We collected data on the prices of a basket of important staple foods from shopkeepers across all villages of origin in our sample. If food markets are not well integrated across villages, the increase in income from migration transfers could create a demand shock and increase the price of food, undermining the benefits of the program in real terms.[23] We collected data on all the main staples and main items sold in village grocery stores that typically double as tea and snack shops.

[23] Cunha et al. (2017) document such an effect for an in-kind food transfer program in Mexico.

Table 16 shows results on the effects of emigration on prices item-by-item, and then a Laspeyres price index. The index was constructed using expenditure shares in a baseline consumption module from July 2008, before any of these villages were ever treated with migration interventions.

We don't detect any systematic increase in the prices of the most common staples. There is absolutely no effect of our intervention on the price of rice (or of flour, *daal* (lentils), sugar, salt or milk). Rice accounts for about 70% of the food budget in our sample. However, we see a statistically significant increase in the price of fish protein. For every 10% increase in emigration, fish price increases by 1.5% ($p<0.1$), and meat price by 0.4%. Bryan et al. (2014) had noted that migrant households increase their consumption of protein, and they also shift towards animal protein. Fish are also more difficult to transport over longer distances, given the low prevalence of refrigeration. It is therefore not surprising that the market for fish appears less spatially integrated than the market for staples like rice and flour.

In contrast, the price of prepared foods (like tea, samosas, and prepared meals), which is an important non-tradable good, falls. For every 10% increase in emigration, the price of cups of tea and other beverages sold at the tea shops decreases by 1.5% ($p<0.1$), and prepared foods by 0.5%. The male household heads (who are the ones induced to migrate) are typically the individuals who congregrate at tea shops and consume such prepared food and beverages. When migrants leave the village, the prices of non-tradable goods they consume fall.

As predicted by simple trade theory, we only observe significant changes in the prices of goods that are less tradable, such as fish (given limited refrigeration) and prepared foods. The net effect of a 10 percentage point increase in emigration is a 0.9% increase in the price of food, as measured by a Laspeyres index aggregating across all 12 food items. The protein price increase dominates the non-tradable price decrease because fish constitutes a bigger share of the household budget than prepared foods and tea. This implies that the 30 percentage point extra emigration induced in our high-intensity villages increases the cost of food by 2.7%. The next section combines the observed income and price effects to estimate changes in the rural economy in real terms.

6. Aggregating Effects of Emigration on the Rural Economy

We combine these price changes with income effects we have estimated for eligible households who received migration subsidy offers and those who did not, to estimate the real effects of the high-intensity migration treatment on the village population. 60% of the village population were landless and "eligible" for the migration treatment, and 70% of those households received a migration subsidy offer. This creates three groups: 42% (0.6 x 0.7) of the populated were treated with offers, 18% were untreated eligibles, and the remaining 40% were ineligible for treatment. The intent-to-treat estimates from Table 5 (column 4) suggest that total income over the entire season increased by about 10% (relative to the control villages) in nominal terms for those who were treated in high-intensity villages, and there was a precisely estimated zero effect on the incomes of untreated eligibles. Since prices increased by 2.7% in those villages assigned to the high-intensity treatment, the 42% of the population gained 7.3% in real terms, while everyone else lost 2.7% in real terms. Taken together, this implies a 1.5% increase in real income for the entire village population. Further, some of the un-treated ineligibles were employers who had to pay the higher wage bills that led to those income increases in the treated population. We estimated that a sixth of the income increase was due to the change in the wage rate. Once we adjust for this within-village transfer, the increase in real income for the entire village population is a more modest 0.8%.

Given inequality considerations, we may care about the intervention's effects on the landless (eligible) population. In this subset, which constitutes 60% of the village population, incomes increased by 4.3% in real terms. Focusing on labor income (Table 5, column 5) - which is more precisely measured in this economy - provides an even more optimistic view: There was 10.6% increase in real labor income in the population, driven by a 17.6% increase among the landless.

7. Conclusion

Researchers have identified barriers to internal mobility as a key obstacle to development in many different contexts (Pritchett 2006, Clemens 2011, Brooks and Donovan 2017, Dinkelman et al. 2017). To that, we add rigorous evidence that encouraging seasonal out-migration not only benefits those who receive migration subsidies, but also indirectly benefits others in the village, mostly by making it easier for them to migrate as well. Large-scale temporary emigration creates some slack in the lean-period village labor market and increases available work hours for landless laborers who are in the village during any given week. It also reduces inequality by raising rural agricultural wages, which benefits laborers at the expense of richer landowners who hire them. The concerns expressed by various scholars about the rural economy collapsing, or inequality increasing when migration opportunities are expanded appear to be unfounded, at least in the context of seasonal migration in rural Bangladesh.

Our results call into question the wisdom of instituting mobility restrictions on behalf of rural development goals. Many municipal governments in developing countries have reacted to increased rural-urban migration as if it were an invasion to repel (Simmons, 1981: p. 89), with direct controls such as permits required for transport, settlement or accepting urban employment (Oberai, 1983).[24] China's Hukou system, which restricts freedom of movement, is the most important contemporary example of mobility restrictions for both temporary and permanent moves.

Some policymakers pursue development strategies that emphasize rural development (including a suite of rural support programs in Bangladesh such as *Vulnerable Group Development*, *Food for Asset Creation*, and *Rural Maintenance Program*) and employment creation (such as India's NREGA), while others advocate for improved connectivity between regions so that citizens can take advantage of spatial gaps in wages (Morten and Oliveira 2017; Bryan and Morten 2017). Our results are informative about the relative effectiveness of these alternative strategies. These two broad approaches often act as substitutes: Rural employment guarantee schemes prevents out-migration (Imbert and Papp 2017) by improving rural wages (Imbert and Papp 2015).

The seasonal migration support program we devised straddles these two policy poles by enhancing connectivity without large investments in infrastructure. With the low disbursement cost and its persistent effects, the program we test is at least 5 times as cost effective in improving food security as four major food-for-work and cash transfer programs in Bangladesh evaluated by IFPRI (Ahmed et al 2009). This paper focused on understanding changes to the village labor market as people emigrate, but seasonal migration may induce some other changes in general equilibrium. To provide a more comprehensive evaluation as this program is scaled up further, future research will explore non-economic effects in the social, health and political realms, market spillovers on the destination urban economies, and changes in village risk sharing.

[24] In Jakarta in 1970, migrants were required to register and deposit their return fare. This was true also of forced slum clearance in Delhi during the emergency declared by Indira Gandhi. South Africa has restricted movement of its black population. China has required removal certificates from place of origin and documentation of job offers, and has enforced forced rustication.

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Table 1. Migration in 2014-15 and Remigration in 2015-16 in Response to Treatments in 2014

VARIABLES	(1) At least one migrant (2014-15)	(2) Number of migrants (2014-15)	(3) Migration episodes (2014-15)	(4) Re-migration in 2016, at least one migrant
Offered Grant in Low Intensity Treatment Village	0.248*** (0.0366)	0.260*** (0.0405)	0.390*** (0.0666)	0.188*** (0.0341)
Not Offered Grant in Low Intensity Treatment Village	0.0333 (0.0388)	0.0314 (0.0442)	0.0759 (0.0720)	0.0282 (0.0347)
Offered Grant in High Intensity Treatment Village	0.398*** (0.0333)	0.412*** (0.0376)	0.626*** (0.0630)	0.293*** (0.0352)
Not Offered Grant in High Intensity Treatment Village	0.0965** (0.0397)	0.111** (0.0463)	0.127* (0.0723)	0.127*** (0.0371)
Observations	3,600	3,600	3,600	3,382
R-squared	0.137	0.119	0.124	0.089
Control Mean	.342	.367	.499	.378
Upazila FE	YES	YES	YES	YES
p-value: Offered High = Offered Low	0	0	0	.003
p-value: Non-Offered High = Non-Offered Low	.127	.101	.53	.009

Errors clustered at the village level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The results in this table were generated using household level data from the endline survey.

The dependent variable in specification (1) is an indicator for whether the household had at least one migrant over the period September 15 2014 - April 30 2015. The dependent variable in specification (2) is the total number of unique migrants sent by the household over this period. The dependent variable in specification (3) is the total number of migration episodes (i.e. the total number of trips taken by all migrant members of a household) over this period. The dependent variable in specificaiton (4) is re-migration a year later (September 1 2015 - May 31 2016). No further incentives were provided that year, but we collected data to study longer term responses.

All specifications include Upazila fixed effects (an Upazila is an administrative unit that encompasses groups of villages in the sample; there are a total of 14 Upazilas across our sample of villages). Dependent variables were winsorized at the 98% level (values below the 1st percentile were set to the 1st percentile value and values above the 99th percentile were set to the 99th percentile value)

Table 2. Effect of Household's Network on Probability of Migrating in 2014-15

VARIABLES	(1) At least one migrant (2014-15)	(2) At least one migrant (2014-15)
Offered, Low Intensity Village, Knows Someone Offered	0.209***	
	(0.0444)	
Offered, Low Intensity Village, Can Rely on Someone Offered		0.191***
		(0.0526)
Offered, Low Intensity Village, Knows but Can't Rely on Someone Offered		0.226***
		(0.0522)
Offered, Low Intensity Village, Does not Know Someone Offered	0.208***	0.216***
	(0.0637)	(0.0636)
Not Offered, Low Intensity Village, Knows Someone Offered	0.0991*	
	(0.0556)	
Not Offered, Low Intensity Village, Can Rely on Someone Offered		0.122*
		(0.0678)
Not Offered, Low Intensity Village, Knows but Can't Rely on Someone Offered		0.0666
		(0.0772)
Not Offered, Low Intensity Village, Does not Know Someone Offered	-0.0443	-0.0473
	(0.137)	(0.137)
Offered, High Intensity Village, Knows Someone Offered	0.311***	
	(0.0589)	
Offered, High Intensity Village, Can Rely on Someone Offered		0.344***
		(0.0717)
Offered, High Intensity Village, Knows but Can't Rely on Someone Offered		0.249***
		(0.0692)
Offered, High Intensity Village, Does not Know Someone Offered	0.112	
	(0.374)	
Not Offered, High Intensity Village, Knows Someone Offered	0.126**	
	(0.0632)	
Not Offered, High Intensity Village, Can Rely on Someone Offered		0.156*
		(0.0833)
Not Offered, High Intensity Village, Knows but Can't Rely on Someone Offered		0.0686
		(0.0865)
Not Offered, High Intensity Village, Does not Know Someone Offered	-0.213	-0.173
	(0.230)	(0.245)
% of people from list household knows	0.0156	0.00459
	(0.0590)	(0.0594)
Observations	998	994
Control Mean	.331	.331

Errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The results in this table were generated using a combination of network data from 2013 and migration and treatment data from the 2014 endline survey. Estimations are at the household level. All specifications include Upazila fixed effects.

The network data was generated by asking subject households to answer questions about each of 20 randomly selected households from the same village, including: whether the respondent household knows them at all; whether it knows them well; and whether it can rely upon them. The dependent variable in all specifications is the probability that the respondent household had any member who migrated in 2014. The results shown are average marginal effects on a probit regression. Thus the coefficients represent the change in the probability that a household will have a migrant based on the treatment arm and connection to other households. Specification (1) measures household connectivity as a binary distinction: whether the respondent knows another household with/without offer. Specification (2) subdivides connectivity into three categories: (a) whether the respondent "can rely on" on another household with/without offer, (b) whether it "knows" another household but cannot rely on it, or (c) that it does not know another household with/without offer at all. The independent variables intersect the network data with the treatment data, thus placing households in groups according to two criteria: whether they themselves were offered a subsidy to migrate, and whether they know households that were offered a subsidy. Thus, for instance, "Offered Grant in Low Intensity Treatment Village and Connected to Someone Offered" refers to a household in a low intensity village that was made a migration grant offer and knows another household with someone who was also offered a grant.

Table 3. Accomodation Sharing and Traveling with Companions Among Migrants (2015-16)

	(1)	(2)
VARIABLES	Number of companions with whom sharing accomodation	Number of travel companions
Offered Grant in Low Intensity Treatment Village	-0.123	0.586
	(0.778)	(0.583)
Not Offered Grant in Low Intensity Treatment Village	-0.164	1.007
	(1.017)	(0.642)
Offered Grant in High Intensity Treatment Village	1.293	2.819***
	(0.892)	(0.708)
Not Offered Grant in High Intensity Treatment Village	-0.286	2.434***
	(0.781)	(0.641)
Observations	1,678	1,756
R-squared	0.052	0.091
Control Mean	10.123	6.17
Upazila FE	YES	YES
p-value: Offered High = Offered Low	.116	.002
p-value: Non-Offered High = Non-Offered	.906	.041

Errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The results in this table were generated using household level data from the longer term follow-up survey conducted in 2016.

Both specifications use the subset of the sample that migrated in the year subsequent to the intervention-year i.e. the 1,793 households that sent at least one migrant during the period September 1 2015 - May 31 2016. The dependent variable in specification (1) is the number of companions with whom a migrant shared their accomodation during this period. The dependent variable in specification (2) is the number of companions with whom a migrant traveled during this period.

All specifications include Upazila fixed effects. Dependent variables were winsorized at the 98% level (values below the 1st percentile were set to the 1st percentile value and values above the 99th percentile were set to the 99th percentile value)

Table 4. Migration Response at the Village Level

VARIABLES	(1) Proportion of Landless Households Migrated	(2) Landless Migration Rate as a Fraction of Total Households in the Village	(3) Proportion of Landless Households Migrated	(4) Landless Migration Rate as a Fraction of Total Households in the Village	(5) Proportion of Landless Households that Re-Migrated in 2015-16
Low Intensity Treatment Village	0.0729*	0.0332	0.0796*	0.0465*	0.0564*
	(0.0381)	(0.0281)	(0.0403)	(0.0273)	(0.0303)
High Intensity Treatment Village	0.304***	0.115***	0.278***	0.154***	0.198***
	(0.0386)	(0.0285)	(0.0425)	(0.0289)	(0.0320)
Observations	133	127	117	111	117
R-squared	0.548	0.460	0.542	0.543	0.553
Control Mean	.347	.207	.347	.207	.358
Sample	133 villages	133 villages	117 villages	117 villages	117 villages
p-value: High Intensity = Low Intensity	0	.002	0	0	0

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The results were generated using a combination of the 2015 endline survey and the 2015 employer survey.

The dependent variable in specification (1) is the proportion of landless households eligible for a subsidy in each village that migrated at any point over the period September 15, 2014 - April 30, 2015. The number of eligible households in a village (the denominator) computed based on census data collected in 2008. The formula we used to compute the fractions accounts for the fact that differing fractions of offered and non-offered households were sampled, and we know the sampling probabilities. Specification (2) changes the denominator to "number of total households in the village" also reported in the census data. 6 villages drop when we use this dependent variable (133->127 and 117->111). Specifications (3)-(4) are identical to (1)-(2), but with the sample limited to villages where we have the highest quality listing data on numbers of total and eligible landless households in the village (which are the denominators of the dependent variables). Specification (5) measures re-migration, i.e. migration from 2015-2016, and also limits the sample to villages where we have the highest quality listing data on numbers of total and eligible landless households in the village.

All specifications are at the village level. All specifications include Upazila fixed effects.

Table 5. Intent-to-Treat Effects on Migration Income, Savings, Labor Income, Profits at Home and Income from Re-Migration, using Endline Survey

VARIABLES	(1) Migration income	(2) All non-migration income (incl. all labor income and enterprise profits)	(3) Labor income at home	(4) All Income (Migration + Home Labor Inc. + Enterprise profit/loss)	(5) All labor income (migration + home)	(6) Savings	(7) Income from Re-migration in 2015-16
Offered Grant in Low Intensity Treatment Village	3,516***	-920.2	-747.9	2,589***	2,715***	16.71	5,392***
	(712.7)	(999.6)	(783.3)	(958.6)	(899.8)	(201.1)	(1,359)
Not Offered Grant in Low Intensity Treatment Village	902.7	-705.2	523.9	127.7	1,444*	-91.24	241.6
	(730.3)	(924.5)	(751.2)	(917.1)	(864.7)	(221.6)	(1,196)
Offered Grant in High Intensity Treatment Village	4,815***	-2,628***	-1,599**	2,105**	3,231***	-15.26	7,500***
	(680.1)	(891.6)	(701.6)	(869.2)	(843.0)	(205.4)	(1,380)
Not Offered Grant in High Intensity Treatment Village	1,559**	-1,512*	517.2	20.45	2,093**	116.6	3,867***
	(732.7)	(910.9)	(697.9)	(898.4)	(905.9)	(271.4)	(1,370)
Observations	3,600	3,600	3,600	3,600	3,600	3,600	3,382
R-squared	0.085	0.031	0.032	0.044	0.055	0.011	0.097
Upazila FE	YES	YES	YES	YES	YES	YES	YES
Control Mean	5016	18759	11810	23903	16975	1987	9204.65
p-value: Offered High = Offered Low	.097	.077	.201	.604	.535	.862	.137
p-value: Non-Offered High = Non-Offered Low	.443	.348	.991	.902	.46	.445	.004

Errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The results in this table use household level data from the 2014-15 endline survey.

The dependent variable in specification (1) is gross income from migration that migrants generated during the period September 15 2014 - April 30 2015; (2) includes all income and profits earned at home (all income from household's enterprises, and both ag and non-ag wages minus the household's costs in the income-generating activities); (3) is the labor market wages earned at home by working on others' farms or businesses, and is a subset of (2); (4) is the sum of (1) and (2); (5) is the sum of (1) and (3); (6) is savings reported by the household, accruing over the same period; and (7) is migration income from re-migration in 2015-2016.

There are a few massive outliers in reported income, and all columns therefore trim out the extreme 1% of values for the dependent variable (top and bottom).

All specifications include Upazila fixed effects. Dependent variables were winsorized at the 98% level (values below the 1st percentile were set to the 1st percentile value and values

Table 6. LATE (IV) Estimates to Study the Differential Effects of Migration from Low-Intensity and High-Intensity Villages

	(1)	(2)
VARIABLES	Migration income	Migration income
Migrated, Low Intensity Treatment Village	4,672***	6,294***
	(1,138)	(1,030)
Migrated, High Intensity Treatment Village	6,173***	7,520***
	(946.7)	(1,032)
Observations	3,600	3,600
R-squared	0.150	0.171
Upazila FE	YES	YES
Instruments	High/Low Intensity	High/Low Intensity, Offered/Nonoffered
chi2-test High Intensity=Low Intensity	1.760	1.240
Prob > chi2	0.185	0.266
First Stage Partial R^2	0.393	0.422
First Stage F-test Statistic	305.4	165.4
First Stage p-value	0	0

Errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The results in this table show IV specifications using household level data from the endline survey.

The dependent variable in both specifications (1)-(2) is gross income from migration that migrants generated during the period September 15 2014 - April 30 2015.

The dependent variable is regressed on two indicators for migrants from low and high intensity villages, respectively. These variables are instrumented in a 2SLS regression using assignment to treatment (as indicated).

All specifications include Upazila fixed effects. Dependent variables were winsorized at the 98% level (values below the 1st percentile were set to the 1st percentile value and values above the 99th percentile were set to the 99th percentile value) to deal with a few outliers in reported income.

Table 7. Labor Market Performance of Primary Workers in the Village and at Migration Destinations

VARIABLES	(1) Workdays away per period	(2) Workdays at home per period	(3) Workdays at home per period	(4) Income away	(5) Income at home	(6) Income at home	(7) Income away per period	(8) Income at home per period	(9) Income at home per period	(10) Number of periods away	(11) Number of periods home	(12) Periods without work away	(13) Periods without work at home	(14) Periods without work at home
Control	5.269	3.872	3.512	4,201	3,726	2,013	1,235	692.9	615.2	0.827	5.067	0.0645	0.568	0.575
Low Intensity	5.702	4.101	3.634	4,712	3,977	2,381	1,372	746.5	680.0	1.115	4.826	0.0455	0.547	0.527
High Intensity	5.912	4.312	4.301	4,922	4,003	2,786	1,403	791.2	825.1	1.356	4.576	0.0163	0.488	0.388
Observations	774	2,128	598	774	2,128	598	774	2,128	598	2,304	2,304	774	2,128	598
Sample	Migrant HHs only sample	Full Sample	Migrant HHs only sample	Migrant HHs only sample	Full Sample	Migrant HHs only sample	Migrant HHs only sample	Full Sample	Migrant HHs only sample	Full Sample	Full Sample	Migrant HHs only sample	Full Sample	Migrant HHs only sample
p-value: Control = Low Intensity	.246	.217	.706	.29	.268	.104	.21	.183	.282	.065	.144	.55	.778	.671
p-value: Control = High Intensity	.079	.018	.006	.142	.217	0	.111	.012	0	.001	.005	.057	.352	.092
p-value: Low Intensity = High Intensity	.459	.255	.025	.639	.904	.094	.696	.263	.012	.209	.201	.181	.446	.126

The results in this table were generated using household level data from the high frequency survey which interviewed households 6 times between 22nd December 2014 to 28th February 2015.

Dependent variables marked "away" average only across those periods in which the primary worker was away, and similarly for "home".
Columns marked "Full Sample" compute averages of the dependent variables for all households in the sample. Columns marked "Migrant HHs only sample" restrict the analysis to households in which a household member migrated at some point in the survey period.

Dependent variables, all averaged across households in the sample: Columns (1)-(3) measure average days worked per household by the primary worker divided by the number of periods in the survey; (4)-(6) measure total income earned by the primary worker; (7)-(9) are similar to (4)-(6) but normalize by number of periods; (10)-(11) measure the total number of periods the primary worker was away or home (respectively) over the survey range; (12)-(14) measure the number of periods in which the primary worker was away or home (respectively) and reported not working during that period.

Table 8. Intent to Treat Effects on Labor Income and Working Days in the Village and at Migration Destinations (using High Frequency Labor Surveys)

Panel A. Full Sample of 133 villages

VARIABLES	(1) Income	(2) Income (home)	(3) Income (away)	(4) Days worked	(5) Days worked (home)	(6) Days worked (away)	(7) Daily income	(8) Daily income (home)	(9) Daily income (away)
Offered Grant in Low	377.5	226.1	184.7	1.330	1.077	0.230	6.374	2.404	16.75**
Intensity Treatment Village	(314.8)	(245.3)	(317.7)	(1.388)	(1.331)	(1.270)	(4.136)	(3.758)	(7.435)
Not Offered Grant in Low	37.79	269.2	-214.1	0.236	1.159	-1.034	2.908	2.910	10.93
Intensity Treatment Village	(309.9)	(223.9)	(297.4)	(1.483)	(1.316)	(1.201)	(4.579)	(3.901)	(8.367)
Offered Grant in High	1,263***	199.4	1,049***	4.839***	0.425	4.367***	10.31***	5.520*	7.159
Intensity Treatment Village	(359.5)	(227.9)	(383.7)	(1.637)	(1.287)	(1.638)	(3.726)	(3.222)	(5.377)
Not Offered Grant in High	419.4	-15.13	460.3	1.652	-0.316	2.002	3.521	0.275	2.223
Intensity Treatment Village	(342.9)	(261.9)	(356.1)	(1.529)	(1.358)	(1.543)	(3.630)	(3.536)	(6.804)
Observations	2,293	2,293	2,293	2,293	2,293	2,293	2,276	2,115	988
R-squared	0.063	0.055	0.044	0.070	0.068	0.047	0.063	0.088	0.123
Control Mean	6760.447	4429.536	2279.069	36.845	26.894	9.83	180.476	165.61	229.097
p-value: Offered High = Offered Low	.017	.923	.046	.03	.666	.023	.367	.422	.178
p-value: Non-Offered High = Non-Offered Low	.273	.329	.088	.377	.353	.07	.899	.532	.362

Panel B. Partial Sample of 117 villages with high quality data on village population

VARIABLES	(1) Income	(2) Income (home)	(3) Income (away)	(4) Days worked	(5) Days worked (home)	(6) Days worked (away)	(7) Daily income	(8) Daily income (home)	(9) Daily income (away)
Offered Grant in Low	411.9	273.4	171.1	1.628	1.461	0.178	5.219	1.973	15.32**
Intensity Treatment Village	(325.4)	(260.8)	(340.3)	(1.447)	(1.454)	(1.368)	(3.935)	(3.850)	(6.918)
Not Offered Grant in Low	87.33	282.1	-184.1	0.816	1.674	-0.999	1.677	0.341	11.91
Intensity Treatment Village	(318.6)	(239.1)	(300.0)	(1.520)	(1.417)	(1.213)	(4.811)	(4.057)	(8.157)
Offered Grant in High	1,401***	265.2	1,094**	5.830***	0.962	4.764**	8.276**	3.835	6.136
Intensity Treatment Village	(417.3)	(242.0)	(458.1)	(1.849)	(1.339)	(1.959)	(3.696)	(3.378)	(5.567)
Not Offered Grant in High	618.9*	106.8	534.8	2.619*	0.457	2.181	3.686	0.691	4.978
Intensity Treatment Village	(345.3)	(268.4)	(384.7)	(1.529)	(1.365)	(1.675)	(3.795)	(3.636)	(6.717)
Observations	2,032	2,032	2,032	2,032	2,032	2,032	2,016	1,878	864
R-squared	0.065	0.059	0.042	0.077	0.077	0.047	0.064	0.091	0.125
Control Mean	6760.447	4429.536	2279.069	36.845	26.894	9.83	180.476	165.61	229.097
p-value: Offered High = Offered Low	.027	.978	.077	.03	.767	.038	.481	.662	.18
p-value: Non-Offered High = Non-Offered Low	.157	.576	.097	.292	.471	.086	.704	.939	.466

Errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The results in this table were generated using household level data from the high frequency survey which interviewed households 6 times between 22nd December 2014 to 28th February 2015. All specifications include upazila FE.
The dependent variable in specification (1) is total income (in takas) generated by the household i.e. income generated from participation in the origin and the away (i.e. migrant) labor markets for the period covered by the high frequency survey. The dependent variables in specifications (2) and (3) are income (in takas) generated by the household from participation only in the origin labor market and income (in takas) generated by the household from participation only in the away (i.e. migrant) labor market respectively for the period covered by the high frequency survey. The dependent variable in specification (4) is the total number of days that working members of the household participated in the origin and the away (i.e. migrant) labor markets for the period covered by the high frequency survey. The dependent variables in specifications (5) and (6) are number of days that working members of the household participated only in the origin labor market and only in the away (i.e. migrant) labor market respectively, for the period covered by the high frequency survey. The dependent variable in specification (7) is the average daily wage rate across home and away labor markets, computed based on the reported income and days worked by the surveyed household for the period covered by the high frequency survey. The dependent variable in specification (8) is the average daily wage rate in the home labor market, computed based on the reported income and days worked by the surveyed household for the period covered by the high frequency survey. The dependent variable in specification (9) is the average daily wage rate in the away labor market, computed based on the reported income and days worked by the surveyed household for the period covered by the high frequency survey. Daily Income can be computed only for households that have positive number of days worked at that location. All specifications include Upazila fixed effects. Dependent variables were winsorized at the 98% level (values below the 1st percentile were set to the 1st percentile value and values above the 99th percentile were set to the 99th percentile value)

Table 9. LATE (IV) Estimates of the Effects of Migration on Labor Income and Days Worked in the Village and at Migration Destinations Uses High Frequency Labor Surveys. Only High Intensity Treatment Villages Compared to Control Villages.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Income	Income (home)	Income (away)	Days worked	Days worked (home)	Days worked (away)	Daily income	Daily income (home)
Migrated	9,135***	1,787	7,171***	34.15***	3.971	29.67***	80.05***	51.16*
	(2,452)	(1,881)	(1,567)	(11.89)	(9.678)	(6.953)	(24.78)	(29.44)
Observations	1,644	1,644	1,644	1,644	1,644	1,644	1,629	1,516
R-squared	-0.121	-0.238	0.478	-0.096	-0.021	0.504	-0.094	-0.324
Upazila FE	YES	YES	YES	YES	YES	YES	YES	YES
Period	ALL	ALL	ALL	ALL	ALL	ALL	ALL	ALL
1st-Stage Instruments	High Intensity Offered, High Intensity Nonoffered	High Intensity Offered, High Intensity Nonoffered	High Intensity Offered, High Intensity Nonoffered	High Intensity Offered, High Intensity Nonoffered	High Intensity Offered, High Intensity Nonoffered	High Intensity Offered, High Intensity Nonoffered	High Intensity Offered, High Intensity Nonoffered	High Intensity Offered, High Intensity Nonoffered
Control Mean	6760.447	4429.536	2279.069	36.845	26.894	9.83	180.476	165.61
First Stage Partial R^2	0.014	0.014	0.014	0.014	0.014	0.014	0.014	0.012
First Stage F-test Statistic	5.636	5.636	5.636	5.636	5.636	5.636	5.217	4.500
First Stage p-value	0.005	0.005	0.005	0.005	0.005	0.005	0.007	0.014

Errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The results in this table show a set of IV specifications that were generated using household level data from the high frequency survey which interviewed households 6 times between 22nd December 2014 to 28th February 2015. The data across six rounds of surveys are pooled.

The dependent variable in specification (1) is total income (in takas) generated by the household i.e. income generated from participation in the origin and the away (i.e. migrant) labor markets for the period covered by the high frequency survey. The dependent variables in specifications (2) and (3) are income (in takas) generated by the household from work at their origin location and income (in takas) generated by the household from work in the away (i.e. migrant) location respectively for the period covered by the high frequency survey. "Work" mostly refers to labor market (wage) income, but could also include some self-employment or business income. The dependent variable in specification (4) is the total number of days that working members of the household participated in the origin and the away (i.e. migrant) labor markets for the period covered by the high frequency survey. The dependent variables in specifications (5) and (6) are number of days that working members of the household participated only in the origin labor market and only in the away (i.e. migrant) labor market respectively, for the period covered by the high frequency survey. The dependent variable in specification (7) is the average daily wage rate across home and away labor markets, computed based on the reported income and days worked by the surveyed household for the period covered by the high frequency survey. The dependent variable in specification (8) is the average daily wage rate in the home labor market, computed based on the reported income and days worked by the surveyed household for the period covered by the high frequency survey. "Migrated"=1 if at least one member of household migrated during the entire period covered by High Frequency Labor Surveys. This variable is instrumented in a 2SLS regression using assignment to treatment (High and Low Intensity, Offered and Non-offered). All specifications include Upazila fixed effects. Dependent variables were winsorized at the 98% level (values below the 1st percentile were set to the 1st percentile value and values above the 99th percentile were set to the 99th percentile value)

Table 10. LATE (IV) Estimates of the Effects of Emigration on Wages Paid in the Home Village as Reported by Employers

VARIABLES	(1) Male wage for agricultural work	(2) Male wage for non-agricultural work	(3) Male wage for agricultural work (log)	(4) Male wage for non-agricultural work (log)	(5) Male wage for agricultural work	(6) Male wage for non-agricultural work	(7) Male wage for agricultural work (log)	(8) Male wage for non-agricultural work (log)
Share of eligible villagers who migrated in 2015-2016	41.36*	2.682	0.216**	0.0647	28.75	-7.485	0.153*	0.0153
	(23.87)	(31.70)	(0.108)	(0.134)	(19.94)	(24.65)	(0.0889)	(0.103)
Observations	338	247	338	247	385	276	385	276
R-squared	0.518	0.259	0.503	0.260	0.557	0.260	0.547	0.265
Upazila FE	YES	YES	YES	YES	YES	YES	YES	YES
Period	ALL	ALL	ALL	ALL	ALL	ALL	ALL	ALL
1st-Stage	High Intensity	High Intensity	High Intensity	High Intensity	High Intensity	High Intensity	High Intensity	High Intensity
Sample	117 villages	117 villages	117 villages	117 villages	133 villages	133 villages	133 villages	133 villages
First Stage Partial R^2	0.463	0.377	0.463	0.377	0.525	0.437	0.525	0.437
First Stage F-test Statistic	57.60	31.67	57.60	31.67	83.32	43.11	83.32	43.11
First Stage p-value	1.04e-10	4.70e-07	1.04e-10	4.70e-07	0	8.52e-09	0	8.52e-09

Standard errors clustered at the village level reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Uses data from the employer survey which interviewed agricultural and non-agricultural employers across all villages in the sample, and asked about wages paid during the period of out-migration. The survey asked separately about male and female wages, and about agricultural and non-agricultural wages.

The dependent variable is regressed on the proportion of the eligible population that migrated in each village. This eligible population are landless households, which is the relevant labor force competing with migrants. This was constructed as a ratio of total migrant households in a village and total eligible households in a village. The number of eligible households was available based on previous census data. The total number of migrants was constructed using the same data and formulas used in Table 1. The independent variable was intrumented with village level assignment to the high intensity treatment.

Columns (1)-(4) restrict the sample to the 117 villages where we have the highest quality listing data on numbers of total and eligible landless households in the village. Columns (5)-(8) use the full, unrestricted sample. Dependent variables are average wages paid by employers for: (1) males in agricultural work; (2) males in non-agricultural work; (3)-(4) identical to (1)-(2) but using log wages; (5)-(8) identical to (1)-(4) but with full sample (see above). Wage variables presented in this table do not include the value of food transferred by employers. Employers sometimes compensate workers with food along with money wage. We present results for wage without food because that was more consistently reported.

All specifications include Upazila fixed effects. Dependent variables were winsorized at the 98% level (values below the 1st percentile were set to the 1st percentile value and values above the 99th percentile were set to the 99th percentile value)

Table 11. Intent-to-Treat Effects on Employment Outcomes Restricting Only to Contributions Made by Non-primary Workers (using High Frequency Labor Surveys)

VARIABLES	(1) Income	(2) Income (home)	(3) Income (away)	(4) Days worked	(5) Days worked (home)	(6) Days worked (away)	(7) Daily income	(8) Daily income (home)	(9) Daily income (away)
Low Intensity Treatment Village	-318.7	14.67	-351.8**	-1.252	0.221	-1.615***	-11.44	-5.752	12.10
	(203.4)	(101.6)	(152.3)	(1.051)	(0.763)	(0.593)	(8.483)	(7.963)	(13.28)
High Intensity Treatment Village	136.9	-18.84	133.3	0.574	-0.170	0.631	-6.091	-8.388	0.0824
	(228.7)	(90.62)	(189.9)	(1.085)	(0.626)	(0.774)	(8.070)	(7.464)	(9.291)
Observations	2,293	2,293	2,293	2,293	2,293	2,293	1,152	973	400
R-squared	0.021	0.031	0.016	0.048	0.074	0.021	0.104	0.155	0.180
Control Mean	2090.1	856.7	1212.0	12.2	6.9	5.2	149.2	121.2	232.1
Upazila FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Period	ALL	ALL	ALL	ALL	ALL	ALL	ALL	ALL	ALL
p-value: High Intensity = Low Intensity	.018	.731	.001	.067	.58	0	.524	.733	.288

Errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The results in this table were generated using household level data from the high frequency survey which interviewed households 6 times between 22nd December 2014 to 28th February 2015. The sample is restricted to the contributions of only non-primary working members to each of the outcomes. All specifications include Upazila fixed effects. Dependent variables were winsorized at the 98% level.

The dependent variable in specification (1) is total income (in takas) generated by the non-primary working members i.e. income generated by non-primary working members from participation in the origin and the away (i.e. migrant) labor markets for the period covered by the high frequency survey. The dependent variables in specifications (2) and (3) are income (in takas) generated by non-primary working members from participation only in the origin labor market and income (in takas) generated by non-primary working members from participation only in the away (i.e. migrant) labor market respectively for the period covered by the high frequency survey. The dependent variable in specification (4) is the total number of days that non-primary working members of the household participated in the origin and the away (i.e. migrant) labor markets for the period covered by the high frequency survey. The dependent variables in specifications (5) and (6) are number of days that non-primary working members of the household participated only in the origin labor market and only in the away (i.e. migrant) labor market respectively, for the period covered by the high frequency survey. The dependent variable in specification (7) is the average daily wage rate across home and away labor markets, computed based on the reported income and days worked by the non-primary working members for the period covered by the high frequency survey. The dependent variable in specification (8) is the average daily wage rate in the home labor market, while in specification (9) it is the average daily wage rate in the away labor market, computed based on the reported income and days worked by the non-primary working members for the period covered by the high frequency survey.

Table 12. Descriptive Statistics on Primary Worker

Panel A. Contributions of the Primary Worker Relative to Other Household Members

VARIABLES (% attributed to Primary Worker)		Control	Low Intensity Treatment Village	High Intensity Treatment Village	All
(1)	Income	0.78	0.84	0.81	0.81
(2)	Income Home	0.82	0.84	0.84	0.83
(3)	Income Away	0.57	0.72	0.69	0.66
(4)	Days Worked	0.75	0.81	0.79	0.78
(5)	Days Worked Home	0.79	0.80	0.80	0.80
(6)	Days Worked Away	0.57	0.72	0.68	0.66
(7)	Daily Income	0.78	0.81	0.80	0.79
(8)	Daily Income Home	0.82	0.84	0.84	0.83
(9)	Daily Income Away	0.88	0.92	0.91	0.90

Panel B. Frequency and Duration of Migration

VARIABLES		Control	Low Intensity Treatment Village	High Intensity Treatment Village	All
Proportion of Time Away	Mean	0.33	0.324	0.324	0.325
	Standard Deviation	0.194	0.186	0.182	0.185
Number of Episodes	Mean	1.509	1.559	1.565	1.556
	Standard Deviation	0.944	0.815	0.844	0.846

Panel C. Timing of Migration

VARIABLE	Control	Low Intensity Treatment Village	High Intensity Treatment Village	All
Probability that one migrant is away while another is home	0.767	0.74	0.742	0.762

The results in this table were generated using household level data from the high frequency survey which interviewed households 6 times between 22nd December 2014 to 28th February 2015.

Panel A describes the proportion of each dependent variable generated by the primary worker of the household. The dependent variables are as follows:

(1) Total income (in takas) i.e. income generated from participation in the origin and the away (i.e. migrant) labor markets for the period covered by the high frequency survey. (2) and (3) Income (in takas) generated from participation only in the origin labor market and income (in takas) generated from participation only in the away (i.e. migrant) labor market respectively for the period covered by the high frequency survey. (4) Total number of days that members of the household participated in the origin and the away (i.e. migrant) labor markets for the period covered by the high frequency survey. (5) and (6) Number of days that members of the household participated only in the origin labor market and only in the away (i.e. migrant) labor market respectively, for the period covered by the high frequency survey. (7) Average daily wage rate across home and away labor markets, computed based on the reported income and days worked by the household members for the period covered by the high frequency survey. (8) and (9) Average daily wage rate in the home labor market and the away labor market respectively, computed based on the reported income and days worked by the household members for the period covered by the high frequency survey.

Panel B measures, for each of the three treatment arms:

(a) The average proportion of the time covered by the survey in which a household member was away on a migration. To accomplish this, the survey range was divided into 31 periods, and each period was marked "1" if a household member was on migration and "0" otherwise. The proportion is the average of that variable for each household, restricted to households that had a migrant at some point in the survey range.

(b) The average number of migration episodes by any member of the household during the survey range, again restricted to households that had a migrant at some point in the survey range.

Panel C presents the probability that, for a given household in a given round of the interview with no members away, at least one other household within their village has a member away (i.e. a member who is migrant). This is generated by running 1,000 simulations in which two random households in an arbitrarily chosen village are chosen and checked to see whether one has a migrant while the other does not. The probability is the number of times this occurred divided by 1,000.

Table 13. Treatment Effects on Employment Outcomes Restricting Only to Contributions Made by Primary Workers while at Home (using High Frequency Labor Surveys)

VARIABLES	(1) Income (home)	(2) Days worked (home)	(3) Daily income (home)
Low Intensity Treatment Village	59.76**	0.231*	3.745
	(29.35)	(0.128)	(2.516)
High Intensity Treatment Village	88.03***	0.349***	4.776**
	(28.47)	(0.133)	(2.408)
Observations	9,730	9,730	8,310
R-squared	0.073	0.074	0.031
Control Mean	592.569	3.365	177.605
Period	ALL	ALL	ALL
p-value: High Intensity = Low Intensity	.316	.326	.672

Table 14. Treatment Effects on Employment Outcomes Restricting Only to Contributions Made by Primary Workers while at Home for Week when they Earned Highest Income (using High Frequency Labor Surveys)

VARIABLES	(1) Income (home)	(2) Days worked (home)	(3) Daily income (home)
Low Intensity Treatment Village	69.61*	0.264*	1.344
	(37.99)	(0.142)	(3.838)
High Intensity Treatment Village	74.36*	0.173	5.145
	(38.35)	(0.148)	(3.755)
Observations	1,985	1,985	1,901
R-squared	0.060	0.091	0.035
Control Mean	1088.652	5.498	199.889
Period	Highest Income Week	Highest Income Week	Highest Income Week
p-value: High Intensity = Low Intensity	.894	.466	.273

Errors clustered at the village level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The results in this table were generated using household level data from the high frequency survey which interviewed households 6 times between 22nd December 2014 to 28th February 2015. The sample is restricted to the contributions of only primary working members to each of the outcomes in the week for which they reported highest income (across all survey periods) and only employment outcomes at origin are studied. All specifications include Upazila fixed effects. Dependent variables were winsorized at the 98% level.

The dependent variable in specification (1) is total income (in takas) generated by the primary working member from participation in the origin labor market during the week in which they earned the highest income (across all survey rounds). The dependent variable in specification (2) is the total number of days that the primary working member of the household participated in the origin labor market during the week in which they earned the highest income (across all survey rounds). The dependent variable in specification (3) is the average daily wage rate in the home labor markets, computed based on the reported income and days worked by the primary working members during the week in which they earned the highest income (across all survey rounds)

Table 15. LATE (IV) Estimates of the Effect of Emigration on Employer Costs, Revenues and Profits

VARIABLES	(1) Wage bill per decimal (Aman 2015)	(2) Non-wage costs per decimal (Aman 2015)	(3) Costs per decimal (Aman 2015)	(4) Change in Costs per decimal from 2013 to 2015	(5) Revenues per decimal Aman 2015 (current)	(6) Change in Revenues per decimal from 2013 to 2015	(7) Profits per decimal Aman 2015 (current)	(8) Change in Profits per decimal from 2013 to 2015
Share of eligible villagers who migrated in 2015-2016	81.41	64.17	145.6	224.1**	-163.1	-83.04	-254.9**	-19.55
	(79.42)	(108.0)	(174.6)	(103.3)	(232.4)	(119.4)	(124.8)	(72.23)
Observations	626	626	626	626	626	626	626	626
R-squared	0.108	0.095	0.108	0.030	0.119	0.063	0.086	0.040
Control Mean	149.011	139.553	288.564	-25.507	367.521	1.68	83.361	-23.578
Control Median	122.125	103.634	232.33	4.962	254.545	-10.714	45.454	-18.399
Upazila FE	YES	YES	YES	YES	YES	YES	YES	YES
First Stage Partial R^2	0.305	0.305	0.305	0.305	0.305	0.305	0.305	0.305
First Stage F-test Statistic	24.56	24.56	24.56	24.56	24.56	24.56	24.56	24.56
First Stage p-value	8.94e-10	8.94e-10	8.94e-10	8.94e-10	8.94e-10	8.94e-10	8.94e-10	8.94e-10

Errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The IV results in these tables were generated with the 2016 Follow-up Employer Survey, combined with 2015 migration rates per village derived from the 2016 Follow-up Household Survey. Analysis is conducted at the village level. All money-related variables are measured in taka.

The dependent variable in specificaiton (1) is wage per decimal (land unit) paid by the employer in 2015 (including labor costs of land preparation, sowing, maintenance and harvesting). The dependent variable in specification (2) encompasses costs per decimal incurred by the employer non-wage costs. Dependent variables in specifications (3)-(4) are all measures of costs per decimal (land unit) paid by the employer: column (3) has cost per decimal for 2015 and column (4) has the change in costs per decimal from 2013 to 2015. Columns (5)-(6) use the same specifications, but applied to revenues earned by the employer. Columns (7)-(8) also have the same specifications, but applied to profits earned by the employer (revenues minus costs).

The dependent variable is regressed on the proportion of the eligible population (landless households, which is synonymous with the relevant labor force) that migrated in each village. This was constructed as a ratio of total migrant households in a village and total eligible households in a village. The number of eligible households was available based on previous census data. The total number of migrants was constructed using the same data and formulas used in Table 1. The independent variable was intrumented with village level assignment to the high intensity treatment. All specifications include Upazila fixed effects. Dependent variables were winsorized at the 98% level (values below the 1st percentile were set to the 1st percentile value and values above the 99th percentile were set to the 99th percentile value)

Table 16. LATE (IV) Estimates of the Effect of Emigration on Local Food Prices

VARIABLES	(1) log Rice (kg)	(2) log Flour (kg)	(3) log Pulses (kg)	(4) log Edible oil (liter)	(5) log Fish (kg)	(6) log Meat (kg)	(7) log Egg (per egg)	(8) log Milk (liter)	(9) log Salt (kg)	(10) log Sugar (kg)	(11) log Beverages	(12) log Prepared Food	(13) log Laspeyres index for 12 goods
Share of eligible villagers who migrated in 2014-2015	-0.0085	-0.0277	-0.0022	0.0321*	0.147*	0.0430	-0.0278	-0.0250	-0.0066	0.0121	-0.146*	-0.0459	0.0884**
	(0.00855)	(0.0171)	(0.0205)	(0.0190)	(0.0827)	(0.0456)	(0.0301)	(0.0324)	(0.0269)	(0.00977)	(0.0779)	(0.0565)	(0.0450)
Observations	2,375	2,375	2,375	2,375	2,375	2,375	2,375	2,375	2,375	2,375	2,375	2,375	2,375
R-squared	0.667	0.267	0.594	0.376	0.749	0.267	0.890	0.502	0.396	0.256	0.930	0.954	0.756
Mean	3.452	3.505	4.613	4.704	5.299	4.725	2.107	3.669	2.368	3.838	1.415	2.288	4.175
Upazila FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Period	ALL	ALL	ALL	ALL	ALL	ALL	ALL	ALL	ALL	ALL	ALL	ALL	ALL
Firststage_R2partial	0.398	0.398	0.398	0.398	0.398	0.398	0.398	0.398	0.398	0.398	0.398	0.398	0.398
Firststage_Ftest	57.12	57.12	57.12	57.12	57.12	57.12	57.12	57.12	57.12	57.12	57.12	57.12	57.12
Firststage_Pvalue	0	0	0	0	0	0	0	0	0	0	0	0	0

Errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The results in this table were generated using shopkeeper (grocery store) level data from the high frequency survey which interviewed them 6 times between 22nd December 2014 to 28th February 2015. The dependent variable in each specification is the price per unit of a given item of food in the local village market, measured in logs. The dependent variable in column (13) is log of the Laspeyres index of the preceding 12 items, defined as:

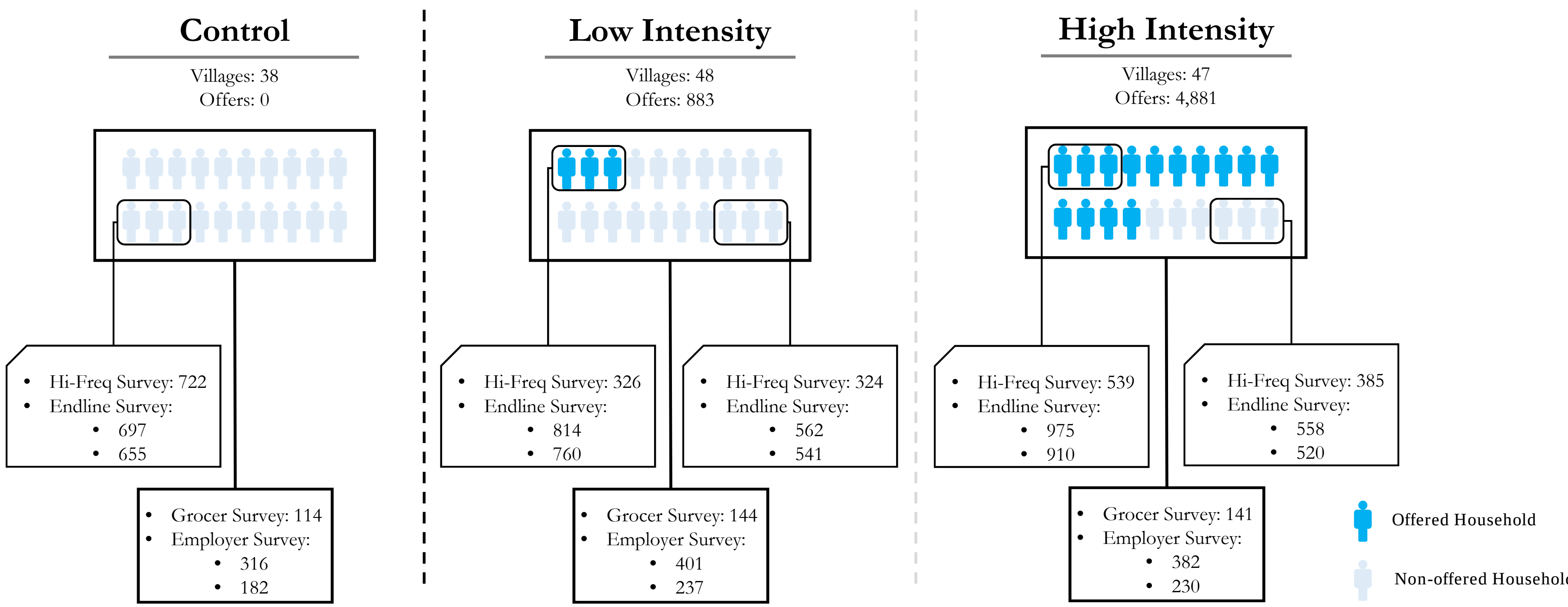
$$\frac{\sum_i p_{ni} * q_{0i}}{\sum_i p_{0i} * q_{0i}}$$

where for each good i, q0 is the quantity of the good consumed at baseline, p0 is the price of the good at baseline, and pn is the price of the good during each period of the High Frequency Origin Survey.

The dependent variable is regressed on the proportion of the eligible population (landless households, which is synonymous with the relevant labor force) that migrated in each village. This was constructed as a ratio of total migrant households in a village and total eligible households in a village. The number of eligible households was available based on previous census data. The total number of migrants was constructed using the same data and formulas used in Table 1. The independent variable was intrumented with village level assignment to the high intensity treatment.

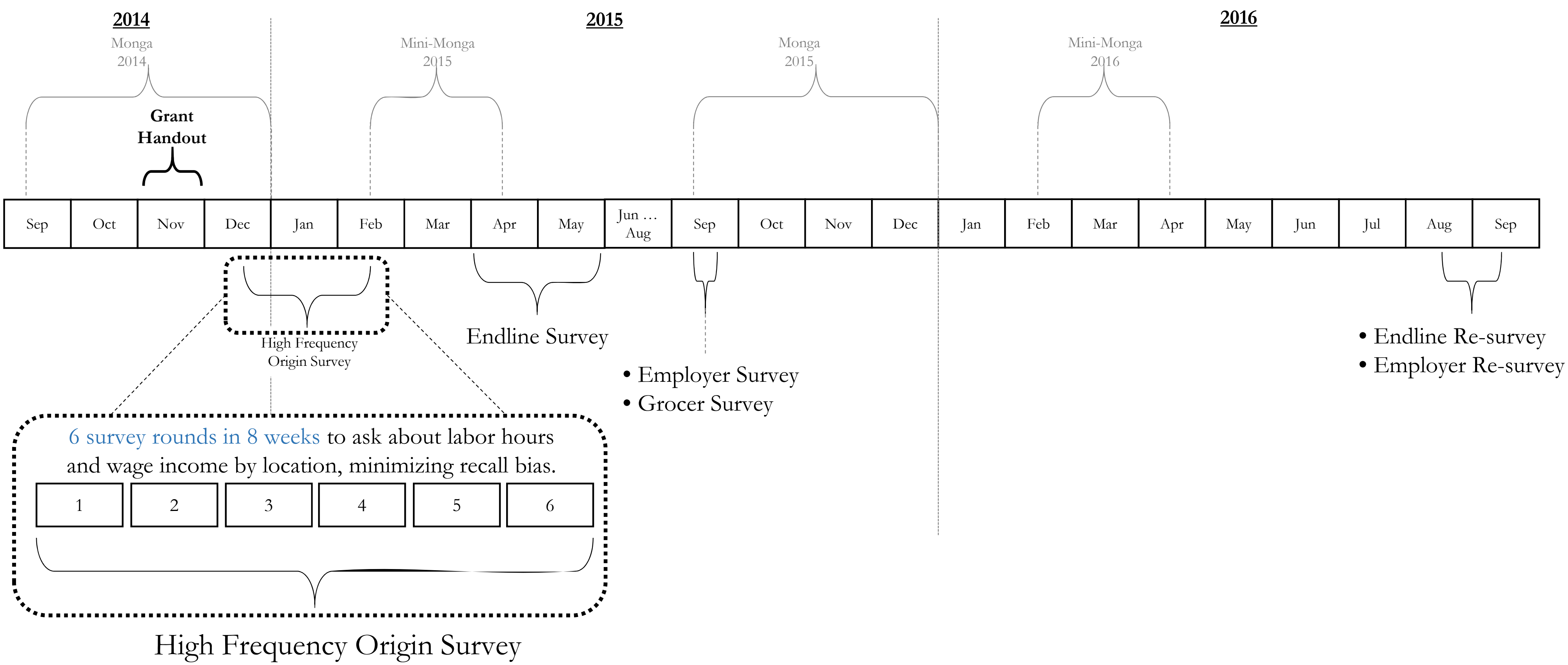
All specifications include Upazila fixed effects. Expenditure values below the 5th percentile were set to the 5th percentile value and values above the 95th percentile were set to the 95th percentile value.

Figure 1: Data Collection and Experimental Design



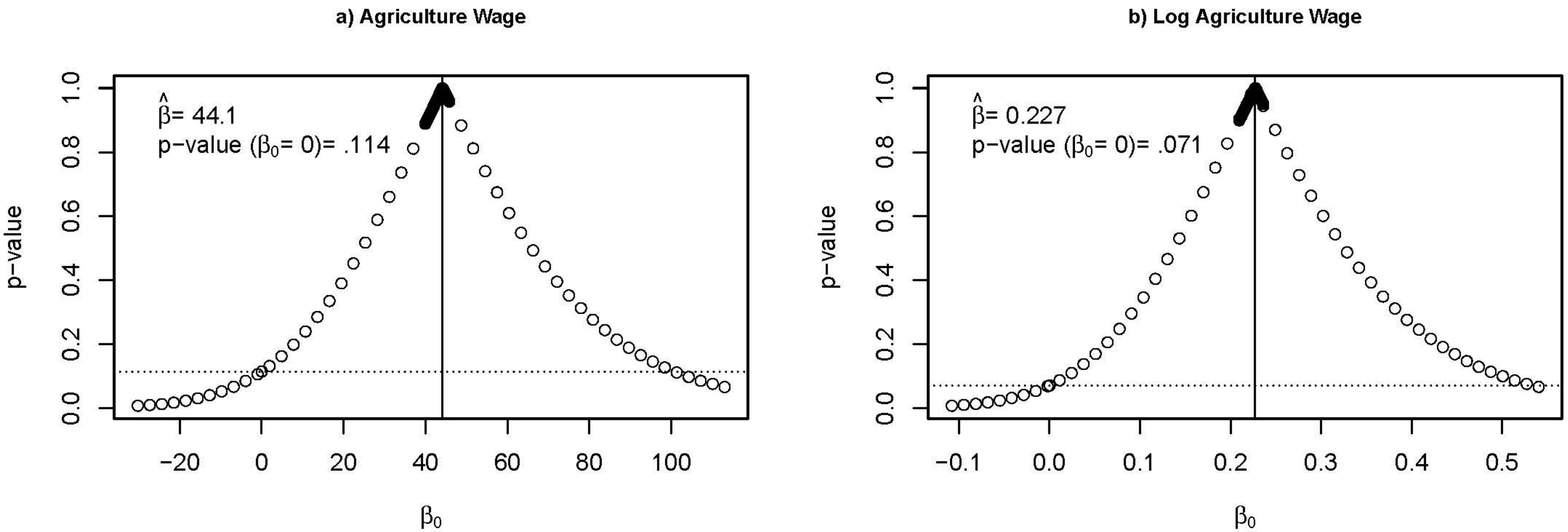
Boxes in upper-half denote experimental design, with light-blue figures representing eligible villagers not offered the travel grant and dark-blue representing eligible villagers offered the grant. Boxes in the bottom-half of the picture denote data collection. Boxes with a notched top-corner specify sample sizes for high-frequency and endline household surveys (of both offered and non-offered households). A detailed endline was administered in 2015 and a second, compact, endline was administered in 2016; the sample size for each of these is shown in the two sub-bullets under "Endline Survey". Unnotched boxes specify sample sizes for the grocer (shopkeeper) and employer surveys. The employer survey was administered twice – once in 2015 and once in 2016, with the respective sample sizes for each year specified by each sub-bullet under "Employer Survey".

Figure 2: Intervention and Data Collection Calendar



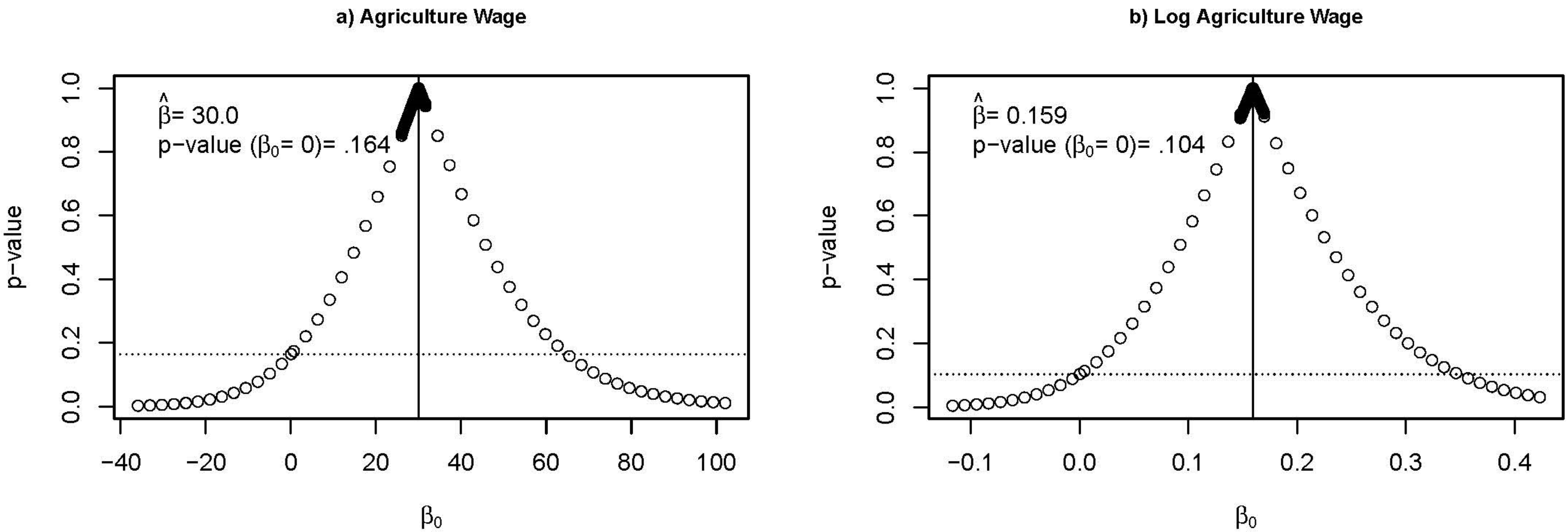
Occurrences of the major and minor lean seasons during the agricultural calendar over the period of study are above the timeline (indicated by gray braces), and the time that the intervention was carried out (indicated by bold brace). Below the middle-bar are the timing of surveys administered.

Figure 3: Randomization Inference P-Values for the Effect of Emigration on Agricultural Wages (117 Village Sample)



Figures show p-values produced via the randomization inference procedure outline in Appendix 2 using the partial sample of 117 villages. The x-axis, β_0, is the assumed effect of emigration on the (log) agricultural wage, analogous to the coefficient estimates in Table 10. The solid vertical line in each panel marks the point estimate, $\hat{\beta}$. The dotted horizontal line marks the p-value for the null hypothesis H_0: $\beta_0 = 0$.

Figure 4: Randomization Inference P-Values for the Effect of Emigration on Agricultural Wages (Full Sample)



Figures show p-values produced via the randomization inference procedure outline in Appendix 2 using the full sample of villages. The x-axis, β_0, is the assumed effect of emigration on the (log) agricultural wage, analogous to the coefficient estimates in Table 10. The solid vertical line in each panel marks the point estimate, $\hat{\beta}$. The dotted horizontal line marks the p-value for the null hypothesis H_0: β_0 = 0.

Online Appendices: Not for Publication

Appendix 1: Effects of the Experiment on the Equilibrium Wage Rate in the Origin Village

Suppose each landless household who has not migrated out has a Cobb-Douglas utility function,

$$U = L^{\alpha} C^{1-\alpha} \tag{4.2.1}$$

Where C denotes consumption goods measured in taka and L are hours of leisure. C is given by $C = wh + V$, where h is labor hours supplied within the village, w is wage in the village and V is outside income including income from migration. The time constraint function is given by $1 - h = L$

The household maximizes expected utility subject to the budget and time constraint,

$$\underset{h}{\text{Max}}\, U = (1-h)^{\alpha}(wh + V)^{1-\alpha} \tag{4.2.2}$$

The first order condition: $h = 1 - \alpha - \frac{\alpha V}{w}$ (4.2.3)

The labor supply function in this simple setting derived above depends on village wage, *w*, and outside income, V. Assume that the village has N workers in total. The total working hours (*TW*) that workers are willing to supply is,

$$TW = [\mathrm{N} - \mathrm{M(x)}][1 - \alpha - \frac{\alpha V}{w}] \tag{4.2.4}$$

Assume that the profit function for the landed farmers in the village is given by,

$$\pi = l^{\beta} k^{(1-\beta)} - wl - rk \tag{4.2.5}$$

where l is hired labor, w is the prevailing village wage, k are other inputs, and r is the (rental) price of those inputs. The labor demand of a landed farmer in the village can be expressed as,

$$l = k\left(\frac{\beta}{w}\right)^{\frac{1}{1-\beta}} \tag{4.2.6}$$

Given the fixed number of farmer-employers E within the village, the equilibrium occurs when,

$$[\mathrm{N} - \mathrm{M(x)}]\left[1 - \alpha - \frac{\alpha V}{w}\right] = Ek\left(\frac{\beta}{w}\right)^{\frac{1}{1-\beta}} \tag{4.2.7}$$

The FOC is,

$$\frac{\partial w}{\partial x} = -\frac{\frac{\partial F}{\partial x}}{\frac{\partial F}{\partial w}} = \frac{M'(x)}{[\mathrm{N} - \mathrm{M(x)}]\frac{\alpha V}{w^2} + \frac{1}{1-\beta}Ek\left(\frac{1}{w}\right)^{\gamma}} \tag{4.2.8}$$

Where $\gamma = \left(\frac{1}{1-\beta} + 1\right)$. Given that $M'(x) > 0$, and the denominator is positive, $\frac{\partial w}{\partial x} > 0$.

As for labor supply, $\frac{\partial h}{\partial x} = \frac{\partial h}{\partial w} \cdot \frac{\partial w}{\partial x} = \frac{\alpha V}{w^2}\left(\frac{M'(x)}{[\mathrm{N}-\mathrm{M(x)}]\frac{\alpha V}{w^2} + \frac{1}{1-\beta}Ek\left(\frac{1}{w}\right)^{\gamma}}\right) = \frac{\alpha V M'(x)}{w^2\left([\mathrm{N}-\mathrm{M(x)}]\frac{\alpha V}{w^2} + \frac{1}{1-\beta}Ek\left(\frac{1}{w}\right)^{\gamma}\right)}$ (4.2.9)

By the same logic described above $\frac{\partial h}{\partial x} > 0$.

Appendix 2: Randomization Inference to Test Statistical Precision of Village-Level Wage Results

For our estimates of the effect of migration on rural male agricultural wages, we compute non-parametric p-values via randomization inference that avoids distributional assumptions. These are plotted in Figures 3 and 4. To compute these p-values, we assume a sharp null hypothesis as originally proposed by Fisher (1935) where the wage paid by employer e in village v and sub-district j (W_{evj}) is a deterministic function of the local migration rate ($Migration\_Rate_{vj}$) and a sub-district fixed effect (φ_j).

$$W_{evj} = \alpha + \beta \cdot Migration\_Rate_{vj} + \varphi_j$$

The sharp null hypothesis allows us to calculate exactly the potential outcome associated with zero migration (W^0_{evj}) under each value of the null H_0: $\beta = \beta_0$.

$$W^0_{evj} = W_{evj} - \beta_0 \cdot Migration\_Rate_{vj}$$

We control for the sub-district by taking the residual of the potential outcome W^{0r}_{evj} that isn't explained by the sub-district.

$$W^{0r}_{evj} = W^0_{evj} - \widehat{W^0_{evj}}$$

Here, $\widehat{W^0_{evj}}$ is the expected value of the potential outcome controlling for sub-district using ordinary least squares regression.

Under the null hypothesis H_0: $\beta = \beta_0$, the potential outcomes are known constants, and there should be no relationship between our potential outcomes (or their residuals W^{0r}_{evj}) and our randomly assigned treatment. We therefore construct a test statistic T^{obs} that is the difference in the average residualized potential outcomes between high-intensity and control villages.

$$T^{obs} = \frac{\sum_{evj} W^{0r}_{evj} \cdot High\_I_{vj}}{\sum_{evj} High\_I_{vj}} - \frac{\sum_{evj} W^{0r}_{evj} \cdot (1 - High_{I_{vj}})}{\sum_{evj}(1 - High_{I_{vj}})}$$

We estimate the distribution of this test statistic under the null hypothesis by randomly re-assigning our village level treatment 100,000 times and recalculating the statistic under each re-assignment. Our p-value p for a given value of β_0 is the proportion of our placebo test statistics that are more extreme than the observed statistic.

$$p = \sum_{R=1}^{100{,}000} I\{|T^R| > |T^{obs}|\}$$

In Figure 3 and 4, we plot the p-values obtained from this procedure for the null hypothesis H_0: $\beta_0 = 0$ and 100 other values of the coefficient.

Appendix Table A1. Randomization Balance on Observables at Baseline

	Control	Low Intensity (L)	High Intensity (H)	L - C	p-Value	H - C	p-Value	Treat. - C	p-Value
Baseline Characteristics for 100 Villages Inducted in 2008									
Value of total purchased meat consumed per HH per month	86.55	62.44	64.18	-24.10*	0.05	-22.37	0.10	-23.25*	0.05
	(11.01)	(5.35)	(7.86)	(12.15)		(13.43)		(11.86)	
Value of total purchased milk-egg consumed per HH per month	33.73	32.29	37.12	-1.44	0.69	3.39	0.40	0.92	0.78
	(2.49)	(2.7)	(3.23)	(3.64)		(4.04)		(3.23)	
Value of total purchased fish consumed per HH per month	156.70	150.18	160.97	-6.52	0.66	4.27	0.79	-1.21	0.93
	(11.2)	(9.69)	(11.31)	(14.69)		(15.79)		(13.32)	
Household size	4.04	3.87	4.00	-0.17	0.12	-0.04	0.70	-0.10	0.26
	(0.08)	(0.08)	(0.07)	(0.11)		(0.10)		(0.09)	
Value of purchased food consumed per HH per month	2161.77	1982.55	1975.06	-179.23*	0.08	-186.72*	0.07	-182.89**	0.05**
	(82.94)	(57.89)	(58.94)	(100.40)		(101.00)		(91.73)	
Monthly total food expenditure	2988.85	2863.19	3002.75	-125.66	0.23	13.89	0.89	-57.12	0.55
	(86.08)	(57.35)	(56.85)	(102.68)		(102.40)		(94.50)	
Value of medical exp incurred for males per HH per month	91.95	71.91	81.05	-20.03*	0.07	-10.90	0.39	-15.55	0.14
	(8.96)	(5.98)	(8.89)	(10.69)		(12.53)		(10.33)	
Value of medical exp incurred for females per HH per month	67.44	72.55	54.42	5.12	0.60	-13.02	0.12	-3.83	0.63
	(6.44)	(7.25)	(5.12)	(9.62)		(8.16)		(7.83)	
Value of clothes and shoes in 3 months per HH	140.70	137.15	150.45	-3.56	0.66	9.75	0.24	2.98	0.68
	(5.98)	(5.37)	(5.79)	(7.97)		(8.25)		(7.13)	
Value of edu exp in 3 months per	30.12	28.80	29.26	-1.31	0.74	-0.86	0.82	-1.09	0.75
	(3.14)	(2.39)	(2.07)	(3.91)		(3.73)		(3.47)	
Montly total non-food expenditure	924.15	952.88	962.29	28.72	0.55	38.14	0.42	33.34	0.42
	(33.87)	(34.37)	(32.67)	(47.90)		(46.70)		(40.94)	
Calorie intake per person per day	1,960.56	1,955.12	1,984.76	-5.44	0.89	24.20	0.53	9.13	0.79
	(27.95)	(30.43)	(26.32)	(41.02)		(38.10)		(34.17)	
Total Calories (per person per day)	2,062.61	2,047.24	2,095.15	-15.36	0.71	32.54	0.39	8.22	0.81
	(29.13)	(29.2)	(24.7)	(40.94)		(37.91)		(34.64)	
Number of males in HH	2.03	1.91	2.05	-0.12	0.11	0.02	0.76	-0.05	0.44
	(0.06)	(0.06)	(0.04)	(0.08)		(0.07)		(0.07)	
Number of females in HH	2.01	1.94	1.95	-0.06	0.29	-0.05	0.35	-0.06	0.25
	(0.05)	(0.05)	(0.04)	(0.06)		(0.05)		(0.05)	
Number of children in HH aged 0-18	1.90	1.78	1.83	-0.11	0.22	-0.07	0.45	-0.09	0.26
	(0.07)	(0.07)	(0.06)	(0.09)		(0.09)		(0.08)	
Number of children in HH aged 6-18 attending school	0.76	0.80	0.78	0.03	0.53	0.02	0.74	0.03	0.59
	(0.04)	(0.04)	(0.04)	(0.05)		(0.05)		(0.05)	
Subjective expectation: Monga occurrence this year	77.79	78.76	77.96	0.97	0.68	0.18	0.95	0.58	0.79
	(1.86)	(1.52)	(1.8)	(2.38)		(2.56)		(2.17)	
Subjective expectation: Can send remittance from Dhaka	58.51	59.27	59.31	0.76	0.73	0.80	0.72	0.78	0.68
	(1.6)	(1.5)	(1.54)	(2.17)		(2.19)		(1.90)	
Subjective expectation: Will get social network help in Dhaka	49.71	53.26	54.21	3.54	0.14	4.50*	0.06	4.02**	0.05**
	(1.56)	(1.81)	(1.84)	(2.37)		(2.39)		(2.00)	
HH Head Education (1=Educated)	0.26	0.23	0.24	-0.04	0.20	-0.02	0.42	-0.03	0.23
	(0.03)	(0.02)	(0.03)	(0.03)		(0.03)		(0.03)	
Number of Males Age>14	1.20	1.15	1.19	-0.06	0.15	-0.01	0.84	-0.03	0.35
	(0.04)	(0.03)	(0.03)	(0.04)		(0.04)		(0.03)	
Number of Children Age<9	1.07	1.01	1.03	-0.06	0.41	-0.04	0.50	-0.05	0.40
	(0.06)	(0.05)	(0.04)	(0.07)		(0.06)		(0.06)	

Appendix Table A1. (Continued) Randomization Balance on Observables at Baseline

	Control	Low Intensity (L)	High Intensity (H)	L - C	p-Value	H - C	p-Value	Treat. - C	p-Value
Baseline Characteristics for 33 Villages Inducted in 2011									
Value of total purchased meat consumed per HH per month	18.39	18.87	11.10	0.48	0.94	-7.29	0.23	-3.40	0.56
	(5.56)	(4.57)	(3.04)	(6.83)		(5.93)		(5.77)	
Value of total purchased milk-egg consumed per HH per month	6.66	5.83	5.76	-0.83	0.75	-0.91	0.74	-0.87	0.73
	(2.61)	(0.9)	(1.13)	(2.56)		(2.65)		(2.47)	
Value of total purchased fish consumed per HH per month	565.28	588.74	590.72	23.46	0.61	25.45	0.57	24.46	0.56
	(43.52)	(22.13)	(17.21)	(45.66)		(43.53)		(41.87)	
Household size	4.20	4.05	4.11	-0.15	0.28	-0.09	0.47	-0.12	0.31
	(0.12)	(0.09)	(0.06)	(0.14)		(0.12)		(0.12)	
Value of purchased food consumed per HH per month	602.76	631.21	619.53	28.45	0.58	16.76	0.73	22.62	0.62
	(45.84)	(28.23)	(20.85)	(50.62)		(46.97)		(45.09)	
Value of medical exp incurred for males per HH per month	13.96	36.02	27.99	22.05***	0.01**	14.03***	0.01**	18.04***	0.01**
	(2.45)	(5.64)	(3.54)	(6.02)		(4.16)		(4.02)	
Value of medical exp incurred for females per HH per month	14.15	22.62	25.24	8.46	0.20	11.09*	0.07	9.77**	0.05**
	(3.37)	(5.63)	(4.82)	(6.38)		(5.69)		(4.75)	
Value of clothes and shoes in 3 months per HH	147.68	157.55	153.22	9.87	0.36	5.54	0.59	7.69	0.41
	(9.12)	(6.36)	(5.77)	(10.49)		(10.15)		(9.30)	
Value of edu exp in 3 months per	32.07	42.62	36.22	10.55	0.18	4.15	0.60	7.33	0.31
	(7.11)	(3.83)	(4.39)	(7.56)		(7.85)		(7.09)	
Montly total non-food expenditure	14,966	17,022	15,531	2,056.09	0.18	565.05	0.70	1,300.36	0.33
	(1270.04)	(889.74)	(832.88)	(1,464.33)		(1,431.04)		(1,306.78)	
Number of males in HH	2.23	2.08	2.15	-0.15	0.24	-0.08	0.47	-0.12	0.30
	(0.12)	(0.07)	(0.05)	(0.12)		(0.11)		(0.11)	
Number of females in HH	1.97	1.97	1.97	0.00	0.99	-0.00	0.98	-0.00	1.00
	(0.12)	(0.05)	(0.05)	(0.12)		(0.12)		(0.11)	
Number of children in HH aged 0-18	4.10	3.94	4.08	-0.16	0.18	-0.02	0.84	-0.09	0.37
	(0.1)	(0.08)	(0.07)	(0.11)		(0.10)		(0.10)	
Number of children in HH aged 6-18 attending school	4.10	3.94	4.08	-0.16	0.18	-0.02	0.84	-0.09	0.37
	(0.1)	(0.08)	(0.07)	(0.11)		(0.10)		(0.10)	
Subjective expectation: Monga occurrence this year	37.58	38.74	36.84	1.17	0.79	-0.74	0.86	0.21	0.95
	(3.43)	(3.03)	(2.9)	(4.35)		(4.26)		(3.73)	
Subjective expectation: Can send remittance from Dhaka	57.37	53.00	52.29	-4.37	0.25	-5.08	0.12	-4.72	0.11
	(2.36)	(3.04)	(2.27)	(3.71)		(3.12)		(2.83)	
Subjective expectation: Will get social network help in Dhaka	48.84	45.23	46.81	-3.62	0.21	-2.03	0.55	-2.83	0.23
	(1.38)	(2.53)	(3.08)	(2.80)		(3.30)		(2.32)	
HH Head Education (1=Educated)	0.15	0.28	0.30	0.13**	0.04**	0.16**	0.02**	0.14**	0.02**
	(0.06)	(0.04)	(0.04)	(0.06)		(0.06)		(0.05)	
Number of Males Age>14	1.27	1.25	1.30	-0.01	0.79	0.03	0.46	0.01	0.82
	(0.03)	(0.05)	(0.04)	(0.05)		(0.04)		(0.04)	
Number of Children Age<9	0.91	0.93	0.93	0.02	0.89	0.01	0.90	0.02	0.89
	(0.11)	(0.09)	(0.06)	(0.12)		(0.11)		(0.10)	

Appendix Table A2. Robustness Checks on Effects of Treatment on Migration

VARIABLES	Dependent Variable: At least one migrant in Household (2014-15)												
Offered Grant in Low Intensity Treatment Village	0.248***	0.238***	0.253***	0.263***	0.253***	0.266***	0.248***	0.238***	0.215***	0.228***	0.215***	0.215***	0.214***
	(0.0366)	(0.0371)	(0.0396)	(0.0672)	(0.0368)	(0.0685)	(0.0402)	(0.0412)	(0.0350)	(0.0411)	(0.0350)	(0.0350)	(0.0351)
Not Offered Grant in Low Intensity Treatment Village	0.0333	0.0246	0.0368	0.0174	0.0360	0.0286	0.0292	0.0198		0.0137			
	(0.0388)	(0.0419)	(0.0402)	(0.0436)	(0.0386)	(0.0394)	(0.0415)	(0.0447)		(0.0429)			
Offered Grant in High Intensity Treatment Village	0.398***	0.391***	0.399***	0.410***	0.398***	0.413***	0.394***	0.386***	0.364***	0.379***	0.301***	0.301***	0.368***
	(0.0333)	(0.0353)	(0.0335)	(0.0337)	(0.0332)	(0.0348)	(0.0344)	(0.0365)	(0.0360)	(0.0380)	(0.0327)	(0.0328)	(0.0358)
Not Offered Grant in High Intensity Treatment Village	0.0965**	0.0882**	0.0987**	0.0858**	0.0974**	0.0942**	0.0934**	0.0846*	0.0632	0.0777*			0.0650
	(0.0397)	(0.0416)	(0.0412)	(0.0411)	(0.0398)	(0.0393)	(0.0417)	(0.0438)	(0.0412)	(0.0436)			(0.0417)
Village with Agricultural Households Targeted		0.0177						0.0180					
		(0.0272)						(0.0273)					
Household Received Incentive in 2013			-0.00608	0.0269			0.00402	0.00499					
			(0.0209)	(0.0308)			(0.0220)	(0.0223)					
Household Received Incentive in 2011							-0.0154	-0.0169					
							(0.0320)	(0.0317)					
Household Received Incentive in 2008							-0.0211	-0.0202					
							(0.0336)	(0.0332)					
Household Received any Incentive Over All Years (2008, 2011, 2013)					-0.00801	0.0129							
					(0.0183)	(0.0241)							
Household Located in Treatment Village in 2013									0.0333				0.0363
									(0.0388)				(0.0393)
Household Located in Treatment Village in 2011													-0.0189
													(0.0273)
Household Located in Treatment Village in 2008													0.00373
													(0.0252)
Village Was a Treatment Village in 2008, 2011 or 2013										0.0467			
										(0.0522)			
Interaction: Low Intensity and Offered X Received Incentive in 2013				-0.0447									
				(0.0784)									
Interaction: High Intensity and Offered X Received Incentive in 2013				-0.0727									
				(0.0441)									
Interaction: Low Intensity and Offered X Received Incentive in any year						-0.0309							
						(0.0741)							
Interaction: High Intensity and Offered X Received Incentive in any year						-0.0589							
						(0.0402)							
Interaction: High Intensity Village X Treatment Village in 2013											0.0632		
											(0.0412)		
Interaction: Low Intensity Village X Treatment Village in any year												0.0137	
												(0.0429)	
Interaction: High Intensity Village X Treatment Village in any year												0.0777*	
												(0.0436)	
Observations	3,600	3,600	3,600	3,600	3,600	3,600	3,600	3,600	3,600	3,600	3,600	3,600	3,600
Mean	0.343	0.343	0.343	0.343	0.343	0.343	0.343	0.343	0.343	0.343	0.343	0.343	0.343

Errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The results in this table were generated using household level data from the endline survey.
The dependent variable is an indicator for whether the household had at least one migrant over the period September 15 2014 - April 30 2015.
All specifications include Upazila fixed effects (an Upazila is an administrative unit that encompasses groups of villages in the sample; there are a total of 14 Upazilas across our sample of villages).

Appendix Table A3. Robustness Checks on Effects of Migration on Migration Income (Endline Survey)

VARIABLES	(1) Migration income	(2) Migration income	(3) Migration income	(4) Migration income	(5) Migration income	(6) Migration income	(7) Migration income	(8) Migration income	(9) Migration income	(10) Migration income	(11) Migration income	(12) Migration income	(13) Migration income
Offered Grant in Low Intensity Treatment Village	3,516***	3,656***	3,793***	4,162***	3,727***	4,192***	3,710***	3,873***	2,613***	3,517***	2,613***	3,517***	2,608***
	(712.7)	(726.7)	(805.5)	(1,284)	(740.6)	(1,297)	(818.6)	(886.1)	(738.7)	(791.7)	(738.7)	(791.7)	(738.6)
Not Offered Grant in Low Intensity Treatment Village	902.7	1,031	1,090	670.4	1,018	854.4	936.6	1,083		904.2		904.2	
	(730.3)	(823.6)	(813.2)	(886.7)	(756.9)	(774.7)	(848.2)	(968.0)		(834.0)		(834.0)	
Offered Grant in High Intensity Treatment Village	4,815***	4,913***	4,899***	5,105***	4,821***	5,133***	4,777***	4,887***	3,912***	4,816***	3,912***	4,816***	3,967***
	(680.1)	(770.5)	(705.8)	(748.9)	(680.0)	(768.0)	(718.5)	(823.1)	(814.9)	(787.1)	(814.9)	(787.1)	(813.3)
Not Offered Grant in High Intensity Treatment Village	1,559**	1,681**	1,678**	1,398*	1,601**	1,528**	1,566**	1,702*	656.4	1,561*	656.4	1,561*	690.0
	(732.7)	(817.1)	(751.3)	(777.2)	(734.6)	(733.8)	(768.6)	(874.5)	(853.4)	(827.8)	(853.4)	(827.8)	(849.9)
Village with Agricultural Households Targeted		-259.6						-279.4					
		(667.3)						(672.3)					
Household Received Incentive in 2013			-328.3	388.7			-138.2	-153.2					
			(447.7)	(663.8)			(502.4)	(512.7)					
Household Received Incentive in 2011							-62.09	-39.07					
							(695.4)	(702.6)					
Household Received Incentive in 2008							-704.4	-717.8					
							(668.6)	(672.0)					
Household Received any Incentive Over All Years (2008, 2011, 2013)					-349.6	119.7							
					(381.8)	(505.5)							
Household Located in Treatment Village in 2013									902.7				964.0
									(730.3)				(743.4)
Household Located in Treatment Village in 2011													-310.2
													(618.8)
Household Located in Treatment Village in 2008													11.53
													(533.9)
Village Was a Treatment Village in 2008, 2011 or 2013										-3.468			
										(915.2)			
Interaction: Low Intensity and Offered X Received Incentive in 2013				-1,152									
				(1,411)									
Interaction: High Intensity and Offered X Received Incentive in 2013				-1,488									
				(987.9)									
Interaction: Low Intensity and Offered X Received Incentive in any year						-885.9							
						(1,334)							
Interaction: High Intensity and Offered X Received Incentive in any year						-1,223							
						(895.3)							
Observations	3,600	3,600	3,600	3,600	3,600	3,600	3,600	3,600	3,600	3,600	3,600	3,600	3,600
Mean	5015.7	5015.7	5015.7	5015.7	5015.7	5015.7	5015.7	5015.7	5015.7	5015.7	5015.7	5015.7	5015.7

Errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The results in this table use household level data from the 2014-15 endline survey. The dependent variable is gross income from migration that migrants generated during the period September 15 2014 - April 30 2015. There are a few massive outliers in reported income, and all columns therefore trim out the extreme 1% of values for the dependent variable (top and bottom). All specifications include Upazila fixed effects. Dependent variables were winsorized at the 98% level

Appendix Table A4. Robustness Checks on Effects of Migration on Total Income (From High-Frequency Surveys)

VARIABLES	Income	Income	Income	Income	Income	Income	Income	Income	Income	Income	Income	Income	Income
Offered Grant in Low Intensity Treatment Village	377.5	523.4	717.7*	2,341***	584.7	2,336***	712.5*	875.7*	339.7	701.1*	339.7	340.4	339.0
	(314.8)	(358.7)	(407.6)	(778.3)	(375.2)	(778.5)	(418.3)	(457.3)	(302.4)	(375.1)	(302.4)	(302.5)	(302.5)
Not Offered Grant in Low Intensity Treatment Village	37.79	183.1	241.8	-7.682	133.9	37.84	270.6	432.9		360.7			
	(309.9)	(347.5)	(339.6)	(359.2)	(326.1)	(335.9)	(354.8)	(393.4)		(367.7)			
Offered Grant in High Intensity Treatment Village	1,263***	1,372***	1,375***	1,446***	1,283***	1,443***	1,402***	1,522***	1,226***	1,575***			1,227***
	(359.5)	(406.7)	(374.6)	(391.9)	(360.0)	(386.5)	(372.4)	(421.6)	(362.8)	(406.5)			(355.7)
Not Offered Grant in High Intensity Treatment Village	419.4	549.6	561.5	383.4	462.8	414.0	587.9	730.6*	381.6	725.9*	-844.0**	-849.3**	384.3
	(342.9)	(387.6)	(355.2)	(347.6)	(346.4)	(346.6)	(358.0)	(404.7)	(347.0)	(393.9)	(338.2)	(338.6)	(348.7)
Village with Agricultural Households Targeted		-287.4						-299.4					
		(333.2)						(335.9)					
Household Received Incentive in 2013			-375.0	72.73			-410.1	-426.5					
			(270.5)	(328.4)			(315.9)	(312.7)					
Household Received Incentive in 2011							-257.4	-234.0					
							(360.6)	(364.2)					
Household Received Incentive in 2008							425.3	432.1					
							(387.1)	(388.4)					
Household Received any Incentive Over All Years (2008, 2011, 2013)					-305.7	-17.61							
					(244.8)	(297.9)							
Household Located in Treatment Village in 2013									37.79				61.05
									(309.9)				(324.1)
Household Located in Treatment Village in 2011													-99.76
													(293.6)
Household Located in Treatment Village in 2008													-2.979
													(255.5)
Village Was a Treatment Village in 2008, 2011 or 2013										-754.5*			
										(437.1)			
Interaction: Low Intensity and Offered X Received Incentive in 2013				-2,234***									
				(836.1)									
Interaction: High Intensity and Offered X Received Incentive in 2013				-702.2									
				(582.6)									
Interaction: Low Intensity and Offered X Received Incentive in any year						-2,142**							
						(828.5)							
Interaction: High Intensity and Offered X Received Incentive in any year						-612.5							
						(576.1)							
Interaction: Low Intensity Village X Treatment Village in 2013											37.79		
											(309.9)		
Interaction: High Intensity Village X Treatment Village in 2013											1,263***		
											(359.5)		
Interaction: Low Intensity Village X Treatment Village in any year												360.7	
												(367.7)	
Interaction: High Intensity Village X Treatment Village in any year												1,575***	
												(406.5)	
Observations	2,293	2,293	2,293	2,293	2,293	2,293	2,293	2,293	2,293	2,293	2,293	2,293	2,293
Mean	6760.4	6760.4	6760.4	6760.4	6760.4	6760.4	6760.4	6760.4	6760.4	6760.4	6760.4	6760.4	6760.4

Errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The results in this table were generated using household level data from the high frequency survey which interviewed households 6 times between 22nd December 2014 to 28th February 2015. All specifications include Upazila fixed effects. Dependent variables were winsorized at the 98% level

The dependent variable is total income (in takas) generated by the household i.e. income generated from participation in the origin and the away (i.e. migrant) labor markets for the period covered by the high frequency survey.

Appendix Table A5. Robustness Checks on Effects of Migration on Income Earned at Destination (From High-Frequency Surveys)

VARIABLES	Dependent Variable: Income Earned Away from the Village												
Offered Grant in Low Intensity Treatment Village	184.7	329.7	420.6	2,066**	365.1	2,077**	374.2	524.0	398.7	473.8	398.7	399.4	394.0
	(317.7)	(353.1)	(411.5)	(1,021)	(364.2)	(1,020)	(417.0)	(464.7)	(289.1)	(353.2)	(289.1)	(289.2)	(289.1)
Not Offered Grant in Low Intensity Treatment Village	-214.1	-69.68	-72.54	-387.2	-130.4	-236.8	-133.9	14.95		74.45			
	(297.4)	(346.3)	(334.2)	(357.1)	(310.1)	(319.9)	(340.8)	(399.5)		(330.6)			
Offered Grant in High Intensity Treatment Village	1,049***	1,157**	1,126***	1,249***	1,066***	1,263***	1,085***	1,196**	1,263***	1,328***			1,181***
	(383.7)	(464.9)	(400.7)	(421.9)	(383.7)	(419.9)	(398.6)	(485.1)	(421.6)	(423.6)			(398.7)
Not Offered Grant in High Intensity Treatment Village	460.3	589.7	558.9	335.2	498.1	444.5	524.9	655.8	674.4*	734.1*	-588.8*	-593.6*	646.2*
	(356.1)	(408.9)	(378.5)	(363.0)	(361.8)	(358.5)	(378.4)	(438.6)	(393.4)	(404.7)	(340.8)	(339.4)	(384.4)
Village with Agricultural Households Targeted		-285.6						-274.7					
		(406.5)						(409.5)					
Household Received Incentive in 2013			-260.2	305.4			-181.3	-196.4					
			(277.9)	(330.5)			(311.2)	(310.9)					
Household Received Incentive in 2011							-355.7	-334.2					
							(338.2)	(341.0)					
Household Received Incentive in 2008							88.42	94.71					
							(362.8)	(364.3)					
Household Received any Incentive Over All Years (2008, 2011, 2013)					-266.3	53.91							
					(225.1)	(254.8)							
Household Located in Treatment Village in 2013									-214.1				-16.25
									(297.4)				(331.9)
Household Located in Treatment Village in 2011													-270.9
													(342.4)
Household Located in Treatment Village in 2008													-443.5
													(300.3)
Village Was a Treatment Village in 2008, 2011 or 2013										-674.2*			
										(347.7)			
Interaction: Low Intensity and Offered X Received Incentive in 2013				-2,377**									
				(1,059)									
Interaction: High Intensity and Offered X Received Incentive in 2013				-992.4*									
				(559.2)									
Interaction: Low Intensity and Offered X Received Incentive in any year						-2,123**							
						(1,046)							
Interaction: High Intensity and Offered X Received Incentive in any year						-742.5							
						(529.5)							
Interaction: Low Intensity Village X Treatment Village in 2013											-214.1		
											(297.4)		
Interaction: High Intensity Village X Treatment Village in 2013											1,049***		
											(383.7)		
Interaction: Low Intensity Village X Treatment Village in any year												74.45	
												(330.6)	
Interaction: High Intensity Village X Treatment Village in any year												1,328***	
												(423.6)	
Observations	2,293	2,293	2,293	2,293	2,293	2,293	2,293	2,293	2,293	2,293	2,293	2,293	2,293
Mean	2279.1	2279.1	2279.1	2279.1	2279.1	2279.1	2279.1	2279.1	2279.1	2279.1	2279.1	2279.1	2279.1

Errors clustered at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The results in this table were generated using household level data from the high frequency survey which interviewed households 6 times between 22nd December 2014 to 28th February 2015.
The dependent variable is income (in takas) generated by the household from participation in the away (i.e. migrant) labor market for the period covered by the high frequency survey.
All specifications include Upazila fixed effects. Dependent variables were winsorized at the 98% level

Appendix Table A6. Treatment Effects on Aman Rice Yields

VARIABLES	(1) Total Aman Yield (Kg)	(2) Aman Yield Per Decimal (Kg)	(3) Log of Total Aman Yield (Kg)	(4) Log of Aman Yield Per Decimal (Kg)
Low Intensity Treatment Village	134.0	0.0494	0.0265	0.0270
	(350.0)	(0.881)	(0.0915)	(0.0470)
High Intensity Treatment Village	153.4	-0.836	-0.0129	-0.0651
	(324.1)	(0.840)	(0.0933)	(0.0497)
Observations	611	611	561	561
R-squared	0.153	0.205	0.194	0.169
Control Mean	3298.926	14.22	7.953	2.653
Control Median	2800	14	7	2
Upazila FE	YES	YES	YES	YES

Errors clustered at the village level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The results in this table were generated using data from the 2016 Follow-Up Survey's Employer Survey, in addition to migration data from the Endline Survey.

The dependent variable in (1) is the total yield of Aman rice in kilograms for a given employer; (2) is the Aman rice yield divided by the area of land cultivated by the employer for Aman, measured in decimals; (3)-(4) are identical to (1)-(2), respectively, but are in logs.

All specifications include Upazila fixed effects. Dependent variables were winsorized at the 98% level (values below the 1st percentile were set to the 1st percentile value and values above the 99th percentile were set to the 99th percentile value)

Appendix Figure A1: Seasonality in Rangpur, where our sample villages are located

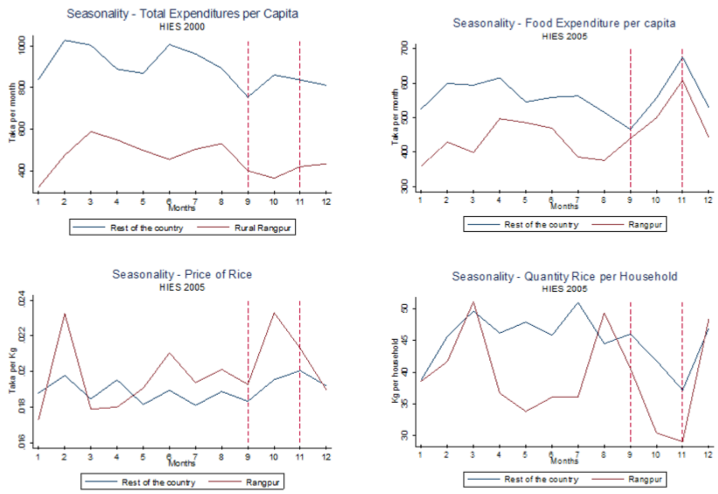
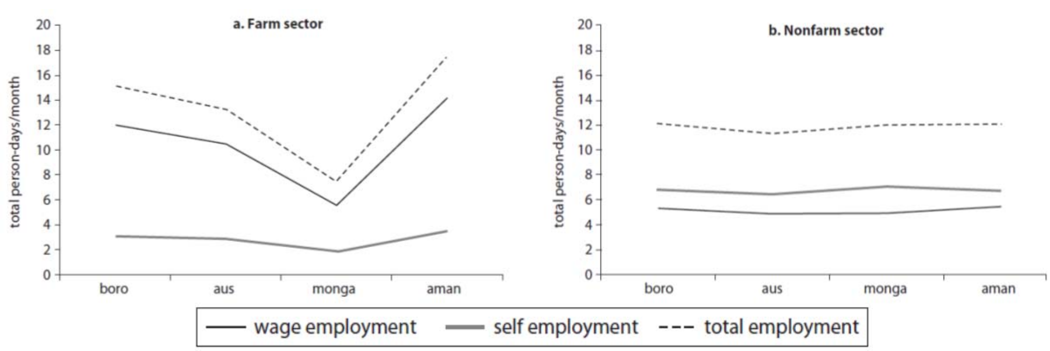


Figure reproduced from Bryan et al (2014). Households spend less money overall but spend more on food during the lean season in the last three months of the year. In addition, the figures illustrate that this increased expenditure is due to a rise in the price of rice (rather than a rise in quantity), and that quantity of rice consumed in fact falls.

Appendix Figure A2: Seasonality in the Farm Sector



Figures reproduced from Khandker and Mahmud (2012). The author use data from a large survey conducted by the Institute of Microfinance survey in 2008 to show that days of labor supplied (wage employment) fall in the farm-sector during the lean season called 'monga' (left panel), while they stay relatively constant throughout the year in the nonfarm- sector (right panel).