Seasonal Migration and Risk Aversion

Gharad Bryan, London School of Economics

Shyamal Chowdhury, University of Sydney

A. Mushfiq Mobarak, Yale University*

Abstract

Pre-harvest lean seasons are widespread in the agrarian areas of Asia and Sub-Saharan Africa. Every year, these seasonal famines force millions of people to succumb to poverty and hunger. We randomly assign an $8.50 incentive to households in Bangladesh to out-migrate during the lean season, and document a set of striking facts. The incentive induces 22% of households to send a seasonal migrant, consumption at the origin increases by 30% (550-700 calories per person per day) for the family members of induced migrants, and follow-up data show that treated households continue to re-migrate at a higher rate after the incentive is removed. The migration rate is 10 percentage points higher in treatment areas a year later, and three years later it is still 8 percentage points higher. These facts can be explained by a model with three key elements: (a) experimenting with the new activity is risky, given uncertain prospects at the destination, (b) overcoming the risk requires individual-specific learning (e.g. resolving the uncertainty about matching to an employer), and (c) some migrants are close to subsistence and the risk of failure is very costly. We test a model with these features by examining heterogeneity in take-up and re-migration, and by conducting a new experiment with a migration insurance treatment. We document several pieces of evidence consistent with the model.

Keywords: Migration, Risk Aversion

JEL Codes: O1, O15, J61, R23

December 16, 2011

*Corresponding Author: ahmed.mobarak@yale.edu, 203-432-5787. We are grateful to AusAID, the International Growth Centre and the U.S. Department of Labor for financial support. We thank, without implicating, Tim Besley, Abhijit Banerjee, Judy Chevalier, Chris Udry, Esther Duflo, Rohini Pande, Chris Woodruff, Ben Polak, John Gibson, Dean Yang, Michael Clemens, Francisco Rodriguez, Chung Wing Tse, Angelino Viceisza, conference participants at the 20th BREAD conference, 2010 ASSA conference, Federal Reserve Bank of Atlanta, 2011 NEUDC Conference, 2nd IGC Growth Week 2010, and seminar participants at Yale University, UC-Berkeley, University of Toulouse, Johns Hopkins University, Inter-American Development Bank, UC-Santa Barbara, World Bank, U.S. Department of Labor, IFPRI, Sacred Heart University and Brown University for comments. Alamgir Kabir, Daniel Tello, Talya Wyzanski, Tetyana Zelenska provided excellent research assistance.

1. Introduction

This paper studies the causes and consequences of internal seasonal migration in northwestern Bangladesh, a region where over 5 million people live below the poverty line, and must cope with a pre-harvest seasonal famine almost every year (The Daily Star, 2011). This seasonal famine – known locally as "monga" – is emblematic of the widespread pre-harvest lean or "hungry" seasons experienced throughout South Asia and Sub-Saharan Africa, in which households are forced into extreme poverty for part of the year.[1] Using a randomly assigned program to promote seasonal out-migration, we first document very large economic returns to migrating away in search of employment during the famine season. Next we explore why the people who were induced to migrate by our program were not already migrating, given the positive expected returns. This analysis helps us understand the role of risk aversion in preventing households from engaging in potentially profitable activities more broadly, especially when those households are close to subsistence and may have a lot to lose from experimenting with new ideas.

The proximate causes of the famine season are easily understood – work opportunities are scarce between planting and harvest in agrarian areas, and grain prices rise during this period (Khandker & Mahmud, forthcoming). Understanding how a famine can occur every year despite the existence of numerous potential mitigation strategies is, however, more challenging. We explore one obvious mitigation option – migration to nearby urban areas that offer better employment opportunities – and take a first step toward understanding whether this is a useful strategy and if so, why it is not employed more often. To do so, we randomly assigned an incentive (of $8.50 or 600 Taka, which covers the round-trip travel cost) to households conditional on seasonal migration.

[1] Seasonal poverty has been documented in Ethiopia (Dercon & Krishnan, 2000), who show that poverty and malnourishment increase 27% during the lean season, Malawi and Mozambique (Brune et al., 2011) – where people refer to a "hungry season", Madagascar (Dostie et al., 2002), who estimate that 1 million people fall into poverty before the rice harvest, Kenya (Swift, 1989), who distinguishes between years that people died versus years of less severe shortage, Senegal and Francophone Africa (the *soudure* phenomenon), Thailand (Paxson, 1993), India (Chaudhuri & Paxson, 2002) and inland China (Jalan & Ravallion, 1999).

The random assignment of incentives allows us to generate among the first experimental estimates of the effects of migration. Estimating the returns to migration is the subject of a very large literature, but one that has been hampered by difficult selection issues (Akee, 2010; Grogger & Hanson, 2011; Gibson et al., forthcoming).[2] We estimate large returns: migration induced by our intervention increases food and non-food expenditures of migrants' family members remaining at the origin by 30-35%, and improves their caloric intake by 550-700 calories per person per day. On an initial investment of about \$6-\$8 (the average round-trip cost to a destination), migrants earn \$110 on average during the lean season and save about half of that, suggestive of a very high rate of return on investment. Most strikingly, households in the treatment areas continue to migrate at a higher rate even after the incentive is removed. The migration rate is 10 percentage points higher in treatment areas a year later, and this figure drops only slightly to 8 percentage points 3 years later.

Our results add to an emerging literature that documents very high rates of return to small capital investments in developing countries (Udry & Anagol, 2006; de Mel et al., 2008; Bandiera et al., 2011; Duflo et al., 2011; Fafchamps et al., 2011). They also help to explain the persistent productivity gap between rural agriculture and urban non-agriculture sectors (D. Gollin et al., 2002; Caselli, 2005; Restuccia et al., 2008; Vollrath, 2009; McMillan & Rodrik, 2011) and the frictions that keep workers in agriculture despite the low relative productivity (Gollin, Lagakos and Waugh 2011). Finally, our results bolster the case made by Clemens et al (2008), Rosenzweig (2006), Gibson and McKenzie (2010), Clemens (2011), Rodrik (2007) and Hanson (2009) that offering migration opportunities has large effects on welfare, even relative to other promising development interventions in health, education, trade or agriculture. The prior literature largely focuses on

[2] Prior attempts use controls for observables (Adams, 1998), selection correction methods (Barham & Boucher, 1998; Acosta et al., 2007), matching methods (Gibson & McKenzie, 2010), instrumental variables methods (Brown & Leeves, 2007; McKenzie & Rapoport, 2007; Yang, 2008; Macours & Vakis, 2010; BenYishay, forthcoming) and natural policy experiments (Clemens, 2010; Gibson et al., forthcoming) to answer this question.

international migration, and we show that the returns to internal migration – a much more common, but under-studied phenomenon[3] – are also large.

The large consumption effects and preference for migration revealed by the re-migration beg one very important question: Why didn't our subjects already engage in such highly profitable behavior? This puzzle is not limited to our sample: according to nationally representative HIES 2005 data only 5 percent of households in Monga-prone districts receive domestic remittances, while 22 percent of all Bangladeshi households do. Remittances under-predict out-migration rates, but the size and direction of this gap is puzzling. The behavior also mirrors broader trends in international migration. The poorest Europeans from the poorest regions were the ones who chose not to migrate during a period in which 60 million Europeans left for the New World, even though their returns from doing so was likely the highest (Hatton & Williamson, 1998). Ardington et al (2009) provides similar evidence of constraints preventing profitable out-migration in rural South Africa.

The set of facts we document can be explained by a model in which experimenting with a new activity is risky, and rational households choose not migrate in the face of uncertainty about their prospects at the destination, even though they expect positive returns. Given a potential downside to migration (which we show exists in our data), households fear an unlikely but disastrous outcome, in which they pay the cost of moving but return hungry after not finding employment during a period when their family is already under the threat of famine. Inducing the inaugural migration by insuring against this devastating outcome (which our grant or loan with implied limited liability managed to do) can lead to long-run benefits where households either learn how well their skills fare at the destination, or improve future prospects by allowing employers to learn about them.

[3] 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 (Anh, 2003).

Three key elements of this model - (a) a risky technology, (b) the potential for individual-specific learning about the technology, and (c) that individuals are close to subsistence for whom the downside risk is disastrous – explain the high take-up rate for the intervention, the positive consumption effects, and the voluntary re-migration in a future period. The theory suggests that we can expect similar large ongoing impacts from small interventions in situations where these elements are present. This provides insight on a number of other important puzzles in growth and development. For example, green revolution technologies led to dramatic increases in agricultural productivity in South Asia (Evenson & Gollin, 2003), but adoption and diffusion of the new technologies was surprisingly slow, partly due to low levels of experimentation and the resultant slow learning (Munshi, 2004). Smallholder farmers reliant on the grain output for subsistence may not find it in their interest to experiment with a new technology with uncertain returns (given the farmer's own soil quality, rainfall and farming techniques), even if they believe the technology is very likely to be profitable.[4] Aversion to experimentation can also hinder entrepreneurship and business start-ups and growth (Hausmann & Rodrik, 2003; Fischer, 2009).

Our theory takes the view that the poor are not able to take advantage of a profitable opportunity because they are "vulnerable" and afraid of losses (Banerjee, 2004). This is closely related to the conceptualization of poverty in several other models (Kanbur, 1979; Kihlstrom & Laffont, 1979; Banerjee & Newman, 1991). The Monga setting therefore provides an opportunity to test the "poverty as vulnerability" theory, and we return to our data and conduct a new round of experiments to test five new implications that are drawn from our model.

First, households that are close to subsistence – on whom experimenting with a new activity imposes the biggest risk – should start with lower migration rates, but should be the most responsive

[4] The inability to experiment due to uninsured risk has been linked to biases towards low risk low-return technologies that stunt long-run growth (Yesuf et al., 2009), and to reduced investments in agricultural inputs and technologies such as new high-yield variety seeds and fertilizer (Rosenzweig & Wolpin, 1993; Dercon & Christiaensen, 2011).

to our intervention. Second, the incentive should have a larger effect on households that do not have network connections at the destination, because they have more to learn about the destination. Third, households should exhibit learning about migration opportunities and destinations in their subsequent choices on whether and where to re-migrate. Specifically, households with successful outcomes should be more likely to re-migrate, especially to the destinations where we originally induced them to migrate. Fourth, because fear of the disastrous negative outcome is the key aversion preventing migration, offering a limited liability loan to migrate should have a similar effect on migration rates as a conditional grant. Fifth, migration should be more responsive to incentives (e.g. credit *conditional* on migration) than to *un*conditional credit, because the latter also improves the returns to staying at home.[5] We find support for all of these predictions using our data and a new round of treatments.

Although we do conduct a new round of experiments to test two of the model's implications, many of these other results are identified through heterogeneity in treatment effects, and are therefore not experimental. There are legitimate omitted variables concerns with the risk-aversion interpretation we provide of the observation that households that are close to subsistence are more responsive to our incentive. That result could be driven by other characteristics correlated with low income, such as behavioral attributes that make certain households liquidity constrained on a regular basis (Banerjee & Mullainathan, 2010; Duflo et al., 2011). Our claim is not that there are no other possible explanations for our findings, but rather that, taken as a whole, the results are consistent with, and highly suggestive of, the model we propose. Furthermore, some of the other supporting evidence we present is based on experimental variation. For example, as part of our incentive scheme we required a subset of households to migrate to a specific destination. This treatment provides exogenous variation in destination choice and we show that being induced to

[5] One might think that this is a simple rationality requirement, but it is not implied by a model in which households fail to migrate because they are liquidity constrained.

migrate to a specific destination in the first round significantly predicts second round destination choice. We interpret this as evidence of location-specific learning or creation of location specific capital, which is a key assumption of our model.

Results of these tests notwithstanding, it is still somewhat puzzling that the households we induced were not experimenting with migration in years in which their income realization was high, or that they did not save up to experiment. Our neo-classical explanation may not be a fully satisfying explanation for this phenomenon (as argued by Duflo et al (2011) to explain low fertilizer use in Kenya). In the penultimate section of this paper we discuss some additional explanations drawing on the literature on behavioral economics. Our view is that our experiment demonstrates that the ingredients of subsistence, risk aversion and learning that we outline in our model are essential parts of any story - very few behavioral stories could explain our results if migration were not risky, households were far from subsistence and there was nothing to learn.

Our analysis provides a possible explanation for surprisingly low adoption rates of efficacious technologies with the potential to address important development challenges. Studies have shown that adoption rates for a range of technologies from tropical diseases treatment, agricultural productivity improvements, and savings products have remained low (Kremer et al., 2009; Dupas & Robinson, 2011; Meredith et al., 2011; G. Miller & Mobarak, 2011). If adoption is risky (e.g. due to risk of crop failure, or uncertainty about durability of an expensive new stove or water purifying technology) then giving households the opportunity to experiment with the new technology by insuring against failure may be an effective marketing strategy (Dupas, 2010). Our experiments are also related to the recent literature on the effects of unconditional and conditional cash transfers on developing country households (Gertler, 2004; Schultz, 2004; Rawlings & Rubio, 2005; Fiszbein & Schady, 2009; Paxson, 2010; Baird et al., forthcoming). Our findings suggest that providing credit to enable households to search for jobs, and to aid spatial and seasonal matching

between people and jobs, may be a useful way to augment the microcredit concept currently more narrowly focused on creating new entrepreneurs and new businesses.

Finally, from a narrow policy perspective, the program we implement appears to be a cost-effective response to the widespread famines that afflict the 5.3 million people residing in the Rangpur region of Bangladesh with disturbing regularity. Such predictable pre-harvest hungry seasons are also widespread in sub-Saharan Africa. The solution we implement is inexpensive; it confers long-run benefits even when offered as a once off; and is therefore more sustainable than subsidizing food purchases. Two important caveats are that our research does not capture long-term psychological and social effects of migration, and we do not study general equilibrium effects. Consideration of general equilibrium effects may not over-turn these findings however, since spillover benefits at the origin (which are found to be substantial in de Brauw and Giles (2008)'s research on migration from rural China) may exceed external costs at the destination. This is because emigrants form a much larger part of the village economy at the origin compared to the destination urban economy.

The next two sections describe the context and the design of our interventions. We present results on program take-up and the effects of migration in Section 4. These findings motivate the risky experimentation model in Section 5. We present statistical tests of various implications of the model in Section 6, discuss alternative explanations of the data in Section 7 and offer conclusions and policy advice in Section 8.

2. The Context: Northwestern Bangladesh and the Monga Famine

Our experiments were conducted in 100 villages in two districts (Kurigram and Lalmonirhat) in the seasonal-famine prone Rangpur region of north-western Bangladesh. The Rangpur region is home to roughly 7% of the country's population, or 9.6 million people. 57% of the region's

population (or 5.3 million people) live below the poverty line.[6] In addition to the level of poverty, the Rangpur region experiences more pronounced seasonality in income and consumption, with incomes decreasing by 50-60% and total household expenditures dropping by 10-25% during the post-planting and pre-harvest season (September-November) for the main Aman rice crop (Khandker & Mahmud, forthcoming). As Figure 1 indicates, the price of rice also spikes during this season, particularly in Rangpur, and thus actual rice consumption drops 22% even as households shift monetary expenditures towards food while waiting for the Aman rice harvest.

The lack of job opportunities and low wages during the pre-harvest season and the coincident increase in grain prices combines to create a situation of seasonal deprivation and famine (Sen, 1981; Khandker & Mahmud, forthcoming).[7] The famine occurs with disturbing regularity and thus has a name: Monga. It has been described as a routine crisis (Rahman, 1995), and its effects on hunger and starvation are widely chronicled in the local media. Agricultural wages in the Rangpur region are already among the lowest in the country over the entire year (BBS Monthly Statistical Bulletins), and further, demand for agricultural labor plunges between planting and harvest. The resultant drastic drop in purchasing power for Rangpur households reliant on agricultural wage employment threatens to take consumption below subsistence.

Several puzzling stylized facts about household and institutional characteristics and coping strategies motivate the design of our migration experiments. First, seasonal out-migration from the monga-prone districts appears to be low despite the absence of local non-farm employment opportunities. According to the nationally representative HIES 2005 data, it is more common for agricultural laborers from other regions of Bangladesh to migrate in search of higher wages and

---

[6] Extreme poverty rates (defined as individuals who cannot meet the 2100 calorie per day food intake even if they spend their entire incomes on food purchases only) were 25 percent nationwide, but 43 percent in the Rangpur districts. Poverty figures are based on Bangladesh Bureau of Statistics (BBS) Household Income and expenditure survey 2005 (HIES 2005), and population figures are based on projections from the 2001 Census data.
[7] Amartya Sen (1981)notes these price spikes and wage plunges as important causes of the 1974 famine in Bangladesh, and that the greater Rangpur districts were among the most severely affected by this famine.

employment opportunities, and this is known to be one primary mechanism by which households diversify income sources in India (Banerjee & Duflo, 2007).

Second, inter-regional variation in income and poverty between Rangpur and the rest of the Bangladesh have been shown to be much larger than the inter-seasonal variation within Rangpur (Khandker & Mahmud, forthcoming). This suggests smoothing strategies that take advantage of inter-regional arbitrage opportunities (i.e. migration) rather than inter-seasonal variation (e.g. savings, credit) may hold greater promise. Moreover, an in-depth case-study of the Monga phenomenon (Zug, 2006) explicitly notes that there are off-farm employment opportunities in rickshaw-pulling and construction in nearby urban areas during the monga season. To be sure, Zug (2006) points out that this is a risky proposition for many, as labor demand and wages drop all over rice-growing Bangladesh during that season. However, this seasonality is less pronounced than that observed in Rangpur (Khandker & Mahmud, forthcoming).

Finally, both government and large NGO monga-mitigation efforts have concentrated on direct subsidy programs like free or highly-subsidized grain distribution (e.g. "Vulnerable Group Feeding,"), or food-for-work and targeted microcredit programs. These programs are expensive, and the stringent micro-credit repayment schedule may itself keep households from engaging in profitable migration (Shonchoy, 2010). There are structural reasons associated with rice production seasonality for the seasonal unemployment in Rangpur, and thus encouraging seasonal migration towards where jobs are appears to be a sensible complementary policy to experiment with.

3. Design of Interventions and Experiment

The two districts where the project is conducted (Lalmonirhat and Kurigram) represent the agro-ecological zones that regularly witness the monga famine. We randomly selected 100 villages in these two districts and first conducted a village census in each location in June 2008. Next we

randomly selected 19 households in each village from the set of households that reported (a) that they owned less than 50 decimals of land, and (b) that a household member was forced to miss meals during the prior (2007) monga season.[8] We conducted a baseline survey of these 1900 households during the pre-monga season in July 2008. Our analysis will draw on additional rounds of follow-up surveys conducted in December 2008, May 2009, December 2009 and July 2011.

In August 2008 we randomly allocated the 100 villages into four groups: Cash, Credit, Information and Control. These treatments were subsequently implemented in collaboration with PKSF [9] through their partner NGOs with substantial field presence in the two districts. The partner NGOs were already implementing micro-credit programs in each of the 100 sample villages.

The NGOs implemented the interventions in late August 2008 for the Monga season starting in September. 16 of the 100 study villages (consisting of 304 sample households) were randomly assigned to form a control group. A further 16 villages (consisting of another 304 sample households) were placed in a job information only treatment. These households were given information on types of jobs available in four pre-selected destinations, the likelihood of getting such a job and approximate wages associated with each type of job and destination (see Appendix 1 for details). 703 households in 37 randomly selected villages were offered cash of 600 Taka (~US$8.50) at the origin conditional on migration, and an additional bonus of 200 Taka (~US$3) if the migrant reported to us at the destination during a specified time period. We also provided exactly the same information about jobs and wages to this group as in the information-only treatment. 600 Taka covers a little more than the average round-trip cost of safe travel from the two origin districts to the four nearby towns for which we provided job information. We monitored

[8] 71% of the census households owned less than 50 decimals of land, and 63% responded affirmatively to the question about missing meals. Overall, 56% satisfied both criteria, and our sample is therefore representative of the poorer 56% of the rural population in the two districts.

[9] PKSF (Palli Karma Sahayak Foundation) is an apex micro-credit funding and capacity building organizations in Bangladesh. It is a not-for-profit set up by the Government of Bangladesh in 1990
.

migration behavior carefully and strictly imposed the migration conditionality, so that the 600 Taka intervention was practically equivalent to providing a bus ticket.[10]

The 589 households in the final set of 31 villages were offered the same information and the same Tk 600 + Tk 200 incentive to migrate, but in the form of a zero-interest loan to be paid back at the end of the monga season. The loan was offered by our partner micro-credit NGOs that have a history of lending money in these villages. There is an implicit understanding of limited liability on these loans since we are lending to the extremely poor during a period of financial hardship. As discussed below, ultimately 80% of households were able to repay the loan.

Table 1 shows that there was pre-treatment balance across the randomly assigned groups in terms of the variables that we will use as outcomes in the analysis to follow. A Bonferroni multiple comparison correction for 27 independent tests requires a significance threshold of $\alpha=0.0019$ for each test to recover an overall significance level of $\alpha=0.05$. Using this criterion, no differences at baseline are statistically meaningful.

In the 68 villages where we provided monetary incentives for people to seasonally out-migrate (37 cash + 31 credit villages), we sometimes randomly assigned additional conditionalities to subsets of households within the village. A trial profile in Figure 2 provides details. Some households were required to migrate in groups, and some were required to migrate to a specific destination. We will not directly analyze the effects of such conditionalities in this paper, but these conditionalities created random within-village variation, which make it possible to study spillover and learning effects from one person to another using instrumental variables.

[10] The strict imposition of the migration conditionality implied that some households had to return the 600 Taka if they did not migrate after accepting the cash. We could not provide an actual bus ticket (rather than cash to buy it) for practical reasons: if that specific bus crashed, then that would have reflected poorly on the NGOs. Our data show that households found cheaper ways to travel to the destination: the average roundtrip travel cost was reported to be 450 Taka. The 150 Taka saving can cover about 5 days of food expenditure for one person at the origin.

4. Program Take-up and the Effects of Seasonal Migration

In this section we report results on take-up of the treatment, the effects of seasonal migration on household consumption at the origin, and on income and savings of the migrant at the destination, and the propensity to re-migrate in 2009 and in 2011 after incentives are removed.

4.1 Migration and Re-migration

Table 2 reports the take-up of the program across the four groups labeled cash, credit, information and control. We have 2008 migration data from two follow-up surveys, one conducted immediately after the monga ended (in December 2008), and another in May 2009. The second follow-up was helpful for cross-checking the first migration report[11], and for capturing the migration experiences of those who left and/or returned later. The two sets of reports were highly consistent with each other, and Table 1 shows the more complete migration rates obtained in May 2009.

In Table 2 we define a household as having a seasonal migrant if at least one household member migrated away in search of work between September 2008 and April 2009. This extended definition of the migration window accounts for the possibility that our incentive merely moved forward migration that would have taken place anyway. This window captures all migration during the Aman cropping season and, as a consequence, all the migration associated with Monga.

About a third (35.9%) of households in control villages sent a seasonal migrant. Providing households information about wages and job opportunities at the destination had no effect on the migration rate (the difference in point estimate is 0.0% and is quite tightly estimated). Either households already had the information that we made available to them, or the information we made available was not useful or credible. With the $8.50 (+$3) cash or credit treatments, the seasonal

[11] Since an incentive was involved, we verified migration reports closely using the substantial field presence of our partner NGOs, by cross-checking migration dates in the two surveys conducted six months apart, by cross-checking responses across households who reported migrating together in a group, and finally, by independently asking neighbors. The analysis (available on request) shows a high degree of accuracy in the cross reports and, importantly, that the accuracy of the cross reporting was not different in incentivized villages.

migration rate jumps to 59.0% and 56.8% respectively. In other words, incentives induced about 22% of the sample households to send a migrant.[12] The migration response to the cash and credit incentives are statistically significant relative to control or information, but there is no statistical difference between providing cash and providing credit.[13] Since households appear to react very similarly to either incentive, we combine the impact of these two treatments for expositional simplicity (and call it "incentive") for much of our analysis, and compare it against the combined information and control groups (labeled "non-incentive").

The lower panel of table 2 compares re-migration rates in subsequent years across the incentive and non-incentive groups. We conducted follow-up surveys in December 2009 and in July 2011 and asked about migration behavior in the preceding lean seasons, but we did not repeat any of the treatments in the villages used for the comparisons in the top half of table 2. Strikingly, the migration rate in 2009 was 10 percentage points higher in treatment villages, and this is after the incentives were removed. Regressions of the re-migration choice (discussed in detail in section 6) shows that if a particular household was induced to migrate in 2008, that roughly doubles the chance (a 45 percentage point effect) that it will send a migrant again in 2009. The July 2011 survey focused on migration during the other (lesser) lean season that coincides with the pre-harvest period for the second (lesser) rice harvest. Even two and a half years later, without any further program or incentive, the migration rate remains 8% higher in the villages randomly assigned to the cash or credit treatment in 2008.

We learn two important things from this re-migration behavior. First, the propensity to re-migrate absent further inducements serves as a revealed preference based indication that the net benefits from migration were positive for many, and/or that migrants developed some asset during

[12] The migrant is almost always male (97%), and often the household head (84% in treatment villages and 76% in control), who is the only migrant from that household (93%). Migrants make 1.73 trips on average during the season, which implies that migrants often travel multiple times within the season. The first trip lasts 42 (56) days for treatment (control) group migrants. They return home with remittance and to rest, and travel again for 40 (40) days or less on any subsequent trips.

[13] Our model will later provide an explanation for this fact.

the initial experience that makes future migration a positive expected return activity.[14] Second, the persistence of re-migration from 2009 to 2011 (without much further decay after the four potential migration seasons in between) suggests that households learnt something valuable or grew some real asset from the initial migration experience. This persistence makes it unlikely that some households simply got lucky one year, and then it took them several tries to determine (again) that they are actually better off not migrating. It also reduces the likelihood that our results are driven by a particularly good migration year in 2008.

78% of all 2011 migrants provided incentives in 2008 report going back to work for the same employer, which further bolsters our interpretation that the migrants who were induced gained something real and valuable: a connection to an employer. A likely source of uncertainty in the returns to migration thus appears to be the (potential) employer's incomplete information about the characteristics of specific migrants – are they reliable, honest, hard-working? This would make it difficult for migrants to "learn" from other villagers to resolve the uncertainty, and could explain the null effect of our information treatment. The fact that learning seems to be individual-specific also provides a possible explanation for the fact that some but not all village members migrate. If villagers could learn from the group we would expect to see complete learning overtime driven by the experiences of early adopters (Foster & Rosenzweig, 1995).

We also find that migrants in the incentive treatments (provided cash or credit in 2008) who continue working for the same employer in 2011 are significantly more likely to have formed a connection to that specific employer in 2008, when they were originally induced to go. Specifically, treatment group migrants are 20% more likely to report forming the job connection to their current

[14] While we will examine the effect of migration on specific economic outcomes measured in the survey, any one of those outcomes will necessarily be incomplete, since it is not possible to combine the social, psychological and economic effects of migration in one comprehensive welfare measure. The revealed re-migration preference is therefore a useful complement to other economic outcomes that we use in the analysis below.

(2011) employer in 2008 instead of 2007, relative to "regular" migrants in the control group.[15] This is again strongly suggestive that the migrants who were induced to migrate by our treatments formed an asset (a connection to an employer) at the destination, which continued to provide value three years later.

4.2 Effects of Migration on Consumption at the Origin

We now study the effects of migration on consumption expenditures amongst remaining household members during the monga season. Consumption is a broad and useful measure of the benefits of migration, aggregating as it does the impact of migrating on the whole family (Deaton, 1997), and takes into account the monetary costs of investing (although it neglects non-pecuniary costs). Consumption can be comparably measured for migrant and non-migrant families alike, and it helps overcome the problems associated with measuring the full costs and benefits of technology adoption that are highlighted in Foster and Rosenzweig (2010). Our consumption data are detailed and comprehensive: we collect expenditures on 318 different food (255) and non-food (63) items (mostly over a week recall, and some less-frequently-purchased items over bi-weekly or monthly recall), and aggregate up to create measures of food and non-food expenditures and caloric intake.

The effects on expenditures are calculated from a regression where the choice to migrate is instrumented with whether or not a household was randomly placed in the incentive group. In particular we estimate the equation:

$$Y_{ivj} = a + \beta\, Migrant_{ivj} + \theta\, X_{ivj} + \varphi_j + \upsilon_{ivj}$$

where Y_{ij} is per capita consumption expenditure for household *i* in village *v* in sub-district *j* in 2008; $Migrant_{ivj}$ is a binary variable equal to 1 if at least one member of household migrated during Monga

15 The estimating equation is: $Introduced_{it} = \alpha + \beta Incentivized_{i,2008} + \varepsilon_{it}$, where $Introduced_{it}$ is a binary variable equal to 1 if introduced to the employer in 2008 and equal to 0 if introduced in 2007; $Incentivized_{i,2008}$ is a binary variable equal to 1 if randomly assigned to receive cash or credit in 2008 and 0 otherwise. P-value for the "sharp" difference test (2008 introduction rather than 2007): 0.09. P-value for the difference test (2008 or 2009 introduction rather than 2007/2006): 0.06. P-value for the difference-in-differences test (2008 vs. 2007, treatment relative to control): 0.26.

in 2008 and 0 otherwise; X_{ivj} is a vector of baseline-level controls for household characteristics, including households income and proxy for assets, φ_j are fixed effects for sub-districts, and υ_{ij} is a mean-zero error term. *Migrant* is likely an endogenous variable, and we produce consistent estimates of β by instrumenting with the experimental treatments. The first stage equation is:

$$Migrant_{ivj} = \lambda + \varrho Z_v + \gamma X_{ivj} + \varphi_j + \varepsilon_{ivj}$$

where the set of instruments Z_v includes indicators for the random assignment at the village level into one of the treatment (cash or credit) or control groups. As is well known, estimates produced in this way show the effect of migration on those households that were induced to migrate by our intervention (that is the local average treatment effect or LATE). In our context this is the most policy relevant parameter: it is the consumption impact of migration on those that are induced to migrate by a policy that incentivizes migration. The LATE is also the average effect of migration on consumption for a well-defined subset of the population.

First stage results in table 3 verify that the random assignments to cash or credit treatments are powerful predictors of the decision to migrate. The second-stage estimates in table 4 show that migration of a household member during the monga season has substantial impact on the remaining household members' well-being. In all cases the left hand side variables are household level averages using the set of people reported to be living in the household at the time of the survey as the denominator. We discuss the appropriate choice of denominator in more detail below.

IV estimates using treatment assignment are always larger than OLS estimates. This likely reflects the fact that rich households at the upper end of our sample income distribution are not very likely to migrate (as we will show in section 6) in our analysis of heterogeneity of take-up). In the IV specification, per capita food, non-food expenditures, and caloric intake among induced migrant households increase by 30% to 35% relative to non-migrant households. We also observe

some changes towards higher quality diets as food consumption shifts towards protein, and more specifically towards meat and fish, which are more attractive, "tasty" sources of protein in the Bangladesh context. Among non-food items, we observe increases in child education expenditures among migrant households. There is also an increase in expenditures on clothing and shoes, but that is likely an effect of migrants bringing back gifts for their family members.

In terms of magnitude of effects, monthly consumption expenditures among migrant families increase by about $5 per person, or $20 per household due to induced migration. Our survey only asked about expenditures during the second month of monga, and the modal migrant in our sample had not yet returned from their current migration episode (which includes cases where they may have returned once, but left again). We therefore expect the effects to persist for at least another month, and the total expenditure increase therefore easily exceeds the amount of the treatment ($8.50). Furthermore, if households engage in consumption smoothing, then some benefits may persist even further in the future. In any case, the $8.50 is spent two months prior on transportation costs.

Since the act of migration increases both the independent variable of interest and possibly reduces the denominator of the dependent variable (household size at the time of interview), any measurement error in the date that migrants report returning can bias the coefficient on migration upwards. We address this problem directly by studying the effects of migration in 2008 on consumption in 2009 (where household size is computed using a totally different survey conducted over a year later). The last column of table 4 shows that 2009 effects are about 60-75% as large as the consumption effects in 2008, but still statistically significant. Migration is associated with a 26% increase in household expenditure which is still substantial. These long-run consumption gains are not necessarily from migrants consuming their 2008 earnings over a long period, but because many of those induced to migrate in 2008 were induced to re-migrate a year later.

Since the migration decision is serially correlated, measurement error in 2009 migration dates can also bias our estimates. We therefore conduct a number of other sensitivity checks on the consumption results by varying the definition of household size (the denominator). We conservatively assume that household members present in the house on the day of the interview were present for the entire prior month to consume the reported expenditures, since this variable is least likely to suffer from measurement error and coding problems. The consumption effects in this specification are about 75-80% as large (e.g. 550 calories per person rather than 713), and statistically significant. The effects are similar when we estimate household size based on an entirely different question in the survey ("who currently lives in the household" as opposed to "who is present on the interview date"). Finally, even if we assume that migrants never left and use household size constructed from baseline data, migration is estimated to increase consumption by 250 calories per person per day.

We can also consider the impact of migration in 2009 on consumption in 2009. Since 2008 treatments do not predict 2009 migration as strongly (first-stage F-stat <5), we have a weak instrument problem, and do not emphasize these results. Nevertheless, we find that migration in 2009 increases per-capita expenditures by a statistically significant 775 Taka in 2009 (compared to the 355 Taka effect observed in 2008). The larger effect is related to the fact that a select group of "successful" migrants from 2008 were the ones who chose to re-migrate in 2009 (as we show in section 6), and the effects among compliers in the 2009 IV regression are therefore larger.

4.3 Income and Savings at the Destination

Next we examine the data on migrants' earnings and savings at the destination to see whether the magnitude of consumption gains we observe at the origin are in line with the amount migrants earn, save and remit. Table 5 shows that migrants earn about $110 (7777 Taka) on average and save about half of that. The average savings plus remittance is about a dollar a day. Remitting

money is difficult and migrants carry money back in person, which is partly why we observe multiple migration episodes during the same lean season. Therefore, joint savings plus remittances is the best available indicator of money available for consumption at the origin. The destination data suggest that this amount is about $66 (4600 Taka).

The "regular" migrants in the control group earn more per episode, save and remit more per day relative to migrants in the treatment group. This is understandable, since the migrants we induce are new and relatively inexperienced in this activity. Even though the induced migrants had lower earning potential, they earned $105 on average and saved and remitted more than half of that, which suggests a very high rate of return on the $8.50 incentive.

Table 6 breaks down the number of migration episodes and average earnings by sector and by destination. Dhaka (the largest urban area) is the most popular migration destination, and a large fraction of migrants to Dhaka work in the transport sector (i.e. rickshaw pulling). Many others work for a daily wage, often as unskilled labor at construction sites. At or around other smaller towns that are nearer to Rangpur, many migrants work in agriculture, especially in potato-growing areas that follow a different seasonal crop cycle than in rice-growing Rangpur. Migrants earn the most in Dhaka and at other "non-agricultural destinations": about 5100 Taka or $71 per migration episode, which translates to $121 per household on average given multiple trips. Those working for daily wages in the non-agricultural sector (e.g. construction sites, brick kilns) earn the most.

It is difficult to infer the income these migrants *would have received* had they not migrated, since we do not have comparable measures of wages and earnings for non-migrants (who engage in a variety of agricultural, self-employment and entrepreneurial tasks at the origin). Observed migrant earnings at the destination (100 Taka per day on average) do compare favorably to the earnings of the sub-sample of non-migrants with salaried employment at the origin (65 Taka per day) and to the profits of small-business entrepreneurs at the origin (61 Taka per day). This comparison is on the

basis of a selected sample of migrants and non-migrants with employment, but it is informative about the source from which the extra consumption among migrant households is derived.

While all our data suggest that the extra consumption at the origin is primarily related to migrant earnings, savings and remittance, it is possible that some intensive margin effects (e.g. a switch towards protein or child expenditures) are realized because the husband is away, and there are intra-household gender differences in spending priorities (Thomas, 1994; Duflo, 2003; G. Miller, 2008). However, the intra-household mechanism is unlikely to explain the overall consumption gain (aggregating across food and non-food expenditures). Another possibility is that the overall caloric requirement increases because the migrant works harder, but that also does not directly explain the consumption gain among household members remaining at the origin.

4.4 Is Migration Risky?

Although migration was profitable on average, and a seemingly sensible strategy for most, there is heterogeneity in the returns to migration. Figure 3 shows that 16% of control group migrants and 27% of migrants from treatment areas earn less than the average earnings for a salaried position at the origin (65 Taka per day). This is admittedly a high bar, since many of the migrants we induced likely would not have been able to secure a steady salaried position at the origin. We asked migrants about their expectations and their actual earnings, and 11% report earning less than they expected. About 80% of households who took a loan to migrate were able to re-pay (there was implicit limited liability on a micro-credit NGOs loan given during a famine). While we cannot accurately measure risk in other (non-economic) dimensions associated with how unpleasant it is for the migrant to stay away from family, the revealed preference (re-migration) data indicates that about half the people induced to migrate the first year choose not to return.

We can examine heterogeneity in the effect of migration on consumption by running "intent to treat" quantile regressions that regresses total expenditures on the cash/credit treatment

assignment at different parts of the distribution. This regression shows that the 10^{th} percentile household in treatment villages experience only a statistically insignificant 14 Taka increase in expenditures per person, whereas the effect in the top half of the distribution is around 75-115 Taka. The quantile regressions suggest that migration is not valuable for about 10-20% of the population.

In addition to the uncertain returns from migration, the size of the initial investment is not trivial. The travel cost (and the inducement we provided) is equivalent to about 10 days of salaried employment at the origin. The inducement would cover about 25 days of food expenditure for one person during the famine season. Almost always the adult male who is the primary wage earner for the family migrates, and women typically do not work outside the home. In summary, the data suggest that households take a non-trivial risk to travel, and there is about a 10-20% risk of "failure".

5. A Model of Risky Experimentation

In this section we develop a simple model that is inspired by the three key facts we documented above: (1) A large number of households were motivated to migrate in response to the 600 Taka incentive, (2) There were positive returns to migration on average, indicating that households were not migrating despite a positive expected profit, and (3) A large portion of the households that were incentivized to migrate in year one continued to send a seasonal migrant in year two (and again in year 4) and continued to earn a high return to this activity.

Our model is specifically designed to capture the fact that the 600 Taka incentive had a large effect on the migration rate, but is not large in comparison to yearly income variation, nor total income. In particular, the average change in *weekly* consumption is 307 Taka between rounds 1 and 2 of our survey and 358 Taka between rounds 2 and 3. The standard deviations of these numbers are 635 and 508 Taka respectively. These figures suggest that variation in yearly income dwarfs the size of our 600 Taka payment, a point made forcefully by the observation that weekly expenditures

would need to drop just 11 Taka from a base of 860 Taka to save for the bus ticket. Given these observations the challenge for the theory is to explain why 600 Taka can have such an effect on peoples' choices.

We propose a simple model that is a stripped down version of the classic poverty trap models of Kihlstrom and Laffont (1979) and Banerjee and Newman (1991), and argue that it can explain the three main program evaluation findings. We then use the model to determine additional predictions that should hold in the data. In section 7 we discuss alternative theories that can also generate similar facts and argue that our interpretation is to be preferred on several dimensions.

Consider a household that lives for an infinite number of periods and in each period decides between staying at home and earning a certain income of y and sending a migrant and earning an income of $y + b$ with probability $\mu(b)$ and $y + g$ with probability μ (g). We assume that $g > b$ and we also assume that after one migration episode the household becomes informed about whether the return is g or b. The best way to interpret the return in this context is that successful migration (g) requires connections at the destination. Before the first migration episode the household is unsure whether it will be able to find a connection, but once the connection is in place it is permanent.

We assume that the household is a discounted expected utility maximizer with utility function u (that obeys all the usual assumptions), discount factor δ and that b and g are such that:

$$u(y + g) > u(y); \text{ and}$$

$$u(y + b) < u(y).$$

Given these assumptions, the return to migrating is

$$V^m = \frac{\mu(g)u(y+g)+\mu(b)\left((1\text{-}\delta)u(y+b)+\delta u(y)\right)}{1\text{-}\delta}$$

while the return to staying home is

$$V^h = \frac{u(y)}{1\text{-}\delta}.$$

Figure 4 shows a typical plot of V^m and V^h as a function of baseline income y. While the two curves need not cross, if they do it is usually true that they cross only once with V^m being optimal above some threshold income y^*.[16] It is much more likely that the curves will cross like this if the outcome b is sufficiently bad so that $u(y + b)$ is in the very steep part of the utility function. Figure 4 is drawn under specific parametric assumptions. In particular u is assumed to be the natural logarithm, and in that case the single crossing requires that $y + b$ be close to 0, the point at which the derivative of the log utility function becomes infinite and the utility of consumption infinitely negative. This is a reasonable interpretation of subsistence.

When the conditions for a single crossing are met, the model generates a poverty trap. Households with income $y < y^*$ never migrate; they consume their initial income y forever. Households that have income of $y \geq y^*$ migrate and learn their migration status. If they are able to find a connection they earn $y + g$ forever after and if they are not able to find a connection they return to consuming y. The long run distribution of income is therefore as depicted in Figure 5. Thus we have a very simple poverty trap, those with initial incomes below a certain level stagnate at that level while those with starting incomes above that point are able to take advantage of migration and push their long run income up by g.

It is well known that this sort of poverty trap requires two things to occur, first, there is a non-convexity in the production technology and second there is a missing market, in that we have shut down borrowing. We believe that both of these assumptions are reasonable in our setting. Migration is clearly a lumpy investment as there is a minimum cost of a bus ticket. Further, despite the influx of micro-credit in this region in recent years, most microcredit organizations require the borrower to spend the money on an entrepreneurial endeavor rather than migration, and

[16] The precise conditions for the single crossing can be found in (Banerjee, 2004). Roughly, if $y+b$ is low enough and u is unbounded below, V^m must lie below V^h in some range. Further, V^m will be above V^h in some range so long as the expected value of migrating is positive, and u exhibits Decreasing Absolute Risk Aversion (DARA) which implies that the impact of risk aversion becomes negligible at high incomes. Given these conditions DARA is sufficient to ensure that V^m is steeper than V^h and for most common parametric assumptions about the slope of the two functions converge to the same as income grows implying single crossing.

furthermore, require borrowers to remain at the origin to meet the obligations of a frequent repayment schedule (Shonchoy, 2010; Khandker & Mahmud, forthcoming). We also provide direct evidence that households would like access to credit to migrate by showing that households took up our credit offer, and migrated in response.

Of course, in reality incomes vary across years and those that are close in average income to the cutoff y^* will eventually jump from the low-income equilibrium to the high-income equilibrium. Nevertheless, poverty trap models such as this one provide a role for policy in helping those households whose income is sufficiently low that it is unlikely that they will move between the equilibria. Further, if the return g is large enough, a policy that helps households make the transition can lead to a modest transformation in people's lives.

5.1 Predictions on the Effects of Different Types of Interventions

We now analyze how external interventions can assist households in making the jump between equilibria. Figure 6 compares three policies. The first panel shows the baseline income threshold y^*, identified as the crossing point of the two curves. The second panel shows the new crossing point after a one-off *unconditional* cash transfer. The third panel shows the crossing point if households are given an *incentive* similar to our "cash" treatment (of the same size as the transfer in the second panel). The fourth panel shows the impact of an "insurance" program that provides a transfer (again of the same size) only if the household migrates and realizes state b. This fourth panel considers a policy that is similar to a limited liability loan, which is akin to our "credit" treatment.

The figure reveals two testable implications. First, because the poverty trap is driven almost entirely by the possibility of a near subsistence outcome in state b, the limited liability credit

(insurance) treatment realizes most of the benefits of the incentive policy.[17] The intuition is that if the poverty trap is driven by income being close to subsistence in the bad state, then removing the possibility of very low consumption removes the lion's share of the barrier to migration. Mathematically, the point of intersection of the curves is determined by the implicit equation

$$u(y+b+x) = [1-\mu(b)\delta]\, u(y) - \mu(g)\, u(y+g)$$

with an insurance of size x, and

$$u(y+b+x) = [1-\mu(b)\delta]\, u(y) - \mu(g)\, u(y+g+x)$$

with an incentive of size x. As discussed above, we require $u(y+b)$ to be small for the poverty trap to exist and as a consequence, adding x to the left hand side of these equations has a large effect. On the other hand, we do not require that $u(y+g)$ be small, in fact it must be relatively large to compensate for the possible downside of outcome b and ensure a positive expected return. As a consequence, we do not expect the right hand side of the second equation to be changed much by the addition of x. The implication then is that the required change in y^* should be similar for an insurance policy or a cash incentive. Empirically, we would predict that our "cash" and "loan" treatments will have a similar size impact on the migration rate.

Second, because the *un*-conditional transfer increases both V^m and V^h, it has a more limited impact on y^* and migration than an incentive which only increases V^m. This point is obvious, but is not true in a model in which liquidity constraints are the force that restricts migration. This test is, therefore, an important one because a pure liquidity constraint is probably the most likely alternative explanation for the three facts documented earlier. Under the parametric assumptions used to draw Figure 6, (log utility and specific assumptions about probabilities and investment returns as listed in the figure) an incentive has the same effect on encouraging migration as an unconditional transfer of

[17] The insurance policy is also much more cost effective; the incentive must be paid with certainty, while the insurance only pays out with probability $\mu(b)$. To translate this into our practical setting, the loan was repaid in roughly 80% of cases and the total cost of the loan treatment (excluding operating costs) is therefore only 20% of the cost of the incentive treatment.

twice the size.[18] Since year-to-year variation in income act like un-conditional transfers, this can explain why our migration incentive scheme induces some households to migrate even when they experience yearly fluctuations in income that exceed the $8.50 incentive.

5.2 Predictions on Treatment Heterogeneity: 'Vulnerability' and Subsistence

For what types of households do we expect the results above on transfers and incentives to hold? The situation considered in Figure 6 has a particular feature: household income y^* is very low. This implies that the transfer policy has a relatively large impact on V^b and therefore that the effect of the incentive is large relative to the effect of the transfer. This is what is required if we are to match the empirical fact that our incentive has a large effect, but the size of the incentive is small relative to yearly variation in income. If y^* is high relative to subsistence then it will no longer be the case that an incentive has a large effect relative to a transfer. Figure 7 illustrates. The left panel shows how a transfer will affect the curves V^m and V^b, while the right panel illustrates the impact of an incentive. Because income (before considering the effect of the bad outcome) in this example is relatively far from the subsistence point (as illustrated by the fact that V^b is relatively flat) the transfer has only a small impact on V^b and as a result the incentive and transfer policies have a similar effect. In such a context it seems unlikely that our incentive would have much of an impact as it is small relative to yearly variation in income and therefore most of the households that are far from subsistence will already have moved to the high income equilibrium. On the other hand, it is easy to see that if income is close to subsistence and V^b is accordingly steeper, then a transfer will have a much smaller impact on migration rates. This reasoning combined with the fact that the overall impact of our incentive on V^m will be largest when y^* is small (due to concavity of the utility function) leads us to predict that our treatments will have a larger effect on those households that are close to subsistence.

[18] This assumes that households are uniformly distributed with respect to income.

5.3 Predictions on Learning

Our model is able to generate such a stark poverty trap because we assume that there is learning about the state of the world after one migration episode. The program evaluation showed that about 50% of households who were induced to migrate in 2008 migrated again in 2009. It would be difficult to generate such a high level of re-migration in a model without learning. We also saw that the majority of households go back to work for the same employer in subsequent years, which suggests that the "learning" is individual-specific, and takes the form of a job connection.

The learning effect generates additional testable implications. First, if we interpret learning as finding out if you can establish a network connection and then keeping that connection, we should see that the incentive has the largest impact on those that do not already have network connections at a destination – this is the group of households that have most to learn. Second, learning should be apparent in the re-migration decision. We should observe households who had a favorable resolution of the uncertainty associated with migration to have higher re-migration propensity, especially in the treatment group. Third, we should see location-specific learning. That is, households that migrate to a particular location (for exogenous reasons) should continue to re-migrate to that location.

Some of the empirical tests implied by the learning assumptions carry an additional benefit in that they help distinguish our model's predictions from that of the most closely competing explanation. That alternative model is a simple liquidity constraint (i.e. no money for a bus ticket) preventing migration. In that model, re-migration without the $8.50 incentive in 2009 and 2011 would have to be related to savings from the 2008 migration that gets used for a bus ticket the next year. However, savings from prior period migration operates similarly to an unconditional transfer policy, and as our analysis showed, those savings would have to be very large to generate the same degree of re-migration. Furthermore, location-specific learning and learning from the experiences of

others are not necessarily implied by the simple liquidity constraint model, and one would have to add a learning component to that model to get those predictions.

6. Additional Tests of the Model

In summary, our model assumes that households in Rangpur know that there are potentially high returns from migration. They also know that there is a potential downside. Given how close some of them are to subsistence during the Monga season, a failed migration episode would be disastrous. In this situation the incentive we provided is enough to cover a bus ticket plus a little food, and therefore sufficient to rule out the disaster. This enables households to experiment and to discover whether they are able to make an ongoing profit from seasonal migration. If this simple description is correct, then the following additional predictions emerge:

Prediction 1: Cash and Credit treatments should have a similar impact on migration rates;

Prediction 2: The incentive should have a larger effect on households that are close to subsistence;

Prediction 3: The incentive should have a larger impact on households that do not have network connections at the destination;

Prediction 4: Households should exhibit learning in their re-migration choices, including location specific learning;

Prediction 5: Migration should be more responsive to a conditional incentive than an unconditional transfer; and

Prediction 6: Households should be very responsive to an explicit insurance program that covers losses from migration.

Predictions 1-4 can be tested with our data, while predictions 5 and 6 require new experiments.

6.1. Tests of Predictions 1-2: Treatment and Migrant Heterogeneity

The first prediction of our model has already been verified. The migration rate in the cash treatment is 59.0% while in the credit treatment it is 56.8% and the difference is not statistically significant. This is consistent with the claim that the cash and credit treatments should have similar effects on the migration rate, with the credit treatment being slightly less effective.

The second prediction of the model is that those who are close to subsistence should be less likely to migrate prior to the program, but these same households should be most responsive to the incentive. We use the portion of total household expenditure spent on food at baseline as a measure of how close the household is to subsistence. Panel A in Figure 8 shows kernel density plots of the distribution of this index among migrant (solid blue line) and non-migrant (dotted red line) households. Proximity to subsistence (food exp./total exp.) increases to the right along the x-axis.

The plot on the right in Panel A shows that in control villages, households close to subsistence are less likely to migrate. The plot of the left shows - using the same kernel density drawn for treatment villages - that once the $8.50 incentive is assigned, the discrepancy in the migrant/non-migrant distributions disappears. In other words, the treatment has the largest effect on households close to subsistence, and induces precisely those households to migrate. These two plots are therefore consistent with the pair of statements embodied in prediction 2.

In Panel B of Figure 8, we show the regression-based evidence on this same theoretical prediction. We run a regression of the form:

$$Migrant_{ivj} = a + \beta^1\, Incentive_v + \beta^2\, Subsistence_{ivj} + \beta^3\, Incentive_v \cdot Subsistence_{ivj} + \gamma X_{ivj} + \varphi_j + \varepsilon_{ivj}$$

where *subsistence* is a measure of the portion of expenditure spent on food as discussed above. When the migration decision is regressed on treatment, subsistence, and the interaction effect between the two, we see a significant negative coefficient on subsistence, and a significant positive coefficient of the interaction term treatment*subsistence. These are the two predictions from the theory.

In Panel C we show histograms of migration rates across different values of Food Exp./Total Exp. separately for treatment and control villages. We see that in control villages (light blue), migration rate increases as we move away from subsistence, but that the treatment manages to flatten out this graph. The treatment effects are largest in the part of the distribution where people are spending >95%, >75% or >50% of their expenditures on food alone.

Next we examine the distribution of expenditures post-treatment (in the December 2008 follow-up survey) to uncover the parts of the distribution that are affected most by the incentive. We plot the distribution of expenditures in the treatment villages, the distribution of expenditures in the control villages, and subtract the latter from the former. The result – plotted in Figure 9 – mirrors what we find with respect to subsistence and the propensity to migrate. We see that the treatment manages to decrease the population share of extremely poor households spending between 300 and 700 Taka ($5-$10) per person per month by about 11%, and increases the share of a less-poor category that spends 1200-1600 Taka ($17-23) per person per month by an (almost) equivalent 10%. The treatment therefore seems to have the largest effect on (the consumption outcomes) of people in the 3rd-25th percentile of expenditures. This is the same part of the distribution that was induced to migrate by the treatment.

While these findings are supportive of predictions 1 and 2, they are not conclusive because omitted variables may be correlated with both subsistence and the effect of the incentive.

6.2. Tests of Predictions 3-4: Learning

The third implication of our model is that the incentive should have a larger effect on those households that do not have connections at the destination and therefore have more to learn – or equivalently, households that potential employers need to learn more about. In other words, the treatment should induce households who are still uncertain whether their realization will be *g* or *b*.

Table 7 provides evidence on this conjecture. In this table we compare the characteristics of migrants in the control group (the "regular migrants" who migrate without any incentive) to migrants in the treatment group (a combination of "regular" migrants and people "induced" to migrate through the $8.50 treatment). We see that the incentive led to an increase in the number of households migrating that do not know someone at the destination and that do not already have a job lead at the destination. The table reports these statistics separately by the order of the migration episode within the first follow up survey. The first migration episode is presumably the one most affected by our incentive. During the first episode, the "induced" migrants are clearly much less likely to have network support or job connections at the destination. These differences between treatment and control group migrants persist until the third episode, and after that the induced migrants appear to catch up.

All these observations are again consistent with our model. The bottom panel also shows that treatment group migrants are less likely to travel alone. In summary, these results suggest that our treatment managed to induce households who were otherwise less comfortable migrating: people without access to substitute forms of insurance through job connections and social networks.

A fourth prediction of our model is that households should exhibit learning in their decisions on whether and where to re-migrate. Households that are successful in 2008 should be more likely to migrate in 2009, and this effect should be more pronounced among the incentivized migrants. Figure 10 provides evidence in favor of these conjectures. Panel A examines the 2009 re-migration decision of households as a function of the migration earnings/savings outcomes in 2008. Regardless of the measure of migration success used (total or per-day earnings or savings), re-migrants have a "success" distribution that is shifted to the right. The regression version of these

figures[19] (see table 8) shows that if a household is induced to migrate by the treatment, then it is 45 percentage points more likely to re-migrate the following year. The re-migration probability increases by a further 19 percentage points if the original experience is "successful" (i.e. results in above-median earnings).[20]

Panel B in Figure 10 shows that the learning effect is only present in treatment villages. Treated households that re-migrate are those that were successful in the first round. In contrast, the distribution of 2008 earnings is no different between 2009 re-migrants and non-remigrants in the control villages. This is consistent with the model and the idea that the treatment households have something to learn, while the non-treatment villages do not.

Finally, the theory also suggests that households should learn about destinations. In table 9 we study household destination choices among re-migrants in 2009. For each of the four major destinations, we estimate a separate model of the probability that the migrant returns to that same destination in 2009 as a function of whether he migrated to that destination in 2008 (in an OLS model), or whether he *was induced* to migrate to that destination in 2008 by our treatments (where the 2008 migration decision is instrumented with both the village-level cash/credit treatments as well as the individual level "destination restriction" treatment). The last 4 rows of table 9 repeat these same four models except that the dependent variables are destination choices in 2011 rather than 2009.

We see that the choice to return to Dhaka and Tangail (two of the four major destinations) is persistent: if the randomized treatment induced households to send a migrant to either of these cities in 2008, then those migrants are very highly likely to return to those same cities in 2009, and

---

19 The regression equation is $Migrant_{2009}=a+\varrho_1 Migrant_{2008}+\varrho_2 Migrant_{2008}\cdot Success_{2008}+\varphi_j+\varepsilon_{ivj}$. In this equation, $Migrant_{2008}$ is instrumented with the randomized village-level treatments, and *Migrant*Success* is instrumented with the interaction of those treatments with success. Later specifications in table 8 introduce the effects of friends' experience, which are instrumented with the household-specific randomized treatments such as the requirements to migrate with a group or to a specific destination (see figure 2).

20 The 45 percentage point "self-migration" effect dominates the effects of friends or relatives migrating in 2008, (which increase 2009 migration by only a statistically insignificant -5 to +8 percentage points). This again suggests that a person-specific outcome (such as a job connection to an employer) is the key source of uncertainty, rather than a general lack of information about opportunities at the destination. The within-village randomized intervention conditions (such as the destination and group restrictions – see Figure 2) allows us to study the effects of friends migrating using IV.

then again in 2011. Migrants induced to travel to Bogra or Munshigonj in 2008 were not as enamored with those options; in fact people induced to travel to Munshigonj become *less* likely to return in future years. This is important evidence of migrants acquiring destination-specific information, skill or capital after being induced to migrate by our treatment.

Table 10 aggregates across the four destinations and runs a simpler model of the decision to return to the same destination, and shows that households' learn from both their own experience and their friends'. Households are more likely to experiment with a destination where their friends experienced success in a prior year.

6.3 Tests of Predictions 5-6: Unconditional Credit and Insurance

To test predictions 5 and 6, we examine the migration responsiveness to new treatments applied in March 2011, just prior to the second (minor) lean season that corresponds to the pre-harvest period for a second rice harvest known as Boro. In this round of experiments, we repeated the credit incentive treatment (of 800 Taka) from 2008 in some households, but also added two new treatments: (1) *Un*-conditional credit of the same amount (800 Taka) not tied to any migration requirement, and (2) a migration insurance program that attempts to explicitly cover losses associated with migration.

The insurance program offers the same 800 Taka loan up front conditional on migration, but the loan repayment requirement is conditioned on measured rainfall conditions. Excessive rainfall is an important external event that adversely affects labor demand and work opportunities at the destination. Rain makes it more difficult to engage in unskilled wage work at outdoor construction sites (e.g. breaking bricks), it both increases the cost of pulling rickshaws and lowers the demand for a rickshaw, and if the excessive rain turns to floods it can even delay crop cultivation

and lower agricultural labor demand. Index insurance based on measured rainfall helps us circumvent moral hazard problems associated with insuring individual outcomes.

We purchased 10 years of daily rainfall data for Bogra (a popular migration destination) from the local meteorological department, imputed the probability distribution of rainy days during the pre-harvest migration period, and calculated the actuarially fair insurance premium and payoff amounts. Our contract then stipulated that if the rainfall in Bogra remains within "normal" range – 0-5 days during March-April - then the migrants would need to pay back Tk.950 (i.e. repay the loan with some interest). If it rained between 5 and 10 days, the borrower would have to repay Tk.714. If the number of rainy days exceeded 10, then the borrower would only need to repay Tk.640.[21]

Table 11 shows the basic take-up results for the 2011 interventions, where migration is regressed on the randomized treatments.[22, 23] The simple credit incentive (800 Taka conditional on migration) again led to a statistically significant 17.5 percentage point increase in the propensity to migrate relative to the control group. In contrast, the unconditional credit program led to only a 7.4 percentage point increase, and the effect is not different from zero. These results are consistent with prediction 5: that incentives should have stronger effects than un-conditional transfers. This result is important because it shows that households can remain in the "poverty trap" in our model despite having substantial variation in yearly income.

The insurance contract increases the migration rate by 15.7 percentage points and this is statistically the same as the response to the credit incentive. Furthermore, table 11 shows that this

---

[21] Note that the contract can be explained to borrowers like a standard credit contract, and the insurance feature is only introduced because the credit repayment is state contingent. This helps to avoid confusion about the concept of insurance (Gine & Yang, 2009).

[22] The estimating equation is of the following form: $Migrant2011_{ij} = \beta_0 + \theta Incentive2011_{iv} + \beta_1 ImpureControl_{iv} + \beta_2\lambda_j + \beta_3\varepsilon_{iv}$, where $Migrant2011_{ij}$ is a binary variable equal to 1 if at least one member of household i in locality j migrated in 2011 and 0 otherwise. $Incentive2011_{iv}$ is a vector of binary variables representing random assignment to receive conditional credit, rainfall insurance, or unconditional credit in 2010; omitted category is control and information on job leads in 2011. $ImpureControl_{iv}$ is a binary variable equal to 1 if randomly assigned to control group in 2011and incentivized in 2008 (omitted category is "pure control" which is equal to 1 if randomly assigned to control group or job leads in 2008 and in 2011); λ_j is sub-district fixed effects and ε_{iv} is a mean-zero error term.

[23] The 2011 interventions are offered in March to induce migration during the lesser lean period prior to the Boro harvest while the 2008 interventions were offered for migration in October (prior to the Aman harvest). The level effects are therefore not directly comparable across the two years, and we only compare across treatments within the same year.

average effect masks some significant (and sensible) variation. In particular, designing the insurance contract on the basis of rainfall measured in one particular destination (Bogra) leads to significant "basis risk" (risk of contractual non-performance) for farmers migrating to other destinations whose rainfall may only be weakly correlated to the rainfall in Bogra. To study heterogeneity in treatment response due to this basis risk, we add interaction terms between the insurance treatment and an indicator for households who had expressed interest in traveling to Bogra in our original baseline (July 2008). The last two columns of Table 12 show these effects for non-farmers (on whom extra rainfall poses the biggest risk) and for farmers separately.

The results indicate that households for whom the design of the insurance contract is most appropriate - i.e. non-farmers traveling to Bogra for whom basis risk is minimized – respond very strongly to the insurance program. The insurance contract increases the migration rate of non-farmers by 24.3 percentage points (for comparison, the analogous effect for the credit incentive was 6.0 percentage points). Furthermore, offering the 800 Taka insurance contract to non-farmers who reported back in 2008 that they planned to travel to Bogra leads to a statistically significant 52 percentage point increase in their migration rate.

In summary, this sub-section presents experiment-based evidence that Rangpur households are strongly responsive to an appropriately designed migration insurance program that addresses the downside risk to migration, exactly as predicted by our simple model. We view this evidence to be powerful because the received wisdom about insurance programs in developing countries is that people are generally reluctant to purchase insurance (Gine et al., 2008; Cole et al., 2010), and that adding an insurance component to a credit contract actually reduces adoption (Gine & Yang, 2009). Against this backdrop, we find that the migration response to explicit insurance is just as strong as the response to credit (with implicit limited liability) across the whole sample, and that once basis risk is minimized, the response to the explicit insurance contract is even stronger.

7. Alternative Explanations

A plausible alternative explanation for our empirical results is that households face a strong liquidity constraint – they simply do not have the 600 Taka required to pay for a bus ticket and therefore cannot migrate. Relieving the liquidity constraint one year could lead to ongoing migration either through learning as discussed above or because increased income relaxes the liquidity constraint again the following year.[24] Such an explanation can account for the majority of our facts.

Nevertheless, we do not think a liquidity constraint is the correct explanation for these results. We rule out this explanation because year on year variation in incomes is large relative to the incentive that we provided.[25] For the median households (with annual expenditures of 45000 Taka), our transfer was less than 1.5% of yearly expenditure. Alternatively, if a median household were to save an equal portion each week of the year it would mean reducing expenditures from around 860 to 849 Taka. This seems to be a small portion of income. That is, we would expect households amenable to migration to have received a sufficiently large income shock in the past to have overcome the liquidity constraint and moved to the rich equilibrium prior to our intervention. With a simple liquidity constraint story it is difficult to explain why there are large numbers of households that are not currently migrating, but are willing to be persuaded. This argument suggests that it is not the cash transfer aspect of our treatments that is important but rather the incentive aspect highlighted in our model (the fact transfers were conditional on migration). We provided direct evidence that this is the case when we compared a conditional and unconditional credit treatment. Consistent with the idea that liquidity constraints are not key, we showed that the conditional credit treatment has a much stronger impact on migration rates.

[24] It should be noted that some form of credit constraint is required for our poverty trap model to work. The liquidity constraint discussed here is a more severe form of credit constraint.

[25] Average absolute deviation in weekly expenditure across seasons in our sample is 307 Taka between rounds one and two and 368 Taka between rounds two and three. The standard deviation of absolute deviation in consumption is 635 and 508 Taka respectively. While part of this is no doubt driven by measurement error, these figures indicate that season-on-season variation in incomes is large relative to the 600 Taka of our incentive.

The discussion above applies equally to any mechanism that does not emphasize the incentive effect of our treatments. This observation, however, raises a perhaps troubling point for our preferred model. The basic argument against 600 Taka having a large effect in the form of a transfer is that yearly variation in income is larger. Therefore, for our incentive to work it must be the case that many households would prefer to keep a once off transfer of 600 Taka rather than spend it on migration. But this implies that the welfare impact of our intervention is bounded above by the benefit of giving each household 600 Taka to use as they please, which according to our model will lead most to remain in a "poverty trap". This point is of course obvious in a fully rational model because households always prefer to have money without constraints. We leave it to the reader to determine whether they believe that it is reasonable to assume this. Alternative models that posit less rationality, either in the form of loss aversion, excessive impatience (δ low), non-rational expectations ($\mu(b)$ too low) or some reason why the household is consistently on the poverty line despite variation in income [for example, hyperbolic discounting a´la (Laibson, 1997; Duflo et al., 2011)] can make some room for policy makers to believe that the welfare gains of this intervention are larger than the purely rational model would imply.

8. Conclusions

We conducted a randomized experiment in which we incentivized households in a famine-prone region of Bangladesh to send a seasonal migrant to an urban area. The main results show that a small incentive led to a large increase in the number of seasonal migrants, that the migration was successful (in terms of improving consumption by around 30%), and that households given the incentive in one year continued to be more likely to migrate in future years.

We argue that the results are consistent with a simple (rational) model of a poverty trap where households that are close to subsistence are unable to learn whether migration is successful

due to a small possibility that migrating will turn out badly, leaving households consumption below subsistence. The model helps us to understand the type of situation in which we would expect incentive and insurance policies to lead to the sorts of long-term effects observed in our experiment. We should look for situations in which there is household specific learning, where households are close to subsistence and where there is a reasonable chance of a negative outcome from investment. These predictions also provide an answer to the puzzle that motivated the entire project: why does Rangpur – the poorest region of the country - have a lower migration rate compared to the rest of Bangladesh? Our model suggests that this is because households in Rangpur are close to subsistence at the time when it makes most sense to migrate. This can also explain other peculiar migration patterns noticed in the literature – the lower out-migration rate among poorer Europeans (Hatton & Williamson, 1998) and poorer South-Africans (Ardington et al., 2009).

Banerjee and Duflo (2007) also pose this puzzle in describing the lives of the poor – "*Why Don't the Poor Migrate for Longer…given that they could easily earn much more by doing so?*", and conclude that "*The ultimate reason seems to be that making more money is not a … large enough priority to experience several months of living alone and often sleeping on the ground somewhere in or around the work premises*." The migration experience is almost certainly unpleasant for the household in ways not captured by our consumption and income measures, but the preferences revealed by the persistent voluntary re-migration of our sample households *after* incentives are removed suggest that some constraint or barrier also played a part in their decision not to migrate.[26] The *persistence* of voluntary re-migration is also informative in that it makes it unlikely that households were fooled by one unusually good year, and that observation rules out simpler explanations for our findings of the flavor that migration was actually not beneficial.

[26] Munshi and Rosenzweig (2009) argue that the lack of long-term permanent migration may reflect the value of remaining close to one's social network. However, the circular, seasonal migration we induce should not lead to a loss of social network ties.

Our intervention mitigates the spatial mismatch between where people live, and where jobs are during the pre-harvest months. This approach may be of relevance to other countries that face geographic concentrations of poverty, such as northern Nigeria, eastern islands of Indonesia, northeast India, southeast Mexico, and inland southwest China (Jalan & Ravallion, 1999). More generally, providing credit to enable households to search for jobs, and aid spatial and seasonal matching between employers and employees may be a useful way to augment the microcredit concept currently more narrowly focused on creating new entrepreneurs and new businesses.

Our results support a policy of providing micro-insurance that mitigates the potential downside of experimentation. Our analysis also suggests that such a policy will be particularly beneficial where households are close to subsistence, risk averse and could benefit from a technology that requires individual level experimentation in order to determine profitability.

For the specific case of the Monga famine in Bangladesh, the migration support programs we implement appear more cost-effective than subsidizing food purchases on an ongoing basis, which is the major anti-famine policy tool currently employed by the Bangladesh government (Government of Bangladesh 2005; Khandker & Mahmud, forthcoming). The scale of our experiment does not permit us to analyze potential adverse general equilibrium effects in destination labor markets when such a program is scaled up. There is mixed evidence in the literature on whether these effects are substantial (Borjas, 2003; Borjas & Katz, 2007; Card, 2009; Ottaviano & Peri, 2011). There is also evidence of large spillover benefits of emigration at the origin (Hatton & Williamson, 1993; O'Rourke, 1995; Mishra, 2007; de Brauw & Giles, 2008), and at the destination (Cortes, 2008).

References

Acosta, Pablo, Fajnzylber, Pablo, & Lopez, Humberto J. (2007). The Impact of Remittances on Poverty and Human Capital: Evidence from Latin American Household Surveys. In Ç. Özden, & M. W. Schiff (Eds.), *International Migration, Economic Development and Policy* (pp. 59-98). Washington, D.C.: World Bank.

Adams, Richard H. (1998). Remittances, Investment, and Rural Asset Accumulation in Pakistan. *Economic Development and Cultural Change*, 47(1): 155-173.

Akee, Randall. (2010). Who Leaves? Deciphering Immigrant Self-Selection from a Developing Country. *Economic Development and Cultural Change*, 58(2): 323-344.

Anh, Dang N. (2003). Migration and Poverty in Asia: With Reference to Bangladesh, China, the Philippines and Viet Nam. *Conference proceedings: Ad Hoc Expert Group Meeting on Migration and Development, Organised by the Economic and Social Commission for Asia and the Pacific.* Bangkok, 27–29 August:

Ardington, Cally, Case, Anne & Hosegood, Victoria. (2009). Labor Supply Responses to Large Social Transfers: Longitudinal Evidence from South Africa. *American Economic Journal: Applied Economics*, 1(1): 22-48.

Baird, Sarah, McIntosh, Craig & Özler, Berk. (forthcoming). Cash Or Condition? Evidence from a Cash Transfer Experiment. *Quarterly Journal of Economics*

Bandiera, Oriana, Burgess, Robin, Rasul, Imran, Gulesci, Selim & Sulaiman, Munshi. (2011). *can Entrepreneurship Programs Tranform the Economic Lives of the Poor.* Unpublished manuscript.

Banerjee, Abhijit. (2004). The Two Poverties. In S. Dercon (Ed.), *Insurance Against Poverty* () Oxford Scholarship Online Monographs.

Banerjee, Abhijit & Duflo, Esther. (2007). The Economic Lives of the Poor. *Journal of Economic Perspectives*, 21(1): 141-168.

Banerjee, Abhijit & Mullainathan, Sendhil. (2010). *The Shape of Temptation: Implications for the Economic Lives of the Poor.* Unpublished manuscript.

Banerjee, Abhijit & Newman, Andrew F. (1991). Risk-Bearing and the Theory of Income Distribution. *The Review of Economic Studies*, 58(2): pp. 211-235.

Barham, Bradford & Boucher, Stephen. (1998). Migration, Remittances, and Inequality: Estimating the Net Effects of Migration on Income Distribution. *Journal of Development Economics*, 55(2): 307-331.

BenYishay, Ariel. (forthcoming). *Informational Barriers to Credit for Migrants: Evidence from Guatemala.* Unpublished manuscript.

Borjas, George. (2003). The Labor Demand Curve is Downward Sloping: Reexamining the Impact of Immigration on the Labor Market. *The Quarterly Journal of Economics*: 1374.

Borjas, George, & Katz, Lawrence. (2007). The Evolution of the Mexican-Born Workforce in the United States. In G. Borjas (Ed.), *Mexican Immigration to the United States* (pp. 13-55). Chicago, IL: University of Chicago Press.

Brown, Richard & Leeves, Gareth. (2007). *Impacts of International Migration and Remittances on Source Country Household Incomes in Small Island States: Fiji and Tonga.* No. Discussion Paper No. 347. Australia: School of Economics, The University of Queensland.

Brune, Lasse, Gine, Xavier, Goldberg, Jessica & Yang, Dean. (2011). *Commitments to Save : A Field Experiment in Rural Malawi.* No. Policy Research Working Paper No. 5748. Washington, D.C.: World Bank.

Card, David. (2009). Immigration and Inequality. *American Economic Review*, 99(2): 1-21.

Caselli, Francesco. (2005). Accounting for Cross-Country Income Differences. In P. Aghion, & S. Durlauf (Eds.), *Handbook of Economic Growth* ()

Chaudhuri, Shubham & Paxson, Christina. (2002). *Smoothing Consumption Under Income Seasonality: Buffer Stocks Vs. Credit Markets* Columbia University, Department of Economics, Discussion Papers: 0102-54.

Clemens, Michael. (2010). *The Roots of Global Wage Gaps: Evidence from Randomized Processing of U.S. Visas.* No. Working Paper 212. Washington, D.C.: Center for Global Development.

Clemens, Michael. (2011). Economics and Emigration: Trillion-Dollar Bills on the Sidewalk? *The Journal of Economic Perspectives*, 25(3): 83-106.

Clemens, Michael, Montenegro, Claudio E. & Pritchett, Lant. (2008). *The Place Premium: Wage Differences for Identical Workers Across the U.S. Border.* No. Working Paper No. 148. Washington, D.C.: Center for Global Development.

Cole, Shawn, Gine, Xavier, Tobacman, Jeremy, Topalova, Petia, Townsend, Robert & Vickery, James. (2010). *Barriers to Household Risk Management: Evidence from India.* No. 09-116. Harvard Business School.

Cortes, P. (2008). The Effect of Low-Skilled Immigration on U.S. Prices: Evidence from CPI Data. *Journal of Political Economy*, 116(3): 381-422.

de Brauw, Alan & Giles, John. (2008). *Migrant Labor Markets and the Welfare of Rural Households in the Developing World : Evidence from China.* Unpublished manuscript.

de Mel, Suresh, McKenzie, David & Woodruff, Christopher. (2008). Returns to Capital: Results from a Randomized Experiment. *Quarterly Journal of Economics*, 123(4): 1329-1372.

Deaton, Angus. (1997). *The Analysis of Household Surveys* (3rd ed.). Baltimore: Johns Hopkins University Press.

Dercon, Stefan & Christiaensen, Luc. (2011). Consumption Risk, Technology Adoption and Poverty Traps: Evidence from Ethiopia. *Journal of Development Economics*, 96(2): 159-173.

Dercon, Stefan & Krishnan, Pramila. (2000). Vulnerability, Seasonality and Poverty in Ethiopia. *Journal of Development Studies*, 36(6): 25-53.

Dostie, B., Haggblade, S. & Randriamamonjy, J. (2002). Seasonal Poverty in Madagascar: Magnitude and Solutions. *Food Policy*, 27(5/6): 493.

Duflo, Esther. (2003). Grandmothers and Granddaughters: Old-Age Pensions and Intrahousehold Allocation in South Africa. *The World Bank Economic Review*, 17(1): 1-25.

Duflo, Esther, Kremer, Michael & Robinson, Jonathan. (2011). Nudging Farmers to use Fertilizer: Theory and Experimental Evidence from Kenya. *The American Economic Review*, 101(6): 2350-2390.

Dupas, Pascaline. (2010). *Short-Run Subsidies and Long-Run Adoption of New Health Products: Evidence from a Field Experiment.* No. 16298. National Bureau of Economic Research.

Dupas, Pascaline & Robinson, Jonathan. (2011). *Why Don't the Poor Save More? Evidence from Health Savings Experiments.* Unpublished manuscript.

Evenson, Robert E. & Gollin, Douglas. (2003). Assessing the Impact of the Green Revolution, 1960 to 2000. *Science*, 300: 758-762.

Fafchamps, Marcel, McKenzie, David, Quinn, Simon R. & Woodruff, Christopher. (2011). *When is Capital enough to Get Female Microenterprises Growing? Evidence from a Randomized Experiment in Ghana.* No. Working Paper 17207. National Bureau of Economic Research.

Fischer, Greg. (2009). *Contract Structure, Risk Sharing and Investment Choice.* (Unpublished Department of Economics, London School of Economics,

Fiszbein, A., & Schady, N. R. (Eds.). (2009). *Conditional cash transfers: Reducing present and future poverty.* Washington, D.C., USA: World Bank Publications.

Foster, Andrew & Rosenzweig, Mark R. (1995). Learning by Doing and Learning from Others: Human Capital and Technical Change in Agriculture. *Journal of Political Economy*, 103(6): 1176-1209.

Foster, Andrew & Rosenzweig, Mark R. (2010). Microeconomics of Technology Adoption. *Annual Review of Economics*, 2(1): 395-424.

Gertler, Paul. (2004). Do Conditional Cash Transfers Improve Child Health? Evidence from PROGRESA's Control Randomized Experiment. *The American Economic Review*, 94(2): 336-341.

Gibson, John & McKenzie, David. (2010). *The Development Impact of a Best Practice Seasonal Worker Policy*. No. BREAD Working Paper No. 286.

Gibson, John, McKenzie, David & Stillman, Steven. (forthcoming). Accounting for Selectivity and Duration-Dependent Heterogeneity when Estimating the Impact of Emigration on Incomes and Poverty in Sending Areas. *Economic Development and Cultural Change*

Gine, Xavier, Townsend, Robert & Vickery, James. (2008). Patterns of Rainfall Insurance Participation in Rural India. *The World Bank Economic Review*, 22(3): 539-566.

Gine, Xavier & Yang, Dean. (2009). Insurance, Credit, and Technology Adoption: Field Experimental Evidence from Malawi. *Journal of Development Economics*, 89(1): 1-11.

Gollin, Douglas, Lagakos, D. & Waugh, M. (2011). *the Agricultural Productivity Gap in Developing Countries*. Unpublished manuscript.

Gollin, Douglas, Parente, S. & Rogerson, R. (2002). The Role of Agriculture in Development. *American Economic Review*, 92(2)

Government of People's Republic of Bangladesh. (2005). *Bangladesh: Poverty Reduction Strategy Paper: Unlocking the Potential, National Strategy for Accelerated Poverty Reduction*. Washington, D.C.: International Monetary Fund.

Grogger, Jeffrey & Hanson, Gordon H. (2011). Income Maximization and the Selection and Sorting of International Migrants. *Journal of Development Economics*, 95(1): 42-57.

Hanson, Gordon H. (2009). The Economic Consequences of the International Migration of Labor. *Annual Review of Economics*, 1: 179-207.

Hatton, Timothy & Williamson, Jeffrey. (1993). After the Famine: Emigration from Ireland, 1850-1913. *Journal of Economic History*, 53(3): 575-600.

Hatton, Timothy & Williamson, Jeffrey. (1998). *the Age of Mass Migration: An Economic Analysis* New York: Oxford University Press.

Hausmann, Ricardo & Rodrik, Dani. (2003). Economic Development as Self-Discovery. *Journal of Development Economics*, 72(2): 603-633.

Jalan, Jyotsna & Ravallion, Martin. (1999). Are the Poor Less Well Insured? Evidence on Vulnerability to Income Risk in Rural China. *Journal of Development Economics*, 58

Kanbur, Ravi. (1979). Of Risk Taking and the Personal Distribution of Income. *Journal of Political Economy*: 769-797.

Khandker, Shahidur R. & Mahmud, W. (forthcoming). *Seasonal Hunger and Public Policies: Evidence from Northwest Bangladesh*. Washington, D.C.: World Bank.

Kihlstrom, Richard & Laffont, Jean-Jacques. (1979). A General Equilibrium Entrepreneurial Theory of Firm Formation Based on Risk Aversion. *Journal of Political Economy*, 87(4): 719-748.

Kremer, Michael, Miguel, Edward, Mullainathan, Sendhil, Null, Clair & Zwane, Alix P. (2009). *Making Water Safe: Price, Persuasion, Peers, Promoters, Or Product Design?* Unpublished manuscript.

Laibson, David. (1997). Golden Eggs and Hyperbolic Discounting. *Quarterly Journal of Economics*, 113(2): 443-477.

Macours, Karen & Vakis, Renos. (2010). Seasonal Migration and Early Childhood Development. *World Development*, 38(6): 857-869.

McKenzie, David & Rapoport, Hillel. (2007). Network Effects and the Dynamics of Migration and Inequality: Theory and Evidence from Mexico. *Journal of Development Economics*, 84(1): 1-24.

McMillan, M. & Rodrik, Dani. (2011). *Globalization, Structural Change and Productivity Growth.* No. 17143. Cambridge, MA: NBER Working Paper.

Meredith, Jennifer, Robinson, Jonathan, Walker, Sarah & Wydick, Bruce. (2011). *Keeping the Doctor Away: Experimental Evidence on Investment in Preventative Health Products.* Unpublished manuscript.

Miller, Grant & Mobarak, A. M. (2011). *Intra-Household Externalities and Low Demand for a New Technology:Experimental Evidence on Improved Cookstoves.* Unpublished manuscript.

Miller, Grant. (2008). Women's Suffrage, Political Responsiveness, and Child Survival in American History. *The Quarterly Journal of Economics*, 123(3): 1287-1327.

Mishra, Prachi. (2007). Emigration and Wages in Source Countries: Evidence from Mexico. *Journal of Development Economics*, 82(1): 180-99.

Munshi, Kaivan. (2004). Social Learning in a Heterogeneous Population: Technology Diffusion in the Indian Green Revolution. *Journal of Development Economics*, 73(1): 185-213.

Munshi, Kaivan & Rosenzweig, Mark R. (2009). *Why is Mobility in India so Low? Social Insurance, Inequality and Growth.* No. NBER Working Paper No. 14850. Washington, D.C.: National Bureau of Economic Research.

O'Rourke, . (1995). Emigration and Living Standards in Ireland since the Famine. *Journal of Population Economics*, 8(4): 407-421.

Ottaviano, Gianmarco I. P. & Peri, Giovanni. (2011). Rethinking the Effects of Immigration on Wages. *Journal of the European Economic Association, Forthcoming*

Paxson, Christina. (1993). Consumption and Income Seasonality in Thailand. *Journal of Political Economy*, 101(1): 39-72.

Paxson, Christina. (2010). Does Money Matter? the Effects of Cash Transfers on Child Development in Rural Ecuador. *Economic Development and Cultural Change*, 59(1): 187.

Ping, Huang (2003). China Migration Country Study. *Conference proceedings: Regional Conference on Migration, Development and Pro-Poor Policy Choices in Asia.* Dhaka, Bangladesh: 22-24 June, 2003.

Rahman, Hossain Z. (1995). Mora Kartik: Seasonal Deficits and the Vulnerability of the Rural Poor. In H. Z. Rahman, & M. Hossain (Eds.), *Rethinking Rural Poverty: Bangladesh as a Case Study* (). New Dehli, India: Sage Publications.

Rawlings, Laura B. & Rubio, Gloria M. (2005). Evaluating the Impact of Conditional Cash Transfer Programs. *The World Bank Research Observer*, 20(1): 29-55.

Restuccia, D., Yang, D. T. & Zhu, X. (2008). Agriculture and Aggregate Productivity: A Quantitative Cross-Country Analysis. *Journal of Monetary Economics*, 55: 234-250.

Rodrik, D. (2007, June 1). Be our Guest. *New York Times,*

Rosenzweig, Mark R. (2006). Global Wage Differences and International Student Flows. In S. M. Collins, & C. Graham (Eds.), *Brookings Trade Forum: Global Labor Markets?* (pp. 211-228). Washington, D.C.: Brookings Institution Press.

Rosenzweig, Mark R. & Wolpin, Kenneth I. (1993). Credit Market Constraints, Consumption Smoothing, and the Accumulation of Durable Production Assets in Low-Income Countries: Investments in Bullocks in India. *Journal of Political Economy*, 101(2): 223-244.

Schultz, T. P. (2004). School Subsidies for the Poor: Evaluating the Mexican Progresa Poverty Program. *Journal of Development Economics*, 74(1): 199-250.

Sen, Amartya. (1981). *Poverty and Famines: An Essay on Entitlement and Deprivation.* Oxford, USA: Clarendon Press.

Shonchoy, Abu. (2010). *Seasonal Migration and the Effectiveness of Micro-Credit in the Lean Period: Evidence from Bangladesh.* No. Institute of Developing Economies Discussion paper No. 294.

Staff Correspondent. (2011, March 31). Monga Initiatives Insufficient. *The Daily Star,*

Swift, Jeremy. (1989). Why are Rural People Vulnerable to Famine? *IDS Bulletin*, 20(2): 8-15.

Thomas, Duncan. (1994). Like Father, Like Son; Like Mother, Like Daughter: Parental Resources and Child Height. *Journal of Human Resources*, 29(4): 950-988.

Udry, Christopher & Anagol, Santosh. (2006). The Return to Capital in Ghana. *The American Economic Review*, 96(2): 388-393.

Vollrath, D. (2009). How Important are Dual Economy Effects for Aggregate Productivity? *Journal of Development Economics*, 88(2): 325-334.

Yang, Dean. (2008). International Migration, Remittances and Household Investment: Evidence from Philippine Migrants' Exchange Rate Shocks. *The Economic Journal*, 118(528): 591-630.

Yesuf, Mahmud, Kassie, Menale & Köhlin, Gunnar. (2009). *Risk Implications of Farm Technology Adoption in the Ethiopian Highlands.* No. EfD Discussion Paper 09-13. Washington, D.C.: Environment for Development Intiative and Resources for the Future.

Zug, Sebastian. (2006). Monga—Seasonal Food Insecurity in Bangladesh—Bringing the Information Together. *Journal of Social Studies*, 111

Appendix 1: Description of 2008 Treatments

Out of the 100 villages selected to participate in the study, 16 (304 households) were assigned to the control group, while the remaining 84 villages (1596 households) were assigned to one of three treatments:

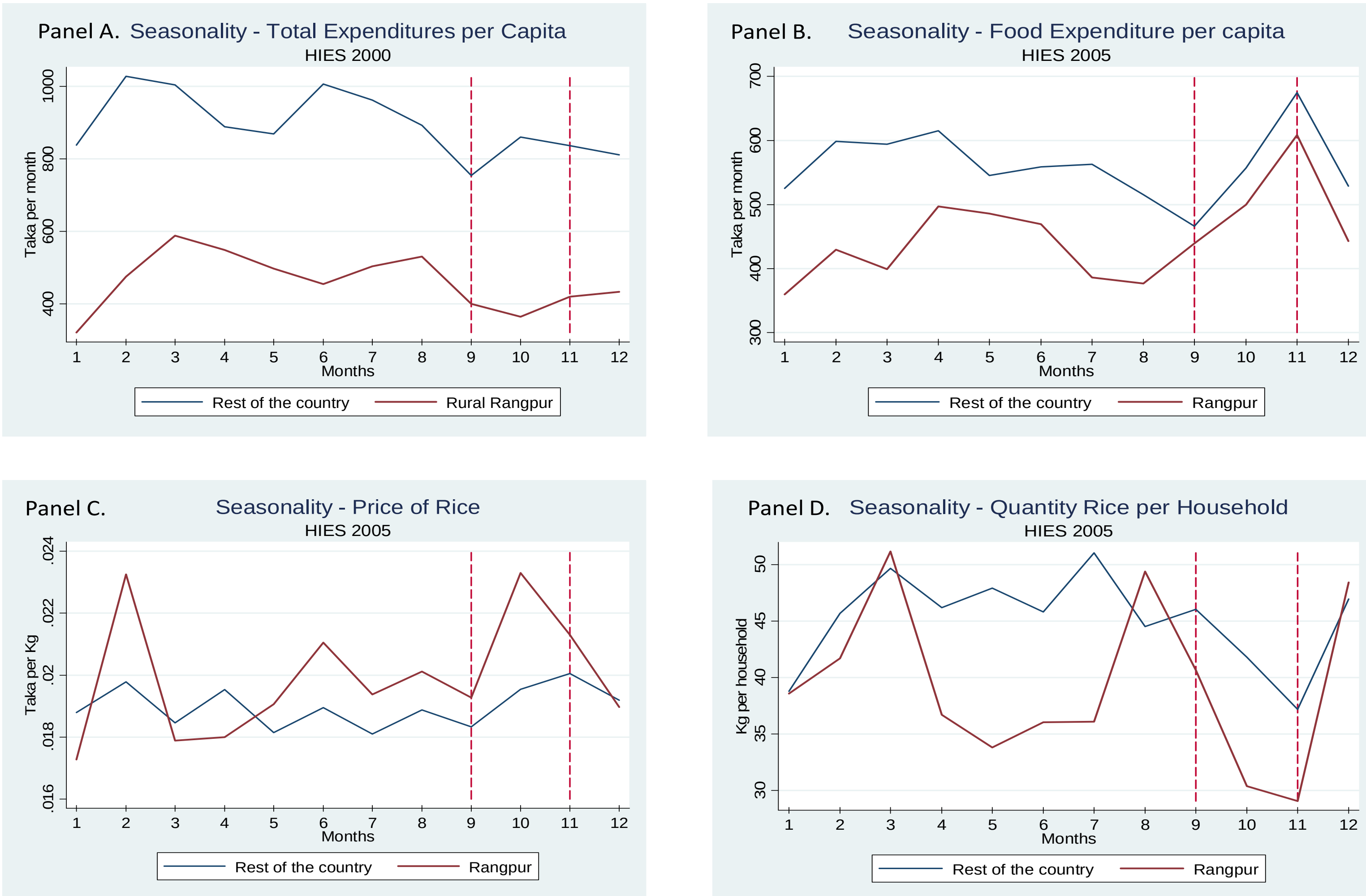
Information (16 villages/304 households): Potential migrants were provided with information on the types of jobs available in each of four areas: Bogra, Dhaka, Munshigonj and Tangail. In addition, they were told the likelihood of finding such a job, and the average daily wage in each job. This information was provided using the following script:

> "We would like to give you information on job availability, types of jobs available and approximate wages in four regions – Bogra, Dhaka, Munshigonj and Tangail. They are not in any particular order. NGOs working in those areas collected this information at the beginning of this month.
>
> Three most commonly available jobs in Bogra are: a) rickshaw pulling, b) construction work, c) agricultural labor. The average wage rates per day are Tk. 150 to 200 for rickshaw pulling, Tk.120 to 150 for construction work, and Tk. 80 to 100 for agricultural laborer. The likelihood of getting such a job in Bogra is medium (not high/not low).
>
> Three most commonly available jobs in Dhaka are: a) rickshaw pulling, b) construction work, c) day labor. The average wage rates per day are Tk. 250 to 300 for rickshaw pulling, Tk.200 to 250 for construction work, and Tk. 150 to 200 for day laborer. The likelihood of getting such a job in Dhaka is high.
>
> Three most commonly available jobs in Munshigonj are: a) rickshaw pulling, b) land preparation for potato cultivation, c) agricultural laborer. The average wage rates per day are Tk. 150 to 200 for rickshaw pulling, Tk.150 to 160 for land preparation, and Tk. 150 to 160 for agricultural laborer. The likelihood of getting such a job in Munshigonj is high.
>
> Three most commonly available jobs in Tangail are: a) rickshaw pulling, b) construction work, c) day laborer in brick fields. The average wage rates per day are Tk. 200 to 250 for rickshaw pulling, Tk.160 to 180 for construction work, and Tk. 150 to 200 for brick field work. The likelihood of getting such a job in Tangail is medium (not high/not low).
>
> Based on the above information, would you/any member of your family like to any of the above location during this monga season? If so, where do you want to go? Note that the job market information given above might have changed or may change in the near future and there is no guarantee that you will find a job, and we're just providing you the best information available to us. Note also that we or the NGOs that collected this information will not provide you with any assistance in finding jobs in the destination."

Cash (37 villages/703 households): Households were read the same script on job availability as given above, and were also offered a cash grant of Taka 600 conditional on migration. This money was provided at the origin prior to migration, and was framed as defraying the travel cost (money for a bus ticket). Migrants had an opportunity to receive Taka 200 more if they reported to us at the destination.

Credit (31 villages/589 households): Households were read the same script on job availability as given above, and were also offered a zero interest loan of Taka 600 conditional on migration. This money was provided at the origin prior to migration, and was framed as defraying the travel cost (money for a bus ticket). Migrants had an opportunity to receive Taka 200 more if they reported to us at the destination. Households were told that they would have to pay back the loan at the end of the Monga season.

Figure 1. Seasonality in Consumption and Price in Rangpur and in Other Regions of Bangladesh



Source: Bangladesh Bureau of Statistics 2005 Household Income and Expenditure Survey

Figure 2. Trial Profile and Timeline

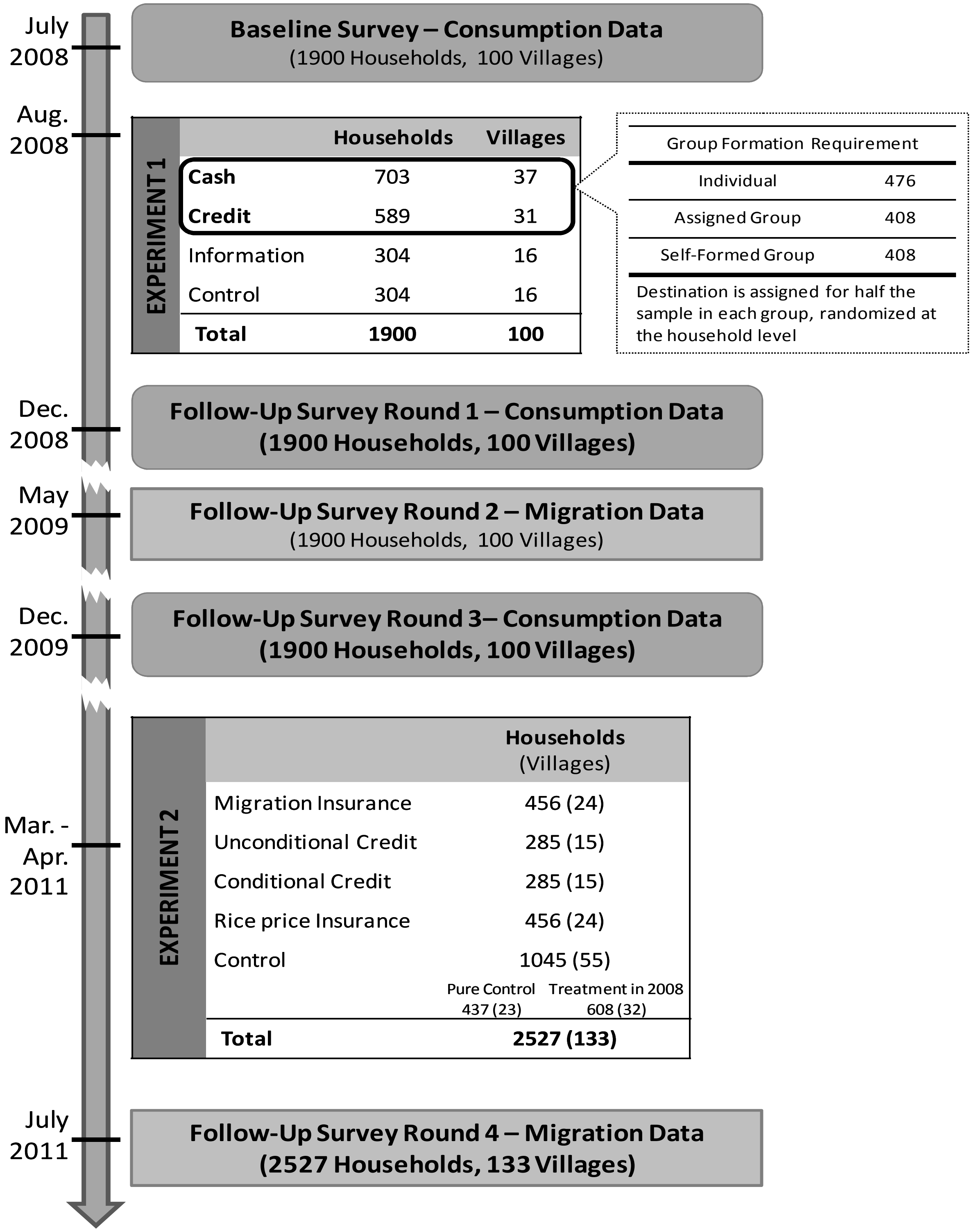


Figure 3: Migrant Distribution of earnings at the Destination (Takas per day)

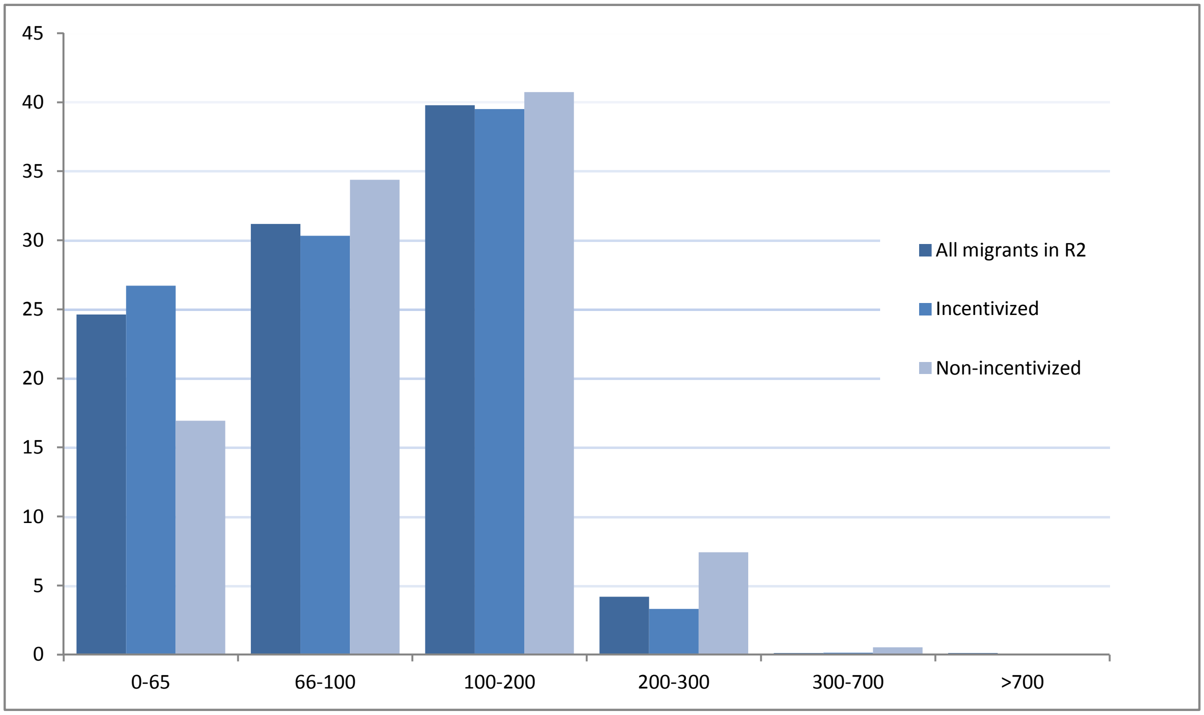
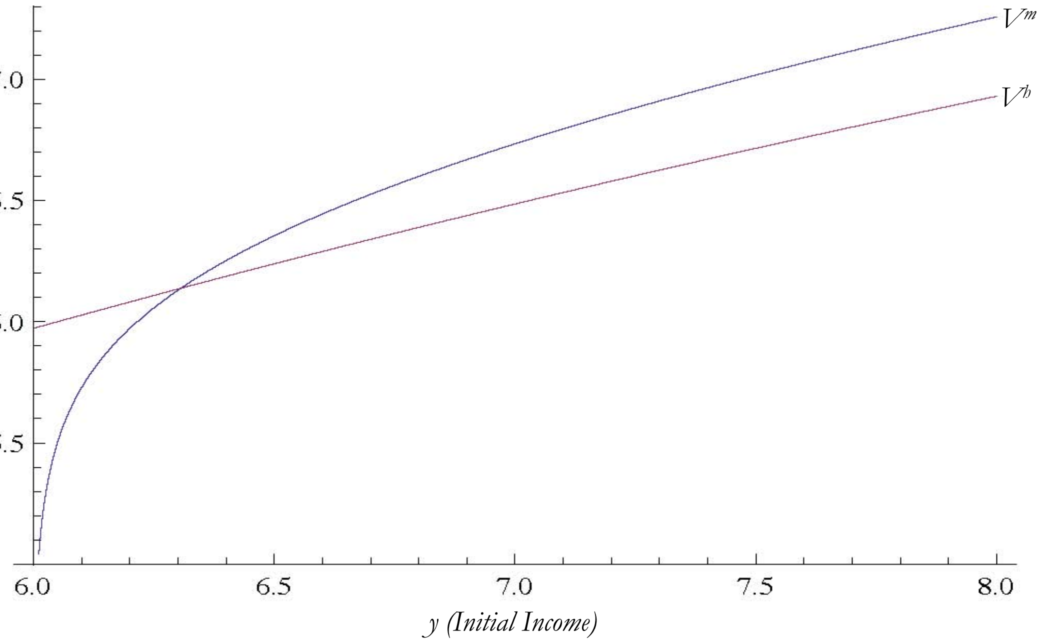


Figure 4: The return to migration as a function of income



u = ln, μ(g) = 0.7, b = -6, g = 3 and δ = 0.7

Figure 5: Expected Long-run Income as a Function of Initial Income

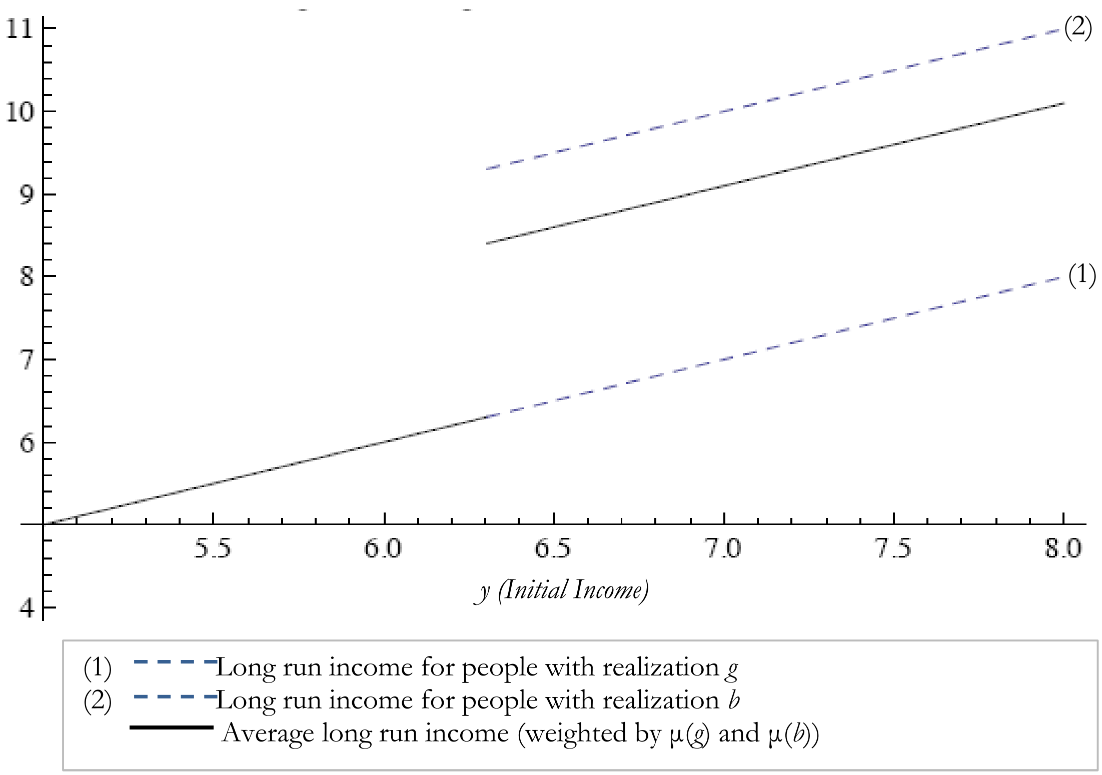
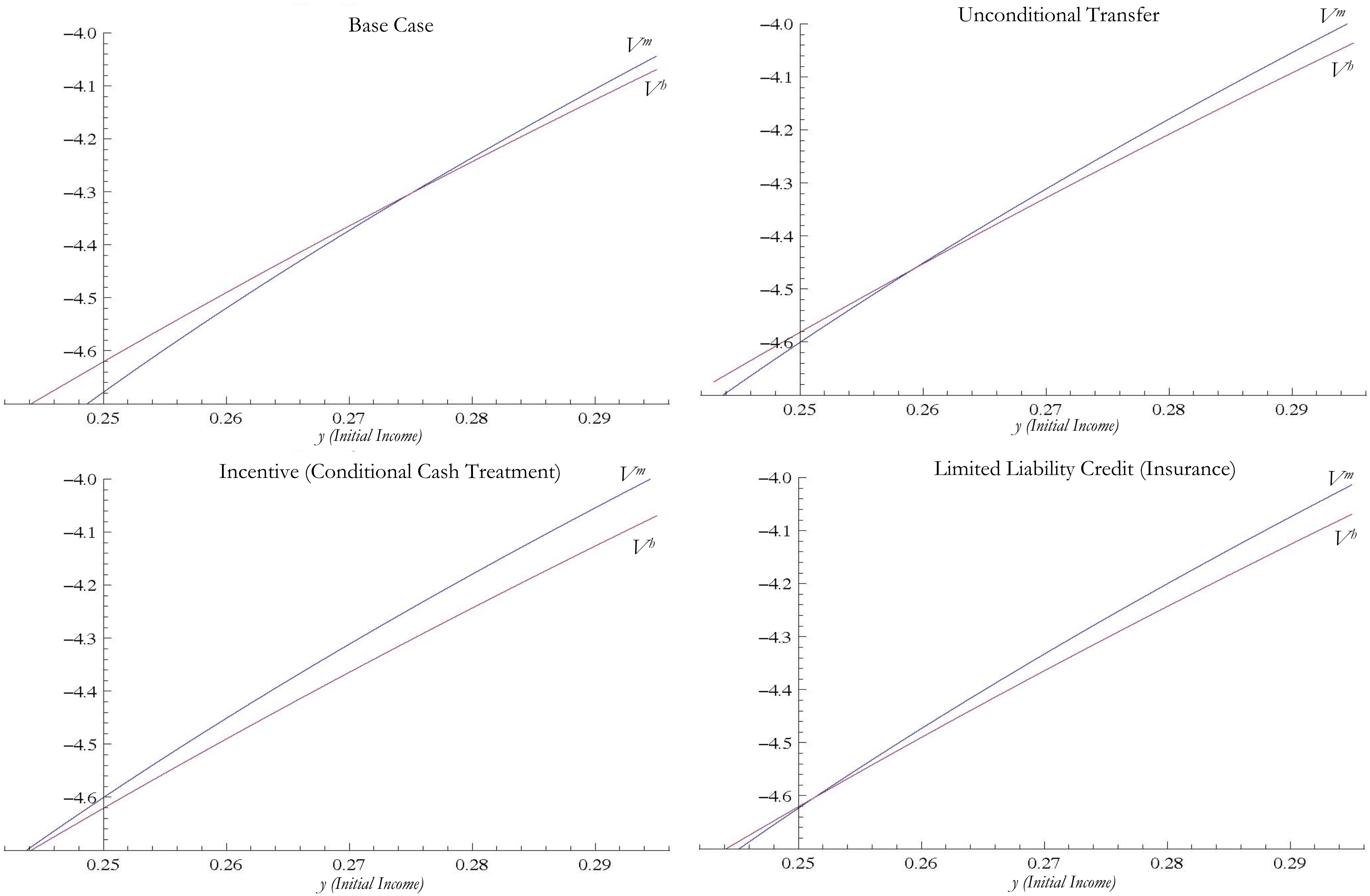
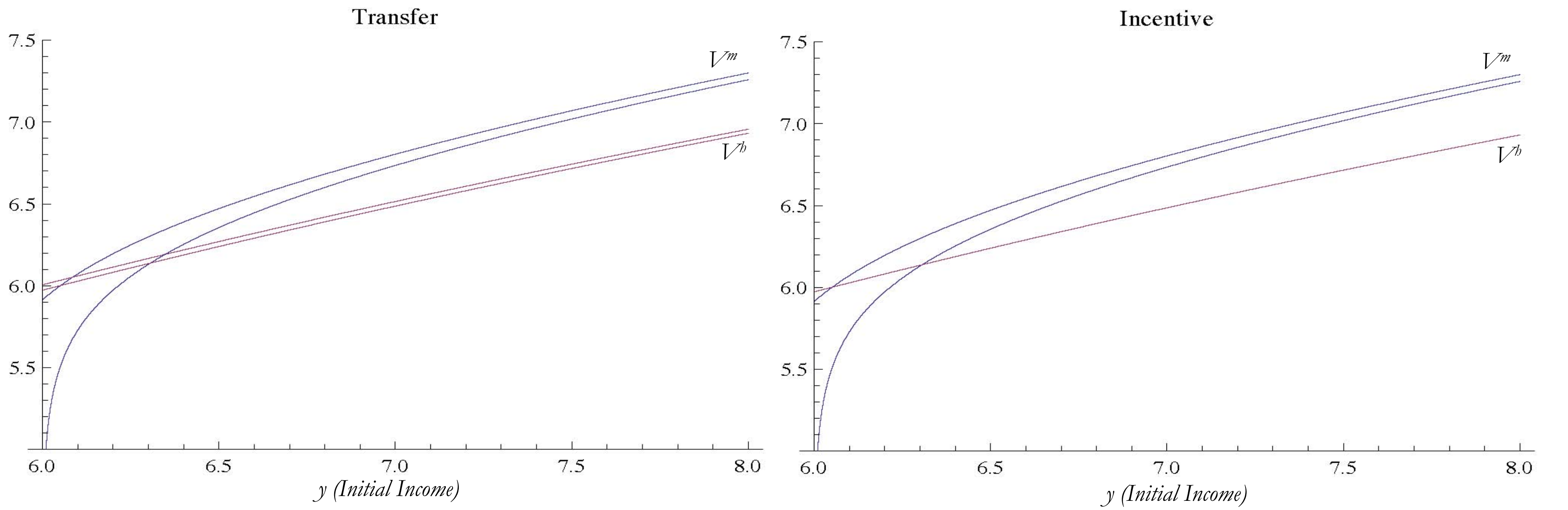


Figure 6: Effects of Various Policies on Migration



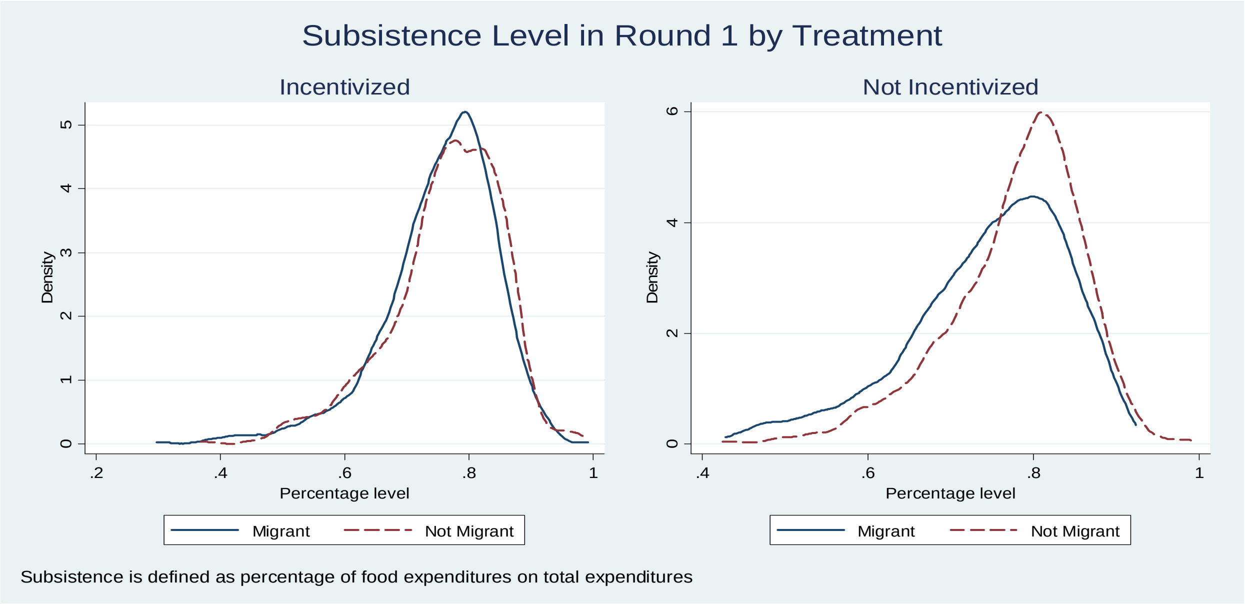
$u = ln$, $\mu(g) = 0.7$, $b = -0.2$, $g = 0.05$ and $\delta = 0.7$

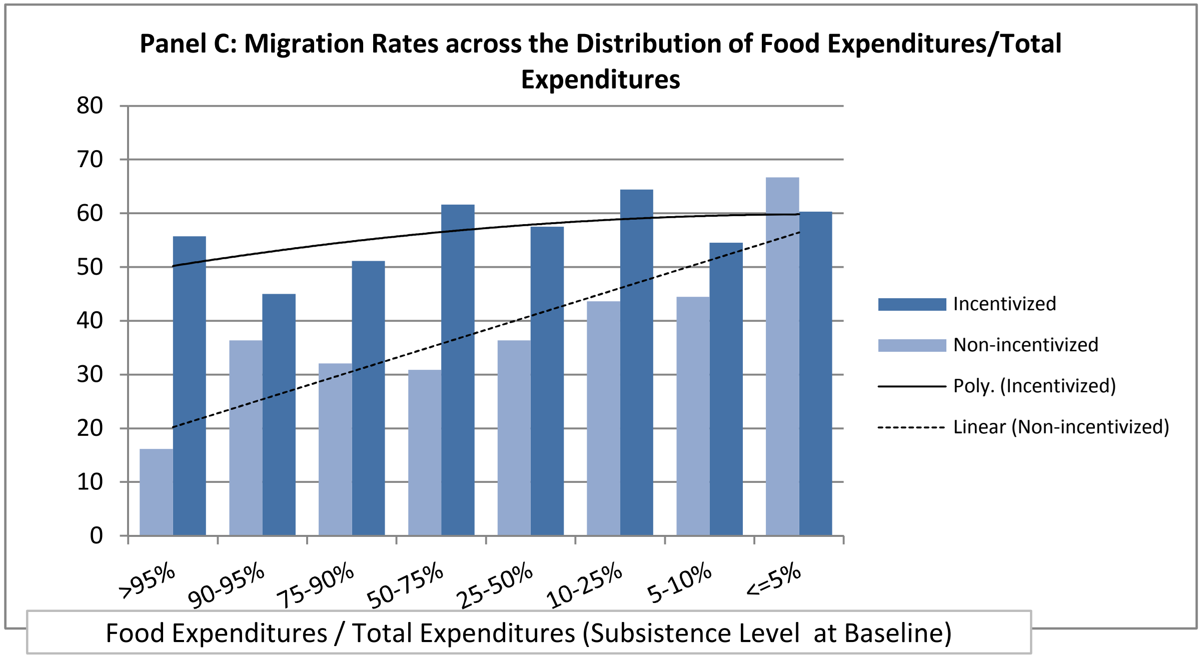
Figure 7: Similar Effects of Transfers and Incentives Farther from Subsistence



$u = ln, \mu(g) = 0.7, b = -6, g = 3$ and $\delta = 0.7$

Figure 8: Heterogeneity in Migration Responsiveness to Treatment by Subsistence Level





Panel B: Migration Decision as a Function of Baseline Subsistence

Incentivized	-0.252
	(0.185)
Ratio of Food Expenditure over Total Expenditure Round 1	-0.870***
	(0.204)
Interaction: Ratio of food to total * Incentivized	0.567**
	(0.240)
Constant	0.412*
	(0.227)
Observations	1860
R-squared	0.189

Robust standard errors in parentheses, clustered by village. *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is "Migration" , a binary variable equal to 1 if at least one member of the household migrated and 0 otherwise. Additional treatment variables included but not shown were: random assignment into individual or group migration and random assignment by migration destination. Additional controls were number of adult males at the baseline, number of children at the baseline, past migration dummy, lacked access to credit, borrowing, total household expenditures per capita measured at baseline, and social network support measured at baseline.

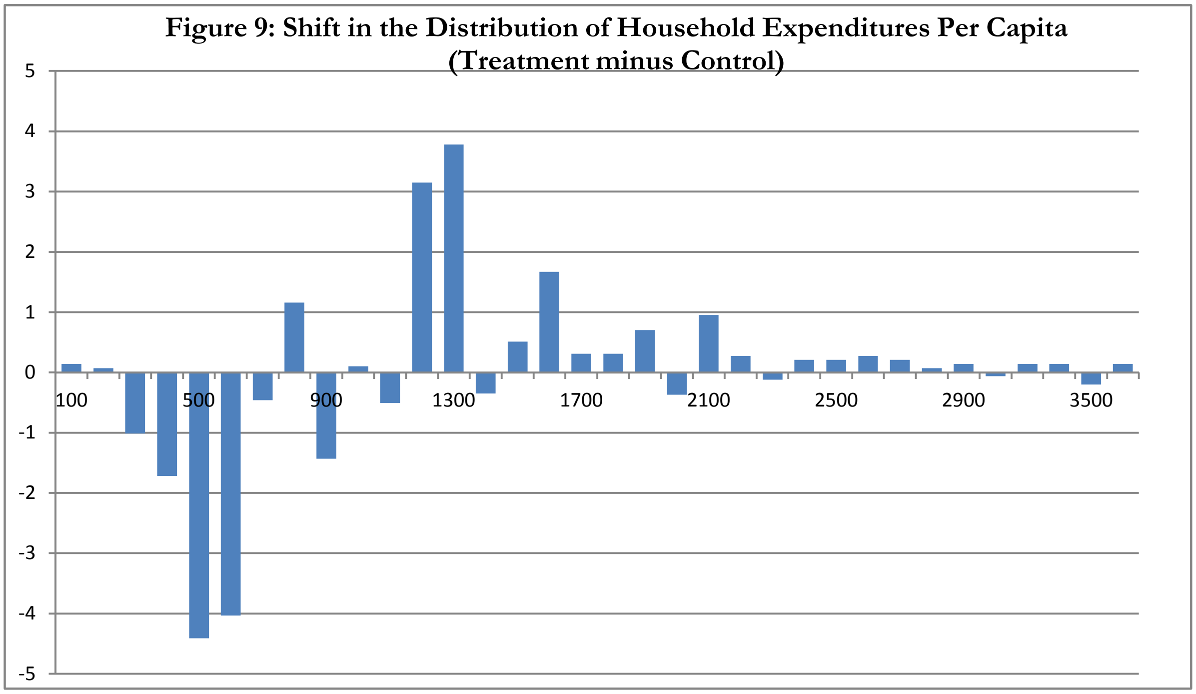
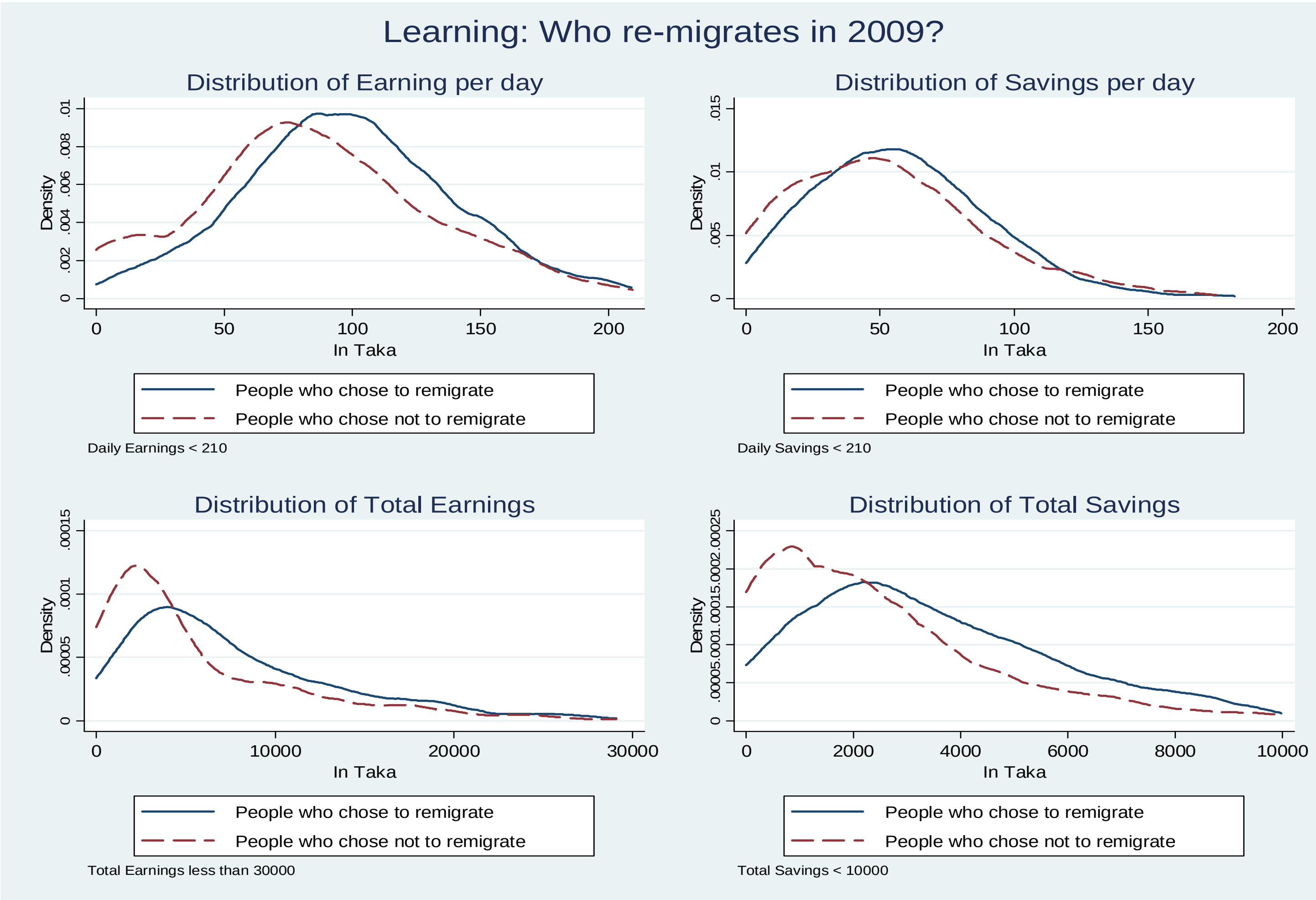


Figure 9: Shift in the Distribution of Household Expenditures Per Capita
(Treatment minus Control)
5
4
3
2
1
0
-1
-2
-3
-4
-5
100
500
900
1300
1700
2100
2500
2900
3500

Figure 10: Learning and Re-migration



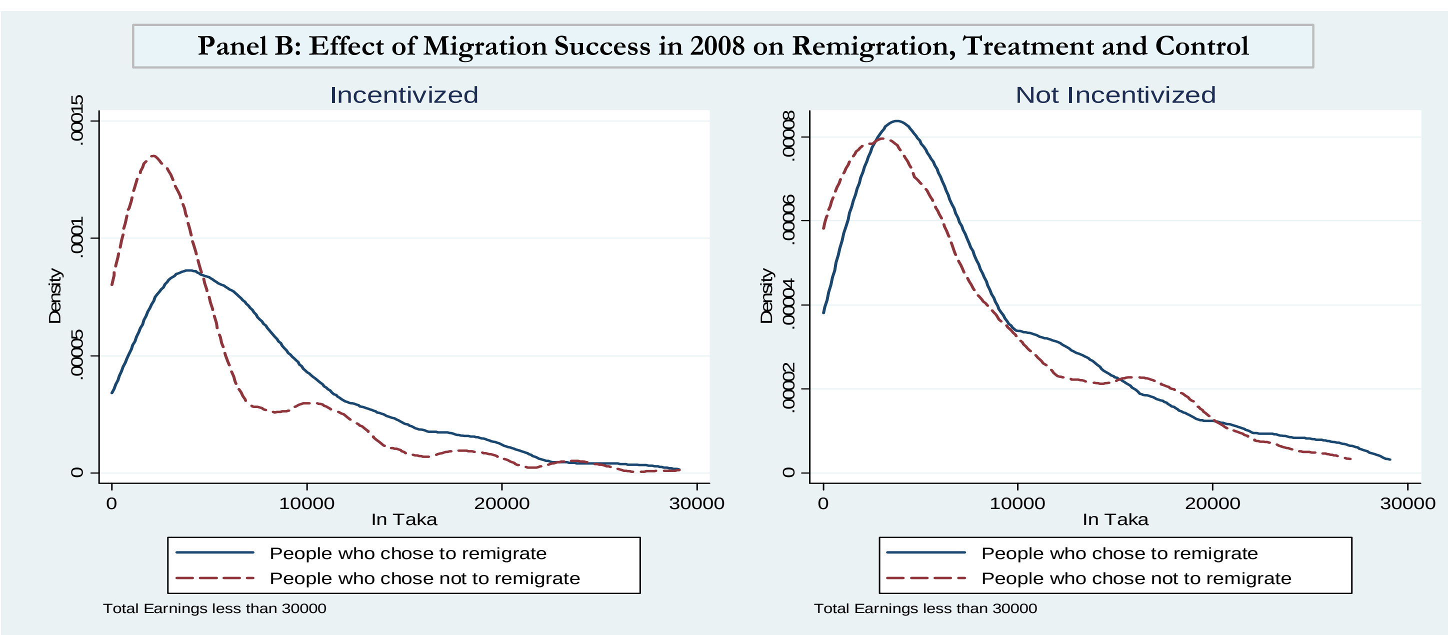


Table 1. Randomization Balance on Observables at Baseline

	Incentivized		Non-Incentivized		Diff I v NI	P-value
	Cash	Credit	Control	Info		
Food Expenditures	805.86	813.65	818.68	768.64	15.84	0.638
	(19.16)	(40.91)	(31.76)	(18.00)	(33.57)	
Non Food Expenditures	248.98	262.38	248.4	237.35	12.23	0.278
	(5.84)	(6.74)	(9.28)	(7.99)	(11.20)	
Total Expenditure	1054.83	1076.03	1067.08	1005.99	28.06	0.465
	(21.11)	(42.08)	(34.55)	(22.77)	(38.29)	
Calorie intake per person per day	2081.19	2079.51	2099.3	2021.31	20.25	0.585
	(20.34)	(22.76)	(30.44)	(32.56)	(36.99)	
Total Calories from Protein per person per day	45.66	45.3	46.26	44.75	-0.01	0.992
	(0.54)	(0.57)	(0.77)	(0.85)	(0.92)	
Expenditures on Meat Products	25.04	18.24	27.13	20.71	-1.97	0.594
	(2.58)	(2.0)	(3.24)	(2.90)	(3.69)	
Expenditures on Milk & Eggs	11.74	9.77	9.96	10.77	0.48	0.675
	(0.79)	(0.80)	(1.12)	(1.19)	(1.13)	
Expenditures on Fish	42.17	39.86	41.36	45.98	-2.56	0.496
	(1.83)	(1.79)	(2.76)	(2.89)	(3.74)	
Expenditure on Children's Education	24.14	27.14	22.31	16.95	6.01	0.016**
	(1.75)	(2.31)	(2.34)	(2.1)	(2.44)	
Expenditures on Clothing and Shoes	37.31	38.8	39.24	38.35	-0.80	0.693
	(0.79)	(0.90)	(1.41)	(1.30)	(2.02)	
Expenditures on Health for Male	52.39	52.9	63.72	47.45	-2.86	0.696
	(5.14)	(5.23)	(8.15)	(6.48)	(7.28)	
Expenditures on Health for Female	37.34	52.5	39.36	49.75	-0.31	0.961
	(3.52)	(5.75)	(5.68)	(7.51)	(6.26)	
Total Saving per HH	1345.55	1366.37	1418.29	1611.05	-160.56	0.255
	(97.54)	(121.26)	(135.04)	(185.56)	(140.09)	
HH size	3.93	3.98	3.99	4.05	-0.07	0.473
	(0.05)	(0.05)	(0.08)	(0.08)	(0.10)	
HH Head Education 1=Educated	0.25	0.24	0.25	0.22	0.01	0.628
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	
Number of Males Age>14	1.19	1.22	1.18	1.18	0.03	0.515
	(0.02)	(0.02)	(0.03)	(0.03)	(0.04)	
Number of Children Age<9	1.01	1.05	1.08	1.15	-0.09	0.093
	(0.03)	(0.04)	(0.05)	(0.05)	(0.05)	
Household has pucca walls	0.29	0.32	0.27	0.30	0.02	0.55
	(0.02)	(0.02)	(0.03)	(0.03)	(0.04)	
Literacy score average	3.37	3.40	3.48	3.30	-0.01	0.84
	(0.04)	(0.04)	(0.05)	(0.06)	(0.06)	
Subjective expectation: Monga occurrence this year	78.79	78.62	78.38	75.72	1.66	0.47
	(0.77)	(0.88)	(1.15)	(1.35)	(2.32)	
Subjective expectation: Will get social network help in Dhaka	58.53	60.82	58.38	57.40	1.68	0.41
	(1.07)	(1.21)	(1.64)	(1.61)	(2.04)	
Subjective expectation: Can send remittance from Dhaka	52.53	52.90	52.42	51.15	0.91	0.70
	(1.13)	(1.25)	(1.78)	(1.72)	(2.40)	
Ratio of food expenditure over total expenditure in round 1	0.77	0.75	0.77	0.77	-0.01	0.21
	(0.003)	(0.09)	(0.01)	(0.004)	(0.01)	
Average skill score received by network	6.53	6.49	6.24	6.20	0.27	0.24
	(0.05)	(0.27)	(0.07)	(0.07)	(0.23)	
Appled and refused for credit or didn't apply because of insufficient collateral	0.03	0.04	0.04	0.04	-0.00	0.75
	(0.01)	(0.004)	(0.01)	(0.01)	(0.01)	
Received credit from NGO, family and friends, or money lender	0.68	0.65	0.70	0.60	0.02	0.55
	(0.02)	(0.02)	(0.03)	(0.03)	(0.04)	
Migration to Bogra in round 1	0.11	0.10	0.16	0.12	0.03	0.30
	(0.01)	(0.01)	(0.02)	(0.02)	(0.03)	

Notes. First four columns show the mean of the corresponding variables; fifth column shows the difference between the means of incentivized and non-incentivized groups. Standard errors are reported in parentheses. P-values are derived from testing the difference between the means of incentivized cash and credit and control control and info groups; linear regression is used where the dependent variables are the variables of interest and the only control is incentivized, a binary variable equal to 1 if treatment group and 0 otherwise ; robust standard errors clustered at the village level are reported. All expenditure categories are monthly totals, reported on per capita basis based on the size of the household .

Table 2: Program Take-up Rates

	Migration Rate in 2008		
Cash	59.0%		
	(1.87)		
Credit	56.8%		
	(2.06)		
Info	35.9%		
	(2.80)		
Control	36.0%		
	(2.76)		
	Incentivized	Not Incentivized	Diff
Migration Rate in 2008	58%	36%	22***
	(1.4)	(1.96)	(2.43)
Migration Rate in 2009	47%	37%	10***
	(1.41)	(2.0)	(2.48)
Migration Rate in 2011	44%	36%	8***
	(1.33)	(1.51)	(2.0)

The P-value is obtained from the testing difference between migration rates of incentivized (Cash and Credit) and non-incentivized households (Info and Control), regardless of whether they accepted our cash or credit. No incentives were offered in 2009. For re-migration rate in 2011, we compare migration rates in "pure control" villages that never received any incentives to villages that only received incentives in 2008 and never again.

Table 3. First Stage: Migration as a Function of Treatments in 2008

	Migration in 2008	
Cash	0.191***	0.193***
	(0.0493)	(0.0453)
Credit	0.177***	0.174***
	(0.0478)	(0.0449)
Info	0.00106	0.00312
	(0.0565)	(0.0523)
Sub-district fixed effects?	yes	yes
Additional controls?	no	yes
Observations	1,871	1,827
R-squared	0.086	0.130
1st F-test	12.633	14.424
1st pvalue	0.000	0.000
1st partial R2	0.028	0.029

Robust standard errors in parentheses, clustered by village. *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is a binary variable equal to 1 if at least one member of household migrated. Additional controls included in columns 2 and 4 were: household education, proxy for income (wall material), percentage of total expenditure on food, number of adult males, number of children, lacked access to credit, borrowing, total household expenditures per capita measured at baseline, and subjective expectations about Monga and social network support measured at

Table 4: Effects of Migration in 2008 on Expenditure Amongst Remaining Household Members

Dependent Variable	Effect of Migration before December 2008 on Consumption in 2008					Effect of Migration in 2008 on Consumption in 2009	
	OLS	OLS	IV	IV	Mean	IV	Mean
Food Expenditures	102.714***	115.073***	280.792**	260.139**	702.9	199.360**	838.86
	(17.147)	(17.170)	(131.954)	(128.053)		(96.529)	
Non Food Expenditures	59.085***	67.187***	115.003**	99.924*	251.2	78.676	314.45
	(8.960)	(8.693)	(56.692)	(51.688)		(64.426)	
Total Expenditures	160.696***	180.894***	391.193**	355.115**	954.1	278.036**	1153.31
	(22.061)	(21.432)	(169.431)	(158.835)		(131.800)	
Caloric intake (per person per day)	350.271***	383.331***	872.820***	788.118***	2060.5	466.420**	1951.97
	(41.971)	(43.225)	(243.244)	(245.798)		(211.315)	
Total Calories from Protein	7.516***	8.283***	18.235***	16.380**	45.3	9.414**	43.72
	(1.022)	(1.050)	(6.962)	(6.730)		(4.420)	
Expenditures on Meat Products	4.497	6.842	32.787	35.208	23.7	10.284	25.45
	(3.970)	(4.163)	(21.577)	(21.416)		(15.395)	
Expenditures on Milk & Eggs	2.005	1.330	-4.646	-6.776	13.6	13.947	20.24
	(1.719)	(1.709)	(9.200)	(9.145)		(11.505)	
Expenditures on Fish	10.119**	11.970***	36.299	30.482	66.0	8.816	61.97
	(3.879)	(3.853)	(24.899)	(22.893)		(19.683)	
Expenditure on Children's Education	-3.651	-5.087**	30.924**	21.552	15.0	0.963	19.82
	(2.355)	(2.424)	(14.145)	(13.541)		(7.795)	
Expenditures on Clothing and Shoes	10.671***	11.131***	11.329	9.409	36.7	3.408	37.05
	(1.678)	(1.641)	(8.882)	(8.359)		(4.418)	
Expenditures on Health for Female	-10.932	-11.844	39.920	32.821	57.8	-37.139	69.62
	(7.250)	(7.551)	(40.482)	(41.732)		(63.125)	
Expenditures on Health for Male	22.823**	24.566***	23.653	7.562	69.1	98.051**	89.19
	(9.180)	(9.336)	(37.574)	(39.319)		(49.793)	
Sub-district Fixed Effects?	yes	yes	yes	yes		yes	
Additional controls	no	yes	no	yes		yes	

Robust standard errors in parentheses, clustered by village. *** p<0.01, ** p<0.05, * p<0.1. Each coefficient entry in the table comes from a separate regression where the dependent variable (in column 1) is regressed on "migration". "Migration" is a binary variable equal to 1 if at least one member of the household migrated and 0 otherwise. The fifth column reports sample mean of the dependent variable in the control group. The third and fourth columns are instrumental variables specifications where migration is instrumented by the random assignment to cash and credit treatments. The sixth column reports instrumental variables specification where the effect of migration in 2008 (including migration after December) is estimated on consumption in 2009. All expenditure variables are measured in units of Takas per person per month, except Caloric Intake which is measured in terms of calories per person per day. Some expenditure items in the survey were asked over a weekly recall and other less frequently purchased items were asked over a bi-weekly or monthly recall. The denominator of the dependent variable (household size) is the number of individuals who have been present in the house for at least seven days. Additional controls included in columns 2 and 4 were: household education, proxy for income (wall material), percentage of total expenditure on food, number of adult males, number of children, lacked access to credit, borrowing, total household expenditures per capita measured at baseline, and subjective expectations about Monga and social network support measured at baseline.

Table 5. Migrant Earnings and Savings at the Destination

	All Migrants	Incentivized	Not Incentivized	Diff	Obs
Total Savings by household	3490.47	3506.59	3434.94	71.65	951
	(97.22)	(110.83)	(202.80)	(232.91)	
Total Earnings by household	7777.19	7451.27	8894.40	-1443.129**	952
	(244.77)	(264.99)	(586.14)	(583.83)	
Savings per day	56.96	56.56	58.37	-1.81	902
	(1.16)	(1.29)	(2.58)	(2.8)	
Earnings per day	99.39	96.09	111.15	-15.06**	926
	(1.75)	(1.92)	(4.0)	(4.2)	
Remittances per day	18.34	16.94	23.33	-6.39**	927
	(1.06)	(1.19)	(2.28)	(2.55)	
Travel Cost per Episode	264.55	264.12	266.00	-1.88	953
	(3.41)	(3.82)	(7.84)	(8.16)	

*** p<0.01, ** p<0.05, * p<0.1, the "Diff" columns tests statistical differences between incentivized and non incentivized groups. Standard errors are reported in parentheses. The measures for total savings and earnings, and savings and earnings per day do not include outliers (Less than 20,000 for total savings and 120000 for earnings, individuals savings per day less than 500 and individuals ernings per day less than 700). Travel cost refers to the cost of food and travel to get to the destination. Average migration duration 76 days.

Table 6: 2008 Migrant Characteristics by Destination and by Sector

Sector	Dhaka	Mushigonj	Tangail	Bogra	Other	Total earnings
Agriculture	17.54	75	91.15	89.62	46.83	3230.52
	(1.71)	(2.50)	(1.89)	(2.26)	(2.26)	(77.68)
Non-ag day laborer	20.56	9	5.75	3.83	19.02	6039.72
	(1.82)	(1.66)	(1.55)	(1.42)	(1.78)	(317.52)
Transport	40.93	11	1.33	1.09	15.34	4993.81
	(2.21)	(1.81)	(0.76)	(0.77)	(1.63)	(203.12)
Other	20.97	5	1.77	5.46	18.81	5645.98
	(1.83)	(1.26)	(0.88)	(1.68)	(1.77)	(321.72)
Number of migration episodes	496	300	226	183	489	1,694
Total earnings	5005.06	3777.30	2897.88	2491.07	5160.60	
	(185.92)	(156.0)	(145.72)	(123.19)	(188.69)	

Notes: Standard errors are in parentheses. Based on migration for work episodes between September 1, 2008 to April 13, 2009. Occupation at the destination is based on the following question: "In which sector were you employed (agriculture, industry, etc)?"

Table 7. Differences in Characteristics Between Migrants in Treatment and in Control Group

Panel A: Percentage of People that Know Someone at Destination

	Incentive	Non incentive	Diff
First Episode	47%	64%	17***
	(1.85)	(3.29)	(3.86)
Second Episode	60%	73%	13**
	(2.47)	(5)	(5.90)
Third Episode	69%	85%	16*
	(4)	(6.27)	(8.69)
Fourth Episode	86%	94%	8
	(6.09)	(6.25)	(9.85)

Panel B: Percentage of People that had a Job Lead at Destination

	Incentive	Non incentive	Diff
First Episode	27%	44%	17***
	(1.64)	(3.41)	(3.55)
Second Episode	29%	47%	18**
	(2.28)	(5.58)	(5.64)
Third Episode	39%	53%	15**
	(4.24)	(8.69)	(9.44)
Fourth Episode	54%	56%	2.00
	(8.54)	(12.81)	(15.32)

Panel C: Percentage of Migrants Traveling Alone

	Incentive	Non incentive	Diff
First Episode	32%	38%	6**
	(1.25)	(2.52)	(2.75)
Second Episode	34%	49%	15***
	(1.49)	(3.33)	(3.52)
Third Episode	36%	60%	24***
	(2.23)	(4.32)	(4.81)
Fourth Episode	54%	83%	29***
	(3.95)	(4.40)	(6.52)
Fifth Episode	54%	84%	30***
	(5.76)	(6.72)	(9.99)

*** p<0.01, ** p<0.05, * p<0.1. Standard errors are in parentheses.

Table 8. Learning from Own Experience and Friends' Experience in 2009 Re-migration Decision

Dep. Var.: Migration in 2009	OLS	IV	OLS	IV	IV	IV	IV
Did any member of the household Migrate in 2008?	0.438***	0.480***	0.473***	0.435***	0.473***	0.435***	0.457***
	(0.129)	(0.128)	(0.126)	(0.126)	(0.126)	(0.126)	(0.142)
Successful migration in 2008 (based on total earnigns above the median)			0.153***	0.188*			
			(0.0311)	(0.113)			
Successful migration (based on total savings above the median)							
Number of friends who migrated					-0.0559		
					(0.0497)		
Number of relatives who migrated					0.00903		
					(0.0285)		
Number of friends with successful migration (based on total earnings above the median)						0.0826	
						(0.139)	
Number of friends with unsuccessful migration (based on total earnings above the median)						-0.0659	
						(0.507)	
Number of relativess with successful migration (based on total earnings above the median)							-0.0549
							(0.347)
Number of relatives with unsuccessful migration (based on total earnings above the median)							-0.0722
							(0.362)
Constant	0.111***	0.110	0.127***	0.112	0.0758	0.0955	0.0862
	(0.0374)	(0.0810)	(0.0313)	(0.0743)	(0.0714)	(0.0727)	(0.0712)
Sub-district fixed effects?	yes	yes	yes	yes	yes	yes	yes
Observations	1,829	1,829	1,810	1,810	1,775	1,775	1,775
R-squared	0.200	0.200	0.215	0.213	0.193	0.202	0.194

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1, Upazila Fixed effects included. Friends, Relatives, and Know Well are categories referring to the relationship of the individual with others. Success is a binary variable defined relative to median earnings, and takes on the value 0 for non-migrants.

Table 9. Destination Choices of Re-migrants

	OLS	IV
Migration in 2009 to Dhaka -	0.413***	0.679*
Migration in 2008 to Dhaka	(0.052)	(0.366)
Migration in 2009 to Bogra -	0.333***	0.0648
Migration in 2008 to Bogra	(0.061)	(0.261)
Migration in 2009 to Tangail -	0.470***	0.908**
Migration in 2008 to Tangail	(0.057)	(0.421)
Migration in 2009 to Munshigonj -	0.233***	-0.0360
Migration in 2008 to Munshigonj	(0.050)	(0.330)
Migration in 2011 to Dhaka -	0.307***	0.521*
Migration in 2008 to Dhaka	(0.053)	(0.308)
Migration in 2011 to Bogra -	0.292***	0.126
Migration in 2008 to Bogra	(0.062)	(0.254)
Migration in 2011 to Tangail -	0.405***	1.016***
Migration in 2008 to Tangail	(0.077)	(0.319)
Migration in 2011 to Munshigonj -	0.271***	-0.130
Migration in 2008 to Munshigonj	(0.057)	(0.295)

Notes. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Each coefficient entry in the table comes from a separate regression where migration to a specific destination in 2009 is regressed on migration to that same destination in 2008. The dependent variable is equal to one if at least one household member migrated to the destination specified in the first column (Dhaka, Bogra, Tangail or Munshigonj) in 2009 (rows 1-4), or in 2011 (rows 5-8). The independent variable whose coefficient is reported is a binary variable equal to 1 if at least one member of the household migrated to that destination in 2008 and 0 otherwise. The second column reports instrumental variables specifications where migration in 2008 to a particular destination is instrumented by the random assignment to cash and credit treatments, and the individual level treatments (see figure 2), including the requirement to travel to a specific destination (omitted category is self-chosen destination). Sub-district fixed effect are included but not reported. The sample includes only households that sent a migrant in both 2008 and 2009.

Table 10. Determinants of Returning to the Same Destination in 2009

Dep var: Migrant Returned to Same Destination in 2009	(1)	(2)
Was your migration successful (Based on Expectations)	0.0748**	0.0712**
	(0.0306)	(0.0302)
Was your migration successful (Based on Earnings)	0.0865**	0.0889**
	(0.0376)	(0.0375)
Number of Relatives and Friends that went to the same destination that were succesful (Earnings)	0.152***	
	(0.0449)	
Number of Relatives & Friends that went to the same destination and were unsuccesful (Earnings)	0.0434	
	(0.0353)	
Number of Relatives and Friends that went to the same destination that were succesful (Expectations)		0.113***
		(0.0322)
Number of Relatives & Friends that went to the same destination and were unsuccesful (Expectations)		0.0632
		(0.0597)
Constant	0.178***	0.178***
	(0.0490)	(0.0484)
Observations	833	833
R-squared	0.065	0.063
Mean of dependent variable	0.46	0.46

Robust standard errors clustered at the village level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Sub-district fixed effects are included. Additional controls included were: random assignment into cash or credit treatment; a binary variable equal to one for whether migrant knew someone at the destination and whether he/she had a job lead; and controls for each of the following destinations: Dhaka, Bogra, Tangail, Mushigonj and Comolla (other destinations is omitted category).

Table 11. Treatment Effects in 2011 Accounting for basis risk in the Insurance Program

	All	Non-farmer	Farmer
Conditional credit	0.175***	0.0697	0.256***
	(0.0707)	(0.0569)	(0.0683)
Rain insurance	0.157***	0.258***	0.0900
	(0.0505)	(0.0537)	(0.0598)
Unconditional credit	0.0741	0.0901	0.0989
	(0.0608)	(0.0574)	(0.0692)
Went to Bogra before baseline		0.157***	0.138***
		(0.0702)	(0.0448)
Went to Bogra before baseline * Rain insurance		0.277***	0.0429
		(0.139)	(0.136)
Went to Bogra before baseline* Cond. credit		0.326	-0.123
		(0.225)	(0.136)
Constant	0.328***	0.197***	0.396***
	(0.0260)	(0.0301)	(0.0306)
Observations	2,407	1,085	1,314
R-squared	0.016	0.052	0.028
F-test conditional credit = rain insurance	0.0577		
Prob>F1	0.811		
F-test Bogra credit = Bogra rain		0.314	0.00
Prob>F2		0.576	1
F-test 0 = Bogra rain		14.95	0.337
Prob>F3		0.00	0.930

Robust standard errors are clustered at the village level in parentheses, * p<0.1, ** p<0.05, *** p<0.01. The dependent variable is migration in 2011, equal to 1 if at least one household member migrated and 0 otherwise. Omitted category is a control group that never received any treatment. Impure control includes households that are control households in 2011 but received cash or credit in 2008. "Went to Bogra before baseline" is a binary variable equal to 1 if household reported sending a migrant to Bogra prior to baseline.