ECONOMIC GROWTH CENTER
YALE UNIVERSITY

P.O. Box 208269
New Haven, CT 06520-8269

http://www.econ.yale.edu/~egcenter

Escaping Famine Through Seasonal Migration

Gharad Bryan
London School of Economics

Shyamal Chowdhury
University of Sydney

Ahmed Mushfiq Mubarak*
Yale University, School of Management

June 2013

Escaping Famine through Seasonal Migration

Gharad Bryan, Shyamal Chowdhury and

Ahmed Mushfiq Mobarak*

June 2013

Abstract

Hunger during pre-harvest lean seasons is widespread in the agrarian areas of Asia and Sub-Saharan Africa. We randomly assign an $8.50 incentive to households in rural Bangladesh to out-migrate during the lean season. The incentive induces 22% of households to send a seasonal migrant, their consumption at the origin increases significantly, and treated households are 8-10 percentage points more likely to re-migrate 1 and 3 years after the incentive is removed. These facts can be explained qualitatively by a model in which migration is risky, mitigating risk requires individual-specific learning, and some migrants are sufficiently close to subsistence such that failed migration is very costly. We document evidence consistent with this model using heterogeneity analysis and additional experimental variation, but calibrations with forward-looking households that can save up to migrate suggest that it is difficult for the model to quantitatively match the data. We conclude with extensions to the model that could provide a better quantitative accounting of the behavior.

Keywords: Seasonal Migration, Bangladesh, Risk
JEL Codes: O1, O15, J61, R23

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 regular pre-harvest seasonal famine (The Daily Star, 2011). This seasonal famine – known locally as monga – is emblematic of the widespread 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] 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, 2012). Understanding how a famine can occur every year despite the existence of potential mitigation strategies is, however, more challenging. We explore one obvious mitigation option – temporary migration to nearby urban areas that offer better employment opportunities. We randomly assigned a cash or credit incentive (of $8.50, which covers the round-trip travel cost) conditional on a household member migrating during the 2008 monga season. We document very large economic returns to migration. To explore why people who were induced to migrate by our program were not already migrating despite these high returns, we build a model with risk aversion, credit constraints and savings.

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

---

[1] Seasonal poverty has been documented in Ethiopia (Dercon & Krishnan, 2000), where poverty and malnourishment increase 27% during the lean season, Mozambique and Malawi (Brune et al., 2011) – where people refer to a "hungry season", Madagascar, where (Dostie et al., 2002) estimate that 1 million people fall into poverty before the rice harvest, Kenya, where (Swift, 1989) distinguishes between years that people died and years of less severe shortage, Francophone Africa (the soudure phenomenon), Indonesia (Basu & Wong, 2012) ('musim paceklik' or 'famine season' and 'lapar biasa' or 'ordinary hunger period'), Thailand (Paxson, 1993), India (Chaudhuri & Paxson, 2002) and inland China (Jalan & Ravallion, 2001).

by difficult selection issues (Akee, 2010; Grogger & Hanson, 2011).[2] Most closely related to our work is a small number of experimental and quasi-experimental studies of the effects of migration, many of which are cited in McKenzie and Yang (2010) and McKenzie (2012). These studies often exploit exogenous variation in immigration policies to study the effects of permanent international migration.[3]

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. Most strikingly, households in the treatment areas continue to migrate at a higher rate in subsequent seasons, 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.

These large effects on migration rates, consumption and re-migration raise an 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 were likely the highest (Hatton & Williamson, 1998). Ardington et al (2009) provide similar evidence of constraints preventing profitable out-migration in rural South Africa.

---

[2] Prior attempts use controls for observables (Adams, 1998), selection correction methods (Barham & Boucher, 1998), matching (Gibson & McKenzie, 2010), instrumental variables (Brown & Leeves, 2007; McKenzie & Rapoport, 2007; Yang, 2008; Macours & Vakis, 2010), panel data techniques (Beegle et al., 2011) , and natural policy experiments (Clemens, 2010; Gibson et al., 2013) to estimate the causal impact of migration.

[3] A related literature studies the effects of exogenous changes in destination conditions on remittances, savings and welfare at the origin (Martinez,Claudia A.,Yang,Dean, 2005; Aycinena et al., 2010; Chin et al., 2010; Ashraf et al., forthcoming).

To interpret our experimental findings, the second part of our paper provides a simple benchmark 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. Given a potential downside to migration (which we show exists in our data), households may fear an unlikely but disastrous outcome in which they pay the cost of moving, but return hungry after not finding employment during a period in which 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. Such frictions may be part of what keeps workers in agriculture despite the persistent productivity gap between rural agriculture and urban non-agriculture sectors (Gollin et al., 2002; Caselli, 2005; Restuccia et al., 2008; Vollrath, 2009; Gollin et al., 2011; McMillan & Rodrik, 2011).

Experimentation is deterred by two key elements: (a) individual-specific risk, and (b) the fact that individuals are close to subsistence, making migration failure very costly. The model is related to the "poverty as vulnerability" view (Banerjee, 2004) – that the poor cannot take advantage of profitable opportunities because they are vulnerable and afraid of losses (Kanbur, 1979; Kihlstrom & Laffont, 1979; Banerjee & Newman, 1991). A model with these elements may also shed light on a number of other important puzzles in growth and development. 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 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 likely to be profitable. This is especially true in South Asia where the median farm is less than an acre, and therefore not easily divisible into experimental plots (Foster & Rosenzweig, 2011).[4] Similarly, to counter the surprisingly low adoption rates of effective health products (Kremer et al., 2009; Meredith et al., 2011; Miller & Mobarak, 2013), we may need to give households the opportunity to experiment with the new technology (Dupas, 2010), perhaps with free trial periods and other insurance schemes. Aversion to experimentation can also hinder entrepreneurship and business start-ups and growth (Hausmann & Rodrik, 2003; Fischer, 2013).

In the third part of the paper, we return to our data to assess whether empirical relationships are consistent with some of the qualitative predictions of the model. Much of the evidence supports our structure. We show that households that are close to subsistence – on whom experimenting with a new activity imposes the biggest risk – start with lower migration rates, but are the most responsive to our intervention. The households induced to migrate by our incentive are less likely to have pre-existing network connections at the destination, and exhibit learning about migration opportunities and destinations in their subsequent choices on whether and where to re-migrate.

We also conduct a new round of experiments in 2011 to test some further predictions of the model. We show that migration is 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 also implement another new treatment providing insurance for migration, and this offer induces just as many

---

[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).

[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.

households to migrate. Further, they respond to the insurance program design as if the environment is risky, and they are risk averse.

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. To explore, the fourth part of this paper calibrates the model allowing for buffer stock savings, and show that quantitatively, our model does not offer a fully satisfying explanation for the migration phenomena. Once agents in our model are allowed to save up to migrate, the level of risk aversion required to quantitatively account for our data appears to be implausibly high. This leads us to consider departures from full information and rationality and other market imperfections (such as savings constraints). We conclude that our experiment demonstrates that the ingredients of subsistence, risk aversion and learning that we outline in our model are important parts of any story, but some other extension to this basic setup is required to fully account for the experimental results. We therefore advocate care in interpreting our model: because we show that the model is not a complete description, any additional element that is needed to match the data may change or even reverse conclusions from our baseline model.

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 use the model to frame further discussion of the data in Section 6, calibrate the model and discuss its ability to rationalize the experimental results in Section 7, discuss some extensions to the baseline model in Section 8 and offer conclusions and some tentative policy implications in Section 9.

2   The Context: Rangpur 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 higher level of poverty compared to the rest of Bangladesh, 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, 2012). 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, 2012).[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. The drastic drop in purchasing power between planting and harvest threatens to take consumption below subsistence for Rangpur households, where agricultural wages are already the lowest in the country (Bangladesh Bureau of Statistics, 2011).

---

[6] Extreme poverty rates (defined as individuals who cannot meet the 2100 calorie per day food intake) 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.

Several puzzling stylized facts about 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 employment opportunities. Seasonal migration 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, 2012). 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 monga (Zug, 2006) 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, 2012).

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 there are jobs appears to be a sensible complementary policy to experiment with.

3 The Experiment and the Data Collected

The two districts where the project was 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] In August 2008 we randomly allocated the 100 villages into four groups: Cash, Credit, Information and Control. These treatments were subsequently implemented on the 19 households in each village in collaboration with PKSF through their partner NGOs with substantial field presence in the two districts.[9] 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

---

[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.

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 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.

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. These conditionalities created random within-village variation, which we will use as instrumental variables to study spillover effects from one person to another.

3.1 Data

We conducted a baseline survey of the 1900 sample households in July 2008, just before the onset of the 2008 Monga. We collected follow-up data in December

---

[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.

2008, at the end of the 2008 Monga season. These two rounds involved detailed consumption modules in addition to data on income, assets, credit and savings. The follow-up also asked detailed questions about migration experiences over the previous four months. We learnt that many migrants had not returned by December 2008, and therefore conducted a short follow-up survey in May 2009 to get more complete information about households' migration experiences. To study the longer-run effects of migration, and re-migration behavior during the next Monga season, we conducted another follow-up survey in December 2009. This survey only included the consumption module and a migration module. We conducted a new round of experiments to test our theories in 2011, and therefore collected an additional round of follow-up data on the re-migration behavior of this sample in July 2011. In summary, detailed consumption data was collected over 3 rounds: in July 2008 (baseline), December 2008 and December 2009. Migration behavior was collected in December 2008, May 2009, December 2009 and July 2011, which jointly cover three seasons in 2008, 2009 and 2011.

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.

4   Program Take-up and the Effects of Seasonal Migration

In this section we describe the main results of our initial (2008) experiment. Section 4.1 provides results on migration behavior. We first document the impact of the incentive on migration during the 2008 monga season (the season for which the incentive was in place). We then document the ongoing impact of the incentive

10

on migration in 2009 and 2011 (one and 3 years, respectively, after the incentive was removed). In Section 4.2 we look at the effect of the treatment on consumption at the origin (both in the short-run: 2008 and the long-run: 2009). We first provide both intent-to-treat and LATE estimates for consumption in December 2008 and then also look at the ongoing impact of the incentives on consumption in 2009. In Section 4.3 we look at migration income and savings at the destination.

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 quite consistent with each other, and Table 2 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 (36.0%) of households in control villages sent a seasonal migrant.[12] Providing information about wages and job opportunities at the

---

[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.

[12] In a large survey of 482,000 households in the Rangpur region, 36.0% of people report using "out-migration" as a coping mechanism for the Monga (Khandker et al., 2011). Our result appears very consistent

destination had no effect on the migration rate (the point estimate of the difference is 0.0% and is 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 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. 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 – a fact that our model will later account for. 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. Section 6.3.1 will show that this is almost entirely due to (a subset of) migrants who were induced in 2008 re-migrating. In other words, migration appears to be an "experience good". The July 2011 survey measured 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 incentive, the migration rate remains 8% higher in the villages

---

with the large-sample finding. Interestingly, survey respondents who qualified for government safety-net benefits were no more likely to migrate than households that did not.

randomly assigned to the cash or credit treatment in 2008.[13] The re-migration rates in 2009 and 2011 were significantly higher (relative to control) in the cash and credit groups separately.

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

This strong repeat migration also suggests that migration is an absorbing state, at least for some portion of the population. As we discuss further in Sections 6 and 8 this makes it hard to understand how our initial incentive was successful in inducing so much migration.

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

---

[13] Overall in our sample, 953 out of 1871 sample households sent a migrant in 2008 (and 723 of them traveled before our December 2008 follow-up survey), and 800 households sent a seasonal migrant during the 2009 monga season. The overall migration rate in 2011 was 40.8%.

[14] All socio-economic outcomes we measure using our surveys 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.

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 overcomes the problems associated with measuring the full costs and benefits of technology adoption 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 consumption and caloric intake.

We first present pure experimental (intent-to-treat) estimates in Table 3 with consumption measures regressed on the randomly assigned treatments: cash, credit and information for migration. Our regressions take the form

$$Y_{ivj} = \alpha + \beta_1 Cash_{ivj} + \beta_2 Credit_{ivj} + \beta_3 Information_{ivj} + \varphi_j + \upsilon_{ivj}$$

where Y_{ivj} is per capita consumption (money spent on food, non-food, total calories, protein, meat, education, etc in turn) for household i in village v in sub-district j in 2008, and , φ_j are fixed effects for sub-districts. Standard errors are clustered by village, which was the unit of randomization (and this will be true for all our analysis). The first three columns in Table 3 show $\hat{\beta}_1, \hat{\beta}_2$ and $\hat{\beta}_3$ – the coefficients on cash, credit and information – and each row represents a different regression on a different dependent variable. The dependent variables are household averages using the set of people reported to be living in the household for at least 7 days at the time of the survey as the denominator. We discuss the appropriate choice of denominator in more detail below.

Both the cash and credit treatments – which induced 21-24% more migration – result in statistically significant increases in food and non-food consumption. Consumption of food and non-food items increased by about 97 Taka per

household member per month in the 'cash' villages, which represents about a 10% increase over consumption in the control group. The increase in credit villages was 8%. The information treatment, which did not induce any additional migration, does not result in any significant increases in consumption. Calories per person per day increase by 106 under the 'cash' treatment, and consumption of protein increases significantly, especially from meat and fish. For the Bangladesh context, this reflects a shift towards a higher quality diet, as meat and fish are considered more attractive, "tasty" sources of protein. Educational expenditures on children also increase significantly.

Since both cash and credit treatments led to greater migration (Table 2), column 4 reports the intent-to-treat estimates for these two incentive treatments jointly. Average monthly household consumption increases by 68 Taka in these incentive villages (7% over control group), and this results in 142 extra calories per person per day. Column 5 indicates that these effects are generally robust to adding some controls for baseline characteristics.

Next we show the local average treatment effect (LATE), the consumption effect of migration for those households that were induced to migrate by our intervention. This is a well-defined and policy relevant parameter in our setting: programs providing credit for migration and even incentivizing migration seem to be of direct policy interest, and we think it unlikely that any households were dissuaded from migrating by our incentive. We calculate this effect by estimating:

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

where $Migrant_{ivj}$ is a binary variable equal to 1 if at least one member of household migrated during Monga in 2008 and 0 otherwise, and X_{ivj} is a vector of household characteristics at baseline that we sometime control for. The endogenous choice to migrate is instrumented with whether or not a household was randomly placed in the incentive group:

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

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. First stage results in Appendix Table 1 verify that the random assignments to cash or credit treatments are powerful predictors of the decision to migrate.

The intervention may have changed not only households' propensity to migrate on the extensive margin, but also who within the household migrates, how long they travel, the number of migration episodes on the intensive margin. Such changes may affect the interpretation of the IV estimates. Appendix Table 2 shows that the treatment does not significantly alter whether the household sends a male or female migrant, or the number of trips per migrant, or the number of migrants or trips per household (on the intensive margin, conditional on someone in the household migrating once). The effects are concentrated on the extensive margin, inducing migration among households who were previously not migrating at all.[15] However, the treatment does make it more likely that older, heads of households become more likely to migrate.

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 (income has a negative coefficient in the first stage regression in Appendix Table 1). 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. This is very similar to the 36% consumption gains from migration estimated by Beegle et al (2011) for Tanzania. Finally, none of the results discussed above are sensitive to changes in baseline control variables.

---

[15] The migrant is almost always male (97%), and often the household head (84% in treatment villages and 76% in control), who is often 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.

In terms of magnitude of effects, monthly consumption 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 home (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 to the consumption survey on transportation costs.

It is not straightforward to evaluate the returns to migration based on these estimates, and the precise value will depend on assumptions about the period over which the consumption gains are realized, and how to treat the cost that some migrants choose to incur to return home and take a second trip. Under a reasonable assumption that the consumption gains are realized over the 2 months of the monga period, households consume an extra Tk. 2840 (Tk. 355 per capita per month estimated in Table 3 * 4 household members * 2 months) during the monga by incurring a migration costs of Tk. 1038 (Tk.600/trip*1.73 trips). This implies a gross return of 273%, ignoring any disutility from separation.

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). Table 4 shows that 2009 effects are about 60-75% as large as the consumption effects in 2008 across both ITT and LATE specifications, but still statistically significant. Migration is associated with a 28% increase in total

household consumption which is still substantial. The LATE specification for 2009 is more difficult to interpret: many of those induced to migrate in 2008 were induced to re-migrate a year later, but they could have also re-invested their 2008 earnings in other ways that leads to long-run consumption gains.

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). These results are shown in Appendix Table 3. 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. We compute this household size based on different questions in the survey ("who currently lives in the household" as opposed to "who is present on the interview date"). Both ITT and IV results remain statistically significant, but slightly smaller (e.g. 130 or 125 calories rather than 142) in some specifications. Finally, even with the very conservative assumption that migrants never left, migration is estimated to increase consumption by 1169 calories per household (or 292 calories per person) per day in the IV or 194 calories per household per day in the ITT. However, this last result, shown in panel E, is no longer statistically significant.

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. Information on earnings and savings at the destination were only collected from migrants (non-migrants skipped over this section of the survey), and these are not experimental estimates; they merely help to calibrate the consumption results. Table 5 shows that migrants

in the treatment group earn about $105 (7451 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 that becomes available for consumption at the origin. The destination data suggest that this amount is about $66 (4600 Taka) for the season. 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.

We can compute experimental (ITT) estimates on total income (and savings), by aggregating across all income sources at the origin and the destination. Income is notoriously difficult to measure in these settings, with income realized from various sources – agricultural wages, crop income, livestock income, enterprise profits – parts of which are derived from self-employment or family employment where a financial transaction may not have occurred. Appendix Table 4 shows ITT and IV estimates. Households in the treatment group have 585 extra Taka in earnings, and hold 592 extra Taka in savings. In the IV specification, migration is associated with 3300 extra Taka in earnings and savings. We also examine effects on an anthropometric measure we collected – each child's middle-upper-arm-circumference (MUAC). The IV specifications suggest that migrants' children's MUAC grew an extra 5-11 mm, but the result is not statistically significant. MUAC was measured in December 2008, soon after the initial inducement to migrate.

Table 6 is a purely descriptive table that 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. Observed average migrant earnings at the destination (100 Taka per day) 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 entrepreneurs at the origin (61 Taka per day). There is heterogeneity around that average, which introduces some risk, and we will discuss this in Section 6.

5 Theory

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 the induced 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 continued to send a seasonal migrant in subsequent years. Given the first two facts, our model incorporates both risk aversion and a credit constraint. Furthermore, any attempt to identify the frictions that prevent households from engaging in an apparently beneficial activity will have to confront the possibility that households could save up to migrate. We therefore allow for savings, both for migration and to buffer against income shocks.

We first use the model to frame a deeper discussion of the data in Section 6. We will show several patterns in the data are qualitatively consistent with our simple framework. Second, Section 7 will ask whether the model can make sense of the data, quantitatively. To do this, we calibrate the model and then ask how risk averse a potential migrant would have to be for our model to generate our experimental results.

5.1 Baseline Model

We consider the migration and consumption choices of an infinitely lived household in discrete time. In each time period, a state of the world $s \in S$ is drawn according to the distribution μ and the household receives income y_s.[16] We refer to this as background income and assume the process is *iid*.[17] A household that enters the period with assets A and receives background income y has cash on hand $x = A+y$. We assume that the household can save at a gross interest rate R, but cannot borrow for consumption purposes.[18] Therefore, consumption is less than cash on hand ($c \leq x$) in any period.

The household is uncertain about whether it will be good at migrating. With probability π_G the household is type G – good at migrating – and receives a

---

[16] We assume that all households face the same distribution of background income. This is a strong simplifying assumption. In practice there are likely to be poorer and wealthier households. Our model suggests that those that are very poor will not migrate because it is too risky. Those that are very rich will likely not migrate because they do not need to supplement income and those that are in the middle migrate because they can afford to and benefit from doing so. This is consistent with a slightly altered version of the model presented here in which migration truncates the distribution of earning from below. We have explored this alternative model, but find that it leads to similar quantitative results. We do not pursue this approach in the main text as the model is more complicated – because cash on hand is not a sufficient state variable it is also more computationally expensive to use for simulations.

[17] See Deaton (1991) for a discussion of the impact of relaxing this assumption. We think it is a reasonable assumption in our setting and maintain it throughout.

[18] Households have access to microfinance from a range of sources, however, we believe limitations on microfinance borrowing imply that we should think of these households as credit constrained. First, most lending is specifically for women and specifically for entrepreneurial activity. To the extent these requirement are binding, microfinance is not useful for consumption smoothing or migration. Second, typical credit contracts require borrowing on a set loan schedule and require immediate repayment. Again, this means microfinance is very hard to use for smoothing or migration.

positive (net) return to migrating of *m*. With probability $(1 - \pi_G)$ the household is type *B* – bad at migrating – and receives no return to migrating, but faces a cost *F* if it does choose to migrate. We think of type as being a household specific parameter, and not something that can be easily learned or transferred over from other households in the village. We further assume that this uncertainty resolves after one period of experimentation with migration. Migration is, therefore, to be thought of as an experience good.[19] This assumption is motivated by reports that migrants need to find a potential employer at the destination and convince that employer to trust them. Once this link is established it is permanent, but some migrants will not be able to form such a link. A leading example from our data is convincing the owner of a rickshaw that you can be trusted with his valuable asset. Below, we discuss further reasons for modeling risk in this way.

A household that knows it is bad at migrating will never migrate and is essentially a Deaton (1991) buffer stock saver. With cash on hand *x*, such a household solves

$$B(x) = \max_{c \leq x} \left[u(c) + \delta \int_S B\big(y_S + R(x - c)\big)d\mu(s) \right],$$

where *u* is a standard strictly increasing, strictly concave utility function and δ is the household's discount factor. A household that knows it is good at migrating will always migrate and solves a similar problem, but with a higher income. With cash on hand *x* a household that is a good migrator has value

$$G(x) = \max_{c \leq x+m} \left[u(c) + \delta \int_S G\big(y_S + R(x + m - c)\big)d\mu(s) \right].$$

With this formulation we are assuming that the household can migrate before it

---

[19] We thank an anonymous referee for clarification on this point and also the term experience good.

makes its consumption decision, this means that a households that knows that it is a good migrator can always migrate regardless of credit constraints.

We are interested in the behavior of a household that has never migrated before. In each period, such a household chooses both whether to migrate and consumption/savings. If it migrates it discovers that it is a good migrator with probability π_G and has value $G(x)$. If, however, the household migrates and discovers that it is a bad migrator, then it has paid a cost F and receives value $B(x-F)$. We think of π_G as the probability of finding a connection at the destination within a reasonable search time. We think of the cost F as being the cost of transport and lost income while the migrator searches for work. The household will choose to migrate if the expected utility of migration is greater than that of not migrating. Therefore, a household that has never migrated before, and has cash on hand x, solves

$$V(x) = max \left\{ \underset{c \leq x}{max} \left[u(c) + \delta \int_S V(y_s + R(x - c)) d\mu(s) \right], \pi_G G(x) \right.$$
$$\left. + (1 - \pi_G) B(x - F) \right\}.$$

Migration is risky in this model. A household that turns out to be a bad migrator pays a cost F but receives no benefit. This has two implications. First, the household is credit constrained and will have to forego consumption in the current period. Second, the household may face a bad shock in the next period, but will have no buffer stock saving to smooth consumption. Hence, the model has a role for background risk which, given the assumptions we make about the utility function, implies that the riskier the background income process, the less likely is migration for any particular level of cash on hand.[20]

---

[20] See Eeckhoudt et al. (1996) and the literature cited there for a discussion of when background risk leads to a reduction in risk taking.

Throughout our discussion we assume that the household faces a subsistence constraint. We model this by assuming that $u(c) = \tilde{u}(c - s)$ with $\lim_{x \to 0} \tilde{u}'(x) = \infty, \lim_{x \to 0} \tilde{u}(x) = -\infty$, and $\lim_{x \to 0} \frac{\tilde{u}''(x)}{\tilde{u}'(x)} = \infty$. That is, there is a level of consumption s at which the household is unwilling to consider decreasing consumption for any reason, and the household becomes infinitely risk averse. We think of s as a point at which survival requires the household to spend all its current resources on food, with the implication that household members face a threat of serious illness or death if they do not consume at least s. The possibility that consumption is close to this point in our data is highlighted by the fact that the monga famine regularly claims lives. We also show below that many households' expenditure seems to fall below what would be required for a minimal subsistence diet. We believe it reasonable to assume that a household that has such a low consumption level would not be willing to take on any risk. For our simulations we use a fairly standard utility function that incorporates a subsistence point: $u(c) = \frac{(c-s)^{1-\sigma}}{1-\sigma}$.

The model is related to Deaton's buffer stock model, several models from the poverty trap literature (e.g. Banerjee, 2004), and the entrepreneurship literature (e.g. Buera, 2009; Vereshchagina & Hopenhayn, 2009). We now describe the behavior of agents in this model using the value functions, policy functions and simulated time series of choices. Figure 3 provides plots of two value functions, both for households that have never migrated before. The first function shows the value to a household that is forced to migrate in this period:

$$V_M(x) = \pi_G G(x) + (1 - \pi_G)B(x - F).$$

The second function shows the value to a household that decides not to migrate in this period:

$$V_N(x) = \max_{c \leq x} \left[u(c) + \delta \int_S V(y_s + R(x - c)) d\mu(s) \right].$$

As is generally the case, V_M crosses V_N once from below. This implies a cutoff level of cash on hand $\tilde{x}$: for cash on hand below $\tilde{x}$ the household does not migrate, for cash on hand greater than $\tilde{x}$ the household does migrate. Because the two value functions cross, the value V is not convex, which implies that the household would be risk loving at levels of cash on hand close to $\tilde{x}$. We do not allow households any kind of randomization that would help them take advantage of this non-convexity – this is a feature of most poverty trap models. These issues are explored in detail in Vereshchagina and Hopenhayn (2009).

Our cash incentive treatment is easy to incorporate into the model: the payment increases cash on hand by 600 Taka in either the good or bad state of the world. This has the effect of moving V_M up, lowering $\tilde{x}$ to $\tilde{x}'$. Those households that had cash on hand in the interval $[\tilde{x}', \tilde{x}]$ are induced to migrate. Other interventions and policy prescriptions can be analyzed in a similar fashion.

Figure 4 displays typical policy functions – consumption as a function of cash on hand – for the model. The first policy function shows consumption for a household that knows it is bad at migrating (c_B), and the second for a household that has never migrated, but that we restrict to not migrate in the current period (c_M). At low levels of cash on hand, both policy functions lie on the 45 degree line – the household spends all that it can. As cash on hand rises, the household that knows it is a bad migrator begins to buffer, consuming less than cash on hand and saving some money to smooth later consumption. This is the standard result following Deaton (1991). Initially, the household that can migrate does the same thing and the two policy functions lie on top of each other. As cash on hand approaches $\tilde{x}$, however, c_M falls below c_B: the household that can migrate begins to

save up for migration. Thus, the saving of a potential migrator can be divided into two parts: buffering, and saving up for migration. The figure shows that, for some parameter values, consumption is not a monotone function of cash on hand, a result that is consistent with the findings of Buera (2009). As cash on hand rises past $\tilde{x}$, c_M continues to lie below c_B: we have constrained the household not to migrate in this period so it continues to save in the hope of migrating next period. Finally, there is a level of cash on hand past which $c_M > c_B$ – the household that has never migrated knows that it can migrate next period and it is consequently richer (in expectation) than the household that knows it is bad at migrating.

We are not interested in general results as $t \to \infty$ but rather in the behavior over real world time periods. This behavior is inherently stochastic and best understood by looking at simulations. Figure 5 shows simulations of cash on hand and consumption for two households with different starting levels of cash on hand (wealth). Both households are assumed to be good migrators. The panel on the left shows cash on hand and the right shows consumption. The cash on hand simulation shows that the wealthier household quickly saves enough to cross the migration threshold, $\tilde{x}$. After crossing the threshold, cash on hand spikes as the household discovers that it is a good migrator. The poorer household never migrates. The consumption simulations shows that the wealthier households consumes less initially – as it saves up – but after crossing the migration threshold has a higher consumption level. In general, our simulations show that households with a lower mean income $(E_\mu y)$ or with a lower starting cash on hand are less likely to cross the threshold for any finite time period, indicating a kind of poverty trap. It is this poverty trap that can potentially explain our experimental results: a portion of households are stuck in a low income situation in which they cannot migrate, but a small intervention can push them to experiment with migration, with potentially high returns.

We can also use the model to consider other comparative statics. Risk aversion appears intuitively linked to aversion to experimentation, but the model suggests that the relationship is more complicated. Simulations show that an increase in risk aversion has three effects. First, increasing risk aversion increases the cost of experimenting with migration and tends to increase $\tilde{x}$ and thus reduce the propensity to migrate. Second, as risk aversion increases, the return to migration increases because migration can be seen as a risk mitigation strategy. Third, for many utility functions (including the one we use for simulations), absolute prudence increases with risk aversion.[21] As a consequence, as risk aversion increases the household engages in more buffer stock saving, implying that the household is more likely to cross any given threshold level of cash on hand. We have not sought a general characterization of which effect dominates, but do observe all three effects in our simulations. Similar effects apply to an increase in the riskiness of income. On the one hand a riskier income means more background risk and, therefore (for specific utility functions) effectively an increase in risk aversion. On the other hand, more risk means more buffer stock savings.

6 Qualitative Evaluation of the Model's Assumptions and Central Implications

In this section we provide some descriptive and some experimental evidence in favor of the main assumptions and implications of the model.

6.1 Descriptive Evidence on Income Variability and Buffering

A key assumption of the model is that the income process is stochastic. To verify whether this describes our setting, we study the inter-temporal variability in the

---

[21] The coefficient of absolute prudence is defined as $\frac{u'''(x)}{u''(x)}$. See Kimball (1990) for a definition of prudence and the relationship to precautionary savings and concepts of risk aversion including decreasing absolute risk aversion.

three rounds of consumption data collected at baseline (July 2008), December 2008 and December 2009. We conservatively use consumption data rather than income data because income is measured with more error in these settings (Deaton, 1997) and this would artificially inflate variability, and because income is more variable due to seasonality and consumption smoothing.

Even with the conservative measure, we see that average variability in per-capita consumption is high. Mean absolute deviation in weekly consumption in our sample is 307 Taka between rounds one and two and 368 Taka between rounds two and three. The standard deviation of the absolute deviation in income is 635 and 508 Taka respectively. By way of comparison, average per-capita consumption levels in the control group were 1067, 954 and 1227 Taka in the three surveys. In Appendix Figure 1 we plot histograms of second round consumption separately for each of the 10 deciles of first round household consumption. Visual inspection suggests that there is no real permanence in the income distribution - those that were in the lowest decile in the first round do not appear to have a significantly different draw in the second period from those that were in the middle decile. We verify this by regressing consumption in later rounds on in earlier rounds consumption in Appendix Table 5. Every extra dollar of consumption measured in July 2008 is associated with only 10.2 cents extra consumption in December 2008, and 6.7 cents in December 2009. One dollar extra in December 2008 is associated with 45 cents more consumption in December 2009. The R-squared in these regressions are between 0.02-0.13: current consumption does not predict future consumption well. Although measurement error is probably very important in explaining these results, we think it is reasonable to conclude that background income is also very variable.

The reported yearly variation in income and consumption dwarfs the size of our 600 Taka incentive, and thereby poses a significant challenge to the model's ability to rationalize the data. Our model suggests a cutoff point of cash on hand

that would trigger migration. Our incentive presumably works, in part, by increasing cash on hand. But, the data suggests that income (and, therefore, cash on hand) will be higher by the size of the incentive regularly, just by pure chance. This fact is primarily why we do not think that a pure liquidity constraint – the complete inability to raise the bus fare – provides a good description of the setting. We return to this issue below.

Background risk also has important implications for behavior. If households are prudent (i.e. $u''' > 0$) and impatient ($\delta > R$), both of which seem likely in our setting,[22] then high income-variability should lead to buffer stock savings. Appendix Table 6 describes savings behavior in our sample. Although our households are poor, they have a reasonably high level of savings. Conditional on being a saver, the mean holding in cash is 1400 Taka, which is about 35% of monthly expenditure for the household. This is a relatively high savings/expenditure ratio, even compared to the United States. For the full sample (not conditioning on people with positive savings), average cash savings is 745 Taka, and average value of cash plus other liquid assets (e.g. jewelry and financial assets) held by all households is 1085 Taka. This level of savings is not inconsistent with the observation that households in our sample are often close to subsistence. Buffering implies that in each period some households will have zero savings and be consuming hand to mouth, but those same households will have high savings in other periods. Indeed, the data bears this out quite well. 53% of households held cash savings at baseline, and this fraction varies a lot across rounds (57% in December 2008 and 34% in June 2011). The share of households holding liquid assets varies from 42% to 59% to 81%. The standard deviation of savings is also about two times mean savings which is consistent with savings being variable, as it would be in a buffer stock model.

---

[22] The existence of savings constraints in developing countries (Dupas & Robinson, 2013a) makes $\delta > R$ reasonable. There are by now many theoretical and empirical arguments suggesting that prudence is a reasonable assumption for the utility function.

6.2 Descriptive and Experimental Evidence on Migration Risk

Our model assumes both that migration is risky, and that risk takes a particular form: risk is assumed to be idiosyncratic. We begin by discussing evidence on migration risk, and will turn to the specific form of the risk in Sections 6.3 and 6.4.

Figure 6 provides a clear depiction of the migration risk. We take the monthly consumption per household member in December 2008, and subtract the value of the incentive from households that chose to take it. This gives a measure of the possible outcome if the cost of migration had to be born within one month by the household, not subsidized by our incentive program. In panel A, we subtract the histogram for distribution of consumption in the control (non-incentive) villages from this histogram for the distribution of consumption in the treatment (incentive) villages, less the value of the migration incentive paid out. The results show significant amounts of risk: while the treatment moved many poor households from extreme poverty (consuming 500-900 Taka per month) to a less poor (1300 Taka per month) category, many other households would shift to 100-300 Taka per month (which, as discussed below, corresponds to caloric intake at or below subsistence) without the payment to migrate. Panel B shows that the risk disappears when we account for the program's migration incentive payment for those who took the money. This suggests that households at greatest risk were the ones induced to migrate by our incentive, a result we will explore more precisely below by creating a measure of subsistence.

6.2.1 Experiments on Migration Insurance

Motivated by our first two years of findings and the model, we also designed a new experiment to directly test whether households perceive migration to be risky. We returned to our sample villages in 2011 and offered a new set of treatments. Appendix 2 describes the sampling frame and intervention design. To study risk,

the specific treatment was to offer a 800 Taka loan up-front conditional on migration, but the loan repayment requirement is explicitly conditional 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 skilled wage work at outdoor construction sites (e.g. breaking bricks), it both increases the cost of pulling rickshaws and lower the demand for a rickshaw transport. In terms of the model above, we think of high rainfall as reducing the likelihood of finding a connection at the destination (because job opportunities that allow you to display your skills to a potential employer are scarce), as well as reducing the return to migration, *m*.

Appendix 3 develops a simple model of index insurance with basis risk to clarify how this treatment is linked to household perceptions of migration risk. Following Clarke (2011), we formalize basis risk as the probability that income is low, but that rainfall is also low, so that the insurance does not pay out. In terms of the above model, this would be the event of not finding a job connection during your search (i.e. finding out you are a bad migrator) but still being forced to repay the loan. Appendix 3 shows that our formalization implies that the portion of people induced to migrate by the index insurance is decreasing in basis risk, if and only if migration is risky and households are risky averse. We assume that households that migrate to Bogra face lower basis risk, and farmers, for whom high rainfall is usually beneficial, face greater basis risk.[23]

Table 7 shows results of regressing the 2011 migration rate on our 2011 treatments, and interactions of the insurance treatment with an indicator for previous migration to Bogra, and an indicator for farmers. Column (1) shows that the rainfall insurance contract induced migration, and that the effect is similar in size as the effect of the simple (conditional) credit contract. Columns (2) and (3)

---

[23] We use the basis risk variation to test for riskiness because our insurance is valuable even without risk, because also includes a credit element.

show that those with a propensity to travel to Bogra (i.e. lower basis risk) are more affected by the insurance, while farmers (greater basis risk) find the insurance contract less appealing. The farmer-insurance interaction is statistically significant with 99% confidence, but the Bogra interaction is not significant at conventional levels. Finally, column (4) shows that when we control for farmers, the Bogra effect is much stronger (*p*-value of 0.15). For non-farming households who had a preference for Bogra, the rainfall insurance contract induces 45% more migration in 2011. We see this set of results as reasonable strong evidence in favor of our assumption that migration is risky, and households behave as though they are risk averse.

6.3 Learning and Idiosyncratic Risk

Our model makes the assumption that migration risk takes a specific form: that it is individual-specific (idiosyncractic), and resolved after one period of migration (i.e, there is something to learn, or a connection to make.). Our motivation for making this assumption is the strong and consistent repeat migration seen in the data – half of all induced migrants migrate again, and this number is stable over 3 years. This result is very hard to drive without learning or accumulation of a connection. Even if households earn a very large return on the investment F, the impact will dissipate quickly because of the variability in base income.

6.3.1 Is Risk Idiosyncratic in this Setting?

We first examine whether migration risk is idiosyncratic, and try to identify the nature of the risk from our data, before turning to evidence on learning. Our information intervention – which provided general information on wages and the likelihood of finding a job – has a precisely estimated zero impact on migration rates. This is consistent with the assumption that risk is idiosyncratic, but may also reflect the fact that this kind of information is not credible.

We next examine the determinants of 2009 re-migration to study directly whether households are able to learn from others. As discussed above, our 2008 experiments contained several sub-treatments where additional conditions were imposed: some households were required to migrate to specific destinations, some were required to form groups, etc. This variation is within village and implies that we have exogenous variation in the number of a household's friends that migrated. We also collected data at baseline on social relationships between all our sample households to identify friends and relatives within the village. To test for learning we run regressions of the form

$$y_i = \alpha + \beta M_i + \gamma F_i + \epsilon_i$$

where y_i is an indicator for second round migration, M_i is an indicator for first round migration and F_i is a measure of how many of a household's friends migrated. We instrument M_i and F_i with all our treatments (incentives and conditions on the migrant, and incentives and conditions on his friends), and report OLS and IV results in Table 8. If there is learning from others we expect to see $\hat{\gamma} > 0$, because of the strong positive returns to migration. Table 8 shows strong persistence in own migration: that inducing migration in 2008 with the randomized treatments leads those same induced migrants to re-migrate in 2009. However, friends' migration choices the previous year have no impact on 2009 migration decisions, and this is a reasonably precisely estimated zero effect. This suggests that people learn from their own experience, but do not learn from the experiences of others. This provides strong support for the assumption that risk is idiosyncratic as implied by the model.

Why is learning so individual-specific? The 2011 follow-up survey provides a strong hint: Of the 2011 migrants provided incentives in 2008, 60% report going back to work for the same employer at the same destination. Appendix Table 7 shows that being treated in 2008 leads to a 5 percentage point greater likelihood of re-migrating and working for the same 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? The typical employer in Dhaka is a rickshaw garage owner who has to trust a migrant with his valuable asset. Research in India has documented that migrants sometimes abandon the rented rickshaws at the train or bus station (Jain & Sood, 2012). This would make it difficult for migrants to "learn" from other villagers to resolve the uncertainty.[24]

Furthermore, migrants who were provided incentives in 2008 and 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 16% more likely to report forming the job connection to their current (2011) employer in 2008 instead of 2007, relative to "regular" migrants in the control group.[25] 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.

Finally, among households that migrated in 2008 (in both incentive and control groups), we asked whether these households knew someone at the destination, or whether they had a job lead at the destination. These measures can be thought of as proxies for whether the household's type has been revealed – households that have a connection have already determined their status while those that do not have not, or know that they are bad at migrating.[26] Our model implies that the incentive will only have an impact on those that do not know their

---

[24] Friends and relatives could potentially vouch for each other with employers, but this need not be believed. Further, making such a referral could be quite costly, it may put the referrers own job in danger, or require the referrer to look after a new migrant, perhaps providing some risk sharing and sharing housing.

[25] Appendix Table 8 shows the results of the t-tests. Results are statistically significant at conventional levels for the difference tests (e.g. 2007 vs 2008), but not for the difference-in-difference (e.g. 2007 vs 2008, treatment vs control) tests.

[26] According to our model, those that have migrated and know they are bad should not be in this sample that is entirely made up of migrators.

status and so we expect to see more migrators without a connection in the incentive group. Table 9 shows that migrants in the control group are much more likely to know someone at the destination, and to have a job lead, than are those in the incentive treatment. This suggests that our treatment induced migrants among those that had not already determined their status, as implied by the model.

6.3.2 Evidence on Learning

The fact that learning should be destination specific – a connection in Dhaka, for example, is not useful when migrating to Bogra – allows us to test more directly for learning effects using experimental variation induced by our treatments. One of our treatments assigned a specific destination city (Bogra, Dhaka, Munshigonj or Tangail) as a condition of receiving the migration incentive, and creates exogenous variation in the destination choices in 2008. Learning or creating a job connection implies that migrants assigned to a specific location should be more likely to return to that particular location in 2009 than to any other.

Let D_i be an indicator taking value 1 if household i migrated to destination D in 2009, and D_i^{08} be an indicator taking value 1 if household i migrated to destination D in 2008. We run regressions of the form

$$D_i = \alpha + \beta D_i^{08} + \epsilon_i$$

for each of four destinations. The 2008 migration destination choice is instrumented with the location randomly assigned to the household:

$$D_i^{08} = \lambda + \rho T D_i^{08} + \eta_i,$$

where TD_i^{08} is an indicator taking on value one if the households was assigned to location D in 2008. Appendix Table 9 shows these first stage estimates to establish that initial destination assignment had a strong effect on destination choices in 2008. The hypothesis of destination specific learning implies that there should be more than one significant coefficient in the second stage estimates displayed in

Table 10.[27]   We see that all coefficients, instrumented with our location requirements, are positive and that two are significant at the 10% level (Dhaka and Munshigonj). The coefficients also imply quantitatively important stickiness. Households randomly assigned to migrate to Munshigonj in 2008 are 30% more likely to re-migrate to Munshigonj in 2009 than to any other location. We take this as evidence in favour of location specific learning or the accumulation of connections at the destination as being an important driver of migration behavior.

Our model also suggests that some induced migrants should discover that they are bad migrators, while some discover that they are good. Among regular migrants, however, our model predicts no such effects – only households that know they are good at migrating should migrate in the control group. Figure 7 shows evidence consistent with this. In the treatment groups (credit or cash) those that chose to re-migrate in 2009 had a significantly better migration experience in 2008 than those who chose not to re-migrate. In the control group, however, we see no such effect.

6.4 Subsistence

Our model postulates that households may not migrate because they are close to subsistence, and risk falling below subsistence if they have a bad migration outcome. We can study the distribution of expenditures and caloric intake to examine whether this setup is warranted.

The Bangladesh Bureau of Statistics classifies a person as ultra-poor if they consume less than 1605 calories, and it is usually thought that something between

---

[27] There may be inherent differences in profitability of each location, and just showing that those assigned to migrate to Dhaka are more likely than others to re-migrate to Dhaka is consistent with Dhaka simply being the most profitable place to migrate, and re-migration simply reflecting initial success. We overcome this issue by observing that only one destination can be the most profitable, and examining re-migration propensities for all 4 of our assigned destination. We will need to show that migration assignment leads to destination-specific re-migration to at least two different cities. Note that location specific learning does not imply that all regressions would have positive coefficients -- some locations may just be really bad placed to migrate.

600 and 1000 calories are required just to survive. Based on the prices collected in our baseline survey, and assuming very basic calorie composition, we estimate that it would cost about 660 Taka per person per month to meet the ultra-poor level, 450 taka to consume 1000 calories and 250 Taka to consume 600 calories. Comparing these figures to the distribution of per-capita expenditures in our sample presented in Appendix Figure 1, we see that a substantial portion of households are close to subsistence. Appendix Figure 2 shows directly the histogram of calories per person per day in the control group in our December 2008 follow-up. Many households in the control group can be characterized as "close to subsistence" in terms of caloric intake. Comparing the treatment and control histograms, we again see that our treatment moved many people from a subsistence level of consumption (of 800-1300 calories per person per day) to a comfortable level exceeding 2000 calories per person per day.

Our model suggests that if aversion to the risk of falling below subsistence is an important deterrent to migration, then: (a) people close to subsistence should not be migrating in the control group, and (b) our treatment should have the largest effect on households that are close to subsistence: they should be the ones induced to migrate by our incentive. The three panels in Figure 8 show strong evidence in favor of these two claims graphically and in a regression. We measure subsistence as the proportion of total household expenditures devoted to food. The regression and the graphs show that those closer to subsistence are significantly less likely to migrate in the control group, and their migration decisions respond most strongly to the treatment.

6.5   Does the Model Rationalize Responses to all Treatments?

The model allows us to understand the impact of specific treatments designed to help households accumulate sufficient cash on hand to engage in profitable

migration. In this section we compare the impacts of several potential policies on which we have collected data.

First, as noted above our initial treatments included both a cash and a credit incentive. In practice these two incentives have approximately the same impact on the migration rate. Here we argue that this finding is consistent with the model, if credit is seen as incorporating a limited liability aspect. An assumption of limited liability is consistent with the fact that only 80% of households repaid the loan.

We can capture the limited liability effect of credit by noting that households have to have a reason to repay their loans. Let $M(x) = B(x - F)$ if the household is a bad migrator a $M(x) = G(x)$ if the houshold is a good migrator, and consider a household that has a loan of value L and is required to repay Z. The household will repay the loan iff

$$M(x - Z) \geq M(x + L) - P,$$

where P is a utility cost of punishment by the lender. P is assumed to be state independent as the punishment should reflect the long run value of credit to the household. With this formulation there is $\hat{x}^B$ and $\hat{x}^G$ such that the loan will be repaid by a bad migrator if $x \geq \hat{x}^B$ and by a good migrator if $x \geq \hat{x}^G$. It is easy to show that $\hat{x}^B < \hat{x}^G$ and that a bad migrator will always default on the loan if the cost of migration, F, is large enough. Further, because utility becomes infinitely negative when consumption approaches subsistence, to a first approximation, $M(\hat{x}^B + L) \approx M(\hat{x}^B + L) - P$ for a bad migrator for whom $x - F$ is close to subsistence. This implies that the utility cost P need not have a large impact on the ability of the credit contract to provide insurance.

To incorporate these observations into the above model, assume that $\hat{x}^B < \tilde{x} < \hat{x}^G$. This assumption implies that a first time migrator that discovers it is a bad migrator will not repay the loan, but a first time migrator that is good, will

repay the loan. As discussed above, the decision to migrate is determined by the intersection between the curves V^M and V^N, if $B'(\tilde{x})$ is large and positive and $G'(\tilde{x})$ is small, then the impact of credit and cash will be, approximately, the same. Consequently, the model can capture the idea that credit and cash will have the same impact. Obviously from a policy perspective, credit is a much less costly intervention. Interpreted in the light of this analysis, the fact that credit and cash treatments have a similar impact on migration rates again suggests that risk, which is mitigated by the limited liability aspect of the loan, is important in explaining our data.

Second, as noted above, we returned in 2011 and implemented new treatments. One of these treatments was an unconditional credit contract of the same size at the conditional credit transfer. Our motivation for this experiment was to rule out the possibility that households were merely cash constrained. Our model implies that the credit incentive should have a larger impact as it moves only the V^M curve, while the unconditional credit raises the V^N curve as well. This is an implication of any model in which a household weighs the returns to migration relative to other possible uses of the money, but is not an implication of a model where the household knows that migration is profitable, but simply cannot afford it. The results of this experiment are shown in Table 7, and show that, consistent with our model but inconsistent with the cash constraint model, the unconditional transfer has a smaller impact than the conditional transfer.[28]

6.6 Summary of Qualitative Tests

In summary, both descriptive and experimental analyses of the data indicate that our model accurately captures many key aspects of the environment: background income is volatile, migration is risky, savings is high and migration is

---

[28] Although we presented the products in a similar way, if household perceptions of repayment requirements varied between the conditional and unconditional loans, that may also lead to differential take-up.

an experience good. The model also rationalizes most of the data coming from our experiment: the fact that credit and cash have similar sized impacts, the fact that the incentive was most effective for those that are close to subsistence, the relative impacts of unconditional and conditional transfers, and the response to the insurance treatment. What remains to be seen, however, is whether the model brings all of these ingredients of the migration decision together in a way that can quantitatively account for the magnitude of the experimental effects.

7   Quantitative Calibration of the Model

Our quantitative exercise will use the data to calibrate all the free parameters of the model except risk aversion. We then ask what level of risk aversion would be required to match key aspects of the data. Table 11 shows the parameters we use for the quantitative exercise. In all cases we have erred on the side of allowing the model to generate the experimental estimates. This choice reflects the fact that we will ultimately argue that the model in its basic form is not able to rationalize the experimental estimates.

Three choices deserve special mention. First, we assume that there are two opportunities to migrate each year (or two time periods per year): one after each planting season. This means that a time period for the purpose of the model should be thought of as half a year. Second, we assume that the cost of migration, F, must be borne over 1 month, so that consumption when migration is bad is very low. This reflects the fact that most households earn money during the monga season and use it to pay for consumption. Credit constrained households will have to pay for migration out of this income. This choice is obviously quite extreme but could be justified by arguing that the extremely low consumption for a one-month period would have a large effect on utility relative to the remaining months in the monga period with a higher consumption level. Third, we assume that income at

home is distributed $N(700,70)$. This is an attempt to estimate the income distribution of the lowest 50% of households in the sample. We argue that the results of the full model are not sensitive to this choice.

We undertake two different exercises. First, we use the model to determine four cutoff points – $\tilde{x}, \tilde{x}_I, \tilde{x}_C$ and $\tilde{x}_{UCT}$ – the amount of cash on hand required to migrate with no intervention, with our cash incentive, with a credit incentive and with an unconditional cash transfer, respectively. We then match these levels of cash on hand to the histogram of consumption levels in the control group and ask what portion of the distribution lies between the relevant bounds to estimate the set of migrants that our treatments are predicted to induce. For example, we consider the density of households consuming between $\tilde{x}_I$ and $\tilde{x}$ to estimate the portion of households that would be induced to migrate by our incentive. This exercise essentially ignores the repeat migration effect and learning.

Our second exercise is to ask what portion of households can still be induced to migrate after t periods. A household is "induceable" in period t if it has never migrated before. In the model, only such households will be affected by our migration incentive, as other households will have already determined their status as good or bad migrators. For this exercise, we make use of the assumed background income distribution to determine the probability of a household crossing the migrating threshold $\tilde{x}$ in each period. If the number of induceable households is very low after only a small number of time periods, then the model cannot rationalize the experimental results.[29]

We undertake these two exercises under three different sets of assumptions. First, we consider a completely static model, where households do not save for migration and do not consider the benefits of ongoing migration when they make their initial migration choice – i.e. they are myopic past the current migration

---

[29] In fact, in all the results presented below we depart slightly from the above model and assume that households that migrate and are determined to be bad migrators are also induceable. This errs on the side of allowing the model to fit the data.

period. Figure 9 shows results for this static model: the left panel shows the portion of migrants that would be induced assuming no repeat migration and the right panel shows the number of induceable migrants as a function of the time period. Consider first the left panel. The model predicts that with a risk aversion level of $\sigma \approx 1.15$ the incentive would induce about 20% of households to migrate – consistent with our experimental findings. Further, the cash and credit incentives have the same effect, again consistent with our experimental findings. However, the UCT and incentive treatments have similar effects for low levels of risk aversion, and this is not consistent with our results.

The right panel shows that we need to assume a slightly higher risk aversion level to rationalize the data if we account for repeat migration. With a risk aversion level of about 1.65, 40% of the population are induceable after 8 seasons (or 4 years), which corresponds to a 20% treatment effect if the model applies to the poorest half of the sample. If we allow 10 prior years of migration activity, the model suggests that $\sigma \approx 1.7$ would be required to rationalize our treatment effect.[30]

For our second calibration, we continue to assume that there is no savings, but allow households to be forward looking. This has a strong impact on the propensity to migrate. The left panel of Figure 10 shows the results for the fraction of households induced to migrate by different treatments. Comparing this Figure to the left panel of Figure 9 shows that for low levels of risk aversion our incentive is actually better at inducing migration when we account for forward looking behavior. This is because, without the repeat migration effect our incentive does not induce all households to migrate. At higher levels of risk aversion this difference is no longer relevant and the repeat migration incentive leads to higher levels of baseline migration and a smaller impact of our incentive. The hump shape

---

[30] This result is very sensitive to the assumption about the distribution of background risk. If we have underestimated the background risk, then greater risk aversion would be needed to rationalize the data.

occurs because, as risk aversion increases, the value of migration as a risk mitigation activity increases. The figure shows that at some point this effect dominates the other impact of risk aversion, which is to make experimenting with migration less tolerable. The figure suggests that a risk aversion level of 1 is required to rationalize the data if we do not consider the repeat migration effect.

The right panel of Figure 10 shows the fraction of induceable migrants when households are forward looking, but cannot save up. The hump in the portion of induced migrants in the left panel implies that we need not consider risk aversion levels above about 7 – as σ increases past this point risk aversion in fact reduces the propensity to migrate.[31] After 4 years, 40% of the sample will be induceable if risk aversion is as high as 5. If we consider longer time horizons such as 10 years, then the figure implies that no level of risk aversion is high enough to allow for a large number of induceable migrants.

The results in Figure 10 may, however, overestimate the importance of migration. Because we do not allow savings, households are unable to buffer, and the value of migration as a risk mitigation strategy is increased. Figure 11 shows the results for the full model, where we allow for both buffer stock savings, and for the agent to save up for migration. The left panel confirms the intuition that savings reduces the value of migration. The right hand panel, however, shows that the ability to save up dominates: once we allow for savings we would need a risk aversion of 11.5 to replicate our treatment effects allowing 4 years of migration activity, and if we allow 10 years of migration activity, even a risk aversion level of 20 is insufficient to rationalize the results.[32]

---

[31] The hump in the left panel is based on the empirical distribution of consumption levels. For the simulations shown in the right panel we make use of our assumed distribution which leads to a maximal effect of the incentive at a risk aversion level of 7.

[32] These results assume that households begin time with no assets and the lowest possible income shock. We use the model to generate policy functions as well as cutoff values. We then simulate the model for 10,000 households and ask what portion of those 10000 households have not migrated after t periods.

The results allowing for savings suggest a sense in which our main conclusions are robust to our assumption regarding the distribution of income. We have simulated the right panel of Figure 11 for standard deviations in from 40 to 140 and the results are almost identical. As discussed above there are several impacts of increasing the degree of background risk, and the simulations suggest that, given our calibrations, these effects cancel each other out.

We can also use the full model to ask whether the observed level of savings is consistent with the model. For a risk aversion level of 0.5 the model predicts a household will hold, on average 1500 Taka in savings, which is roughly in line with what we see in the data. For higher levels of risk aversion, however, the model predicts far more savings than we observe: at a risk aversion level of 5, predicted average savings is close to 3000 Taka and at $\sigma = 10$ we predict savings of nearly 5000 Taka. It is not possible to match both the level of savings and the responses to the migration treatments at any given level of assumed risk aversion.

8   Extensions

While the qualitative evaluation of the model had shown that households do save, that they respond to migration incentives in ways predicted by the model, and that they perceive migration to be risky, the calibration exercise suggests that to match the magnitudes of responses and household behaviors, we have to extend the model in some ways. It could be that households under-estimate the benefits of migration, or they fail to actively save up for migration, or they are insufficiently forward-looking. In this section we discuss extensions to our baseline model that would allow us to better accommodate the data. In all cases, we do not have the data to determine conclusively which extensions are the most important. We therefore see this section more as an extended call for more work. We provide some suggestive evidence from our data of approaches that are unlikely to work.

In part, we offer a great number of possibilities to highlight the uncertainty and the need for additional experimentation before moving to policy prescriptions.

8.1 Alternative Specifications of the Returns to Migration

It seems clear that the migration process is risky, and m is likely stochastic even for good migrators. To assess the importance of this possibility, we re-simulated the model with the assumption that m was normally distributed around the mean of 550, with a standard deviation of 100. This additional risk does not appreciably alter the results presented in Figure 10 above.

We also explored a slightly different model, in which migration truncates the distribution of income below, rather than adding to it. We draw on Figure 6 to assume that migration truncates the distribution at around 1100 Taka per household member per month. This model does not perform very differently from our baseline model: it explains the data better if we ignore savings, but once savings is accounted for the results are similar.

8.2 Very High Levels of Risk Aversion

If we allow σ to be very high, then the model can rationalize most of the data. The literature has not arrived at a consensus on "reasonable" values for σ: Holt and Laury (2002) state that someone with $\sigma > 1.37$ should "stay in bed", while papers in the equity premium literature (e.g. Kandel & Stambaugh, 1991) argue that values as high as 30 may be reasonable. In our model, households are much more risk averse than implied by their σ because they become infinitely risk averse as consumption approaches the subsistence point. In circumstances analogous to ours, Chetty and Szeidl (2007) show that agents even in developed countries become more risk averse with commitments for consumption. In future research, it would be worth exploring at what point risk aversion might be considered to be

a "mistake" that a policy maker should seek to address. If extreme risk aversion is akin to a behavioral bias, then adding conditions to transfers may improve a migrator's utility over unconditional cash transfers.

8.3 Lowering the Discount Factor

Lowering the discount factor decreases both the willingness to save up for migration, and the extent to which future migration outcomes affect the current choice to migrate. Figure 12 shows the set of induceable migrants using the full model and setting $\delta = 0.8$. The figure shows that if we are willing to assume a risk aversion level of about 7 we can rationalize the data even with a time horizon of 10 years. Lowering the discount factor even more would allow us to match the experimental results for any level of risk aversion. A similar effect can be achieved if we allow for depreciation in the status of being a good migrator due, for example, to random breakdowns of connections at the destination. However, if we bound the depreciation rate to allow for the small drop in migration rates that we observe between 2008 and 2011, it is still the case that very high levels of risk aversion are required to rationalize the data.

8.4 Dis-utility from Migration

Seasonal migration is probably a somewhat unpleasant experience, because it requires migrants to be separated from family, and share more congested space with other men in cities, often in or around slums with poor access to public services. If this utility cost of migration (not captured in our consumption and earnings data) is high enough, it could explain the initial reluctance to migrate. To assess this possibility we asked 1600 households in our sample a stated preference question in 2011: "Would you prefer to stay at home and earn 70 Taka per day, or to migrate and earn x Taka". We asked for $x \in \{90, 110, 130, 150\}$, and the fraction of

respondents who stated they were willing to migrate were {58, 77, 83, 91} respectively. Their responses imply that for every Taka increase in earnings per day at the destination, migration probability increases by 0.5 percentage points. Extrapolating, the respondents would have to be compensated Tk. 15,000 to induce them to migrate for 75 days (which is the average length of migration).[33] These results suggest quite a high utility cost of migration.[34] To incorporate these figures into our quantitative analysis we take a very simple approach: we reduce the return to migration to $m/2$ – an assumption consistent with 70 Taka at home being worth 140 away, towards the high end of the answers we received. The results do not change drastically in the full model (with savings) under this assumption. Migration continues to be a good way to mitigate risk and households will want to save up for it.

8.5 Incorrect Beliefs

In our calibration above we assumed that households were correct in their beliefs about π_G, m and F. If households have incorrect beliefs then it would be much easier to rationalize our empirical observations. Beliefs could be incorrect for two conceptually distinct reasons. First, beliefs may be correct on average, but some households have optimistic beliefs while others are pessimistic. In such a model optimistic households would migrate and the pessimistic would not. Then non-

---

[33] We also estimate this "demand curve for staying at home" with a revealed preference approach, using the fact that re-migration in 2009 was strongly responsive to migration earnings in 2008. That analysis suggests that re-migration probability increased by 1.7 percentage points for every 1000 Taka increases in migration earnings. Under some mild assumption, this implies that migrants induced by our treatment in 2008 would have to be compensated Tk. 21,700 to induce them to re-migrate in 2009.

[34] Banerjee and Duflo (2007) arrive at a similar conclusion while 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?" "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."*

migrators would appear to be systematically biased (Spinnewijn, 2012). Second, it may be that beliefs are systematically pessimistic.[35]

To test whether biased beliefs are important, we asked all migrants in both treatment and control groups about how their migration experience, in terms of time it took to find work and their earnings at destination, compared to their expectations prior to migration. For either interpretation of bias – systematic or on average correct – we would expect that those in the control group, who were already migrating and had had a chance to learn, would have roughly correct beliefs, while those in the treatment group would have beliefs biased toward the overly pessimistic.[36] Results presented in Table 12 are not consistent with biased beliefs: treatment group migrants do not have significantly different beliefs from control group migrants.

8.6 Heterogeneity

Heterogeneity does not seem to be a particularly attractive way to accommodate the data. For example, if we imagine that some households have a high m and some a low m, this helps us to rationalize the lack of migration for the low m households, but makes it even more difficult for the high m households.

8.7 Savings Constraints

The slightly different character of our results for the model with and without savings points to the possible conclusion that it is savings behavior in our setting that is the real anomaly (why are people not saving up to migrate?). Our sample households may be savings-constrained due to sharing norms (Jakiela & Ozier,

---

[35] This could be because non-migrators have access to incorrect information. McKenzie et al. (2007) argue that migrant households provide incorrect information because they do not want to have to share resources, or job connections and accommodation at the destination.

[36] To be clear, it is not evidence of incorrect beliefs that some people found the experience worse than anticipated, this is perfectly consistent with an ex-post statement about an ex-ante risky event. The prediction of biased beliefs is that those in the treatment should be more likely to have done better than expected.

2012), or they may simply be no safe place to store things. This conclusion is consistent with recent research that demonstrates very large impacts of simple interventions that relax savings constraints (e.g. Dupas & Robinson, 2013b). Two caveats should be mentioned, however. First, before citing savings constraints as the key issue, it is necessary to understand why households are able to buffer, but not to save up a lump-sum amount for migration. Second, the right panel of Figure 10, in which we consider a forward looking household that cannot save, suggest that there is a need to understand more than just savings constraints, we must also understand why households act as though they are not aware of the full benefits of migration.

Another related avenue to consider may be the need to share risk and solve public goods problems in general. Risk sharing networks not only constrain savings; they may also deter profitable investments (e.g. Lewis, 1955). Migrating away may undermine network ties, and this may be a hidden cost of migration (Munshi & Rosenzweig, 2009). We lack the data necessary to explore this channel.

8.8   Behavioral or Psychological Explanations

Many models that fall under the rubric of behavioral economics could be used to explain the results. In this area we are particularly wary of making pronouncements without data, as there are many different possible explanations of this type. Here we mention just two models that have been applied to developing country contexts. First, the quasi-hyperbolic discounting model of Laibson (1997) can likely to applied to rationalize the data for some values of β. The version of this model discussed in Duflo, Kremer and Robinson (2011) provides an explanation for low savings. The version of this model discussed in Banerjee and Mullainathan (2010) can explain why households do not undertake profitable investments. Their model also suggests that commitment devices could be useful

policy interventions. Second, Koszegi and Rabin (2006)'s model of reference dependence can likely rationalize the data. That model provides a non-self control based explanation for the fact that household find themselves to be perpetually without the money they need to invest: households adjust their expected consumption in response to shocks and then assess the costs of investments relative to this expected consumption level.

In summary, there are numerous avenues that could be pursued to get a better quantitative accounting of the data generated by our experiment. We have noted just a few. We are currently working on isolating which factors are most relevant in other settings where seasonal migration is relevant.

9 Concluding Remarks

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 on average (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. These results bolster the case made by Clemens et al (2008), Rosenzweig (2006), Gibson and McKenzie (2010), Clemens (2011), Rodrik (New York Times, 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 literature largely focuses on international migration, and we show that the returns to internal migration – a much more common, but under-studied phenomenon[37] – are also large.

---

[37] 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).

We argue that the results are qualitatively consistent with a simple (rational) model of a poverty trap where households that are close to subsistence face a small possibility that migrating will turn out badly, leaving household consumption below subsistence. The model helps us to understand the types of situation in which we would expect incentive and insurance policies to lead to long-term benefits as observed in our experiment. We should look for situations in which the investment is risky, that risk is individual-specific, and where the utility cost of the downside risk is large (e.g. the household is close to subsistence). These predictions also provide an answer to the puzzle that motivated the entire project: why does Rangpur – the poorest region of the country that regularly faces a seasonal famine - have a lower out-migration rate compared to the rest of Bangladesh? 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).

Our quantitative work implies that we cannot provide a fully satisfying explanation for why people in Rangpur had not saved up to migrate.[38] We are therefore hesitant to draw policy implications from our research. However, it is clear that the migration support programs we implement help some Rangpur households cope with the Monga famine, and 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 et al., 2011). Two important caveats are that our research does not capture long-term psychological and social effects of migration, and the scale of our experiment does not permit us to analyze potential adverse

---

[38] Several other papers document 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), and this literature must also confront the same question of why households do not save to invest in these high-return activities.

general equilibrium effects in destination labor markets if the government were to contemplate scaling up such a program.[39]

If there are net efficiency gains, this is likely because 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, 2001). 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.[40] The potential efficiency gains raise an interesting question of why private sector entities do not profit by developing mechanisms that link migrants to employers in the city. To understand this, we interviewed several employers in Dhaka. The employers reported that there are in fact "labor sardars" who bring migrant workers to Dhaka, but the process is fraught with uncertainty and risk. Migrants have to be paid the one-way bus ticket and some salary in advance, but it is difficult to enforce any long-term contract if they disappear and choose to go work elsewhere after the transit cost is paid.

---

[39] There is mixed evidence in the literature on whether these effects are substantial (Ottaviano & Peri, (forthcoming); Borjas, 2003; Borjas & Katz, 2007; Card, 2009). Moreover, general equilibrium effects may be positive in net, if spillover benefits at the origin exceed external costs at the destination. Migrants form a much larger part of the village economy at the origin compared to the destination urban economy.

[40] With credit contracts, it may be difficult to collect regular repayment from migrants who move away, but one of the world's largest micro-credit NGOs, BRAC, has recently introduced credit programs to finance even international migration.

References

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.

Ashraf, Nava, Aycinena, Diego, Martinez, Claudia A. & Yang, Dean. (forthcoming). Remittances and the Problem of Control: A Field Experiment among Migrants from El Salvador. *Review of Economics and Statistics*

Aycinena, Diego, Martinez, Claudia A. & Yang, Dean. (2010). *The Impact of Remittance Fees on Remittance Flows: Evidence from a Field Experiment among Salvadoran Migrants.*

Bandiera, Oriana, Barankay, Iwan & Rasul, Imran. (2011). Field Experiments with Firms. *Journal of Economic Perspectives*, 25(3): 63-82.

Banerjee, Abhijit. (2004). The Two Poverties. In S. Dercon (Ed.), *Insurance Against Poverty* (pp. 59-76). Oxford: Oxford University Press.

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. Working Paper.*

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

Bangladesh Bureau of Statistics. (2011). *Monthly Statistical Bulletins.* Retrieved from
http://www.bbs.gov.bd/home.aspx

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.

Basu, Karna & Wong, Maisy. (2012). *Evaluating Seasonal Food Security Programs in
East Indonesia.*

Beegle, Kathleen, De Weerdt, Joachim & Dercon, Stefan. (2011). Migration and
Economic Mobility in Tanzania: Evidence from a Tracking Survey. *The Review
of Economics and Statistics*, 93(3): 1010-33. doi:10.1162/REST_a_00105

Borjas, George. (2003). The Labor Demand Curve is Downward Sloping:
Reexamining the Impact of Immigration on the Labor Market. *The Quarterly
Journal of Economics*, 118(4): 1335-1374.

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.* Australia: School of Economics, The University of Queensland.
Discussion Paper. No. 347.

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

Buera, Francisco J. (2009). A Dynamic Model of Entrepreneurship with Borrowing
Constraints: Theory and Evidence. *Annals of Finance*, 5: 443-464.
doi:10.1007/s10436-009-0121-2

Card, David. (2009). Immigration and Inequality. *The American Economic Review*,
99(2): 1-21. doi:10.1257/aer.99.2.1

Caselli, Francesco. (2005). Accounting for Cross-Country Income Differences. In P.
Aghion, & S. Durlauf (Eds.), *Handbook of Economic Growth* (pp. 679-741).
Amsterdam: Elsevier.

Chaudhuri, Shubham & Paxson, Christina. (2002). *Smoothing Consumption Under Income Seasonality: Buffer Stocks Vs. Credit Markets* Columbia University, Department of Economics. Department of Economics Discussion Papers. No. 0102-54. Retrieved from http://hdl.handle.net/10022/AC:P:363

Chetty, Raj & Szeidl, Adam. (2007). Consumption Commitments and Risk Preferences. *The Quarterly Journal of Economics*, 122(2): 831-877. doi:10.1162/qjec.122.2.831

Chin, Aimee, Karkoviata, Leonie & Wicox, Nathaniel. (2010). *Impact of Bank Accounts on Migrant Savings and Remittances: Evidence from a Field Experiment.* Universtiy of Houston. mimeo.

Clarke, Daniel J. (2011). *A Theory of Rational Demand for Index Insurance.* University of Oxford. Department of Economics Discussion Paper Series. No. 572. Retrieved from http://www.economics.ox.ac.uk/index.php/department-of-economics-discussion-paper-series/a-theory-of-rational-demand-for-index-insurance

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

Clemens, Michael. (2011). Economics and Emigration: Trillion-Dollar Bills on the Sidewalk? *The Journal of Economic Perspectives*, 25(3): 83-106. doi:10.1257/jep.25.3.83

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

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

Deaton, Angus. (1991). Savings and Liquidity Constraints. *Econometrica*, 59(5): 1221-1248.

Deaton, Angus. (1997). *The Analysis of Household Surveys: A Microeconometric Approach to Development Policy*. Baltimore: Johns Hopkins University Press for the World Bank.

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, 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.* National Bureau of Economic Research. No. 16298.

Dupas, Pascaline & Robinson, Jonathan. (2013a). Savings Constraints and Microenterprise Development: Evidence from a Field Experiment in Kenya. *American Economic Journal: Applied Economics*, 5(1): 163-192.

Dupas, Pascaline & Robinson, Jonathan. (2013b). Why Don't the Poor Save More? Evidence from Health Savings Experiments. *American Economic Review*, 103(4): 1138-1171. doi:10.1257/aer.103.4.1138

Eeckhoudt, Louis, Gollier, Christian & Schlesinger, Harris. (1996). Changes in Background Risk and Risk Taking Behavior. *Econometrica*, 64(3): 683-689. Retrieved from http://www.jstor.org/stable/2171866

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.* Washington, D.C.: National Bureau of Economic Research. Working Paper. No. 17207.

Fischer, Greg. (2013). Contract Structure, Risk Sharing and Investment Choice. *Econometrica*, 81(3): 883-939. doi:10.3982/ECTA9100

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

Foster, Andrew & Rosenzweig, Mark R. (2011). *Are Indian Farms Too Small? Mechanization, Agency Costs and Farm Efficiency.* Yale University: *Working Paper.*

Gibson, John & McKenzie, David. (2010). *The Development Impact of a Best Practice Seasonal Worker Policy.* Bureau for Research and Economic Analysis of Development. BREAD Working Paper. No. 286. Retrieved from http://ipl.econ.duke.edu/bread/abstract.php?paper=286

Gibson, John, McKenzie, David & Stillman, Steven. (2013). 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*, 61(2): 247-280.

Gollin, Douglas, Lagakos, David & Waugh, Michael E. (2011). *The Agricultural Productivity Gap in Developing Countries.* New York University, Leonard N. Stern School of Business, Department of Economics. Working Paper Series. No. 11-14. Retrieved from https://files.nyu.edu/mw134/public/uploads/56836/apg.pdf

Gollin, Douglas, Parente, S. & Rogerson, R. (2002). The Role of Agriculture in Development. *American Economic Review Papers and Proceedings*, 92(2): 160-164.

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. (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.

Holt, Charles A. & Laury, Susan K. (2002). Risk Aversion and Incentive Effects. *The American Economic Review*, 92(5): 1644-1655. Retrieved from http://www.jstor.org/stable/3083270

Jain, Tarun & Sood, Ashima. (2012). *How does Relationship Based Governance Accomodate New Entrants? Evidence from the Cycle Rickshaw Rental Market.*

Jakiela, Pamela & Ozier, Owen. (2012). *Does Africa Need a Rotten Kin Theorem? Experimental Evidence from Village Economies. Working Paper.*

Jalan, Jyotsna & Ravallion, Martin. (2001). Behavioral Responses to Risk in Rural China. *Journal of Development Economics*, 66(1): 23-49. doi:10.1016/S0304-3878(01)00154-7

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

Kandel, Shmuel & Stambaugh, Robert F. (1991). Asset Returns and Intertemporal Preferences. *Journal of Monetary Economics*, 27(1): 39-71. doi:10.1016/0304-3932(91)90004-8

Khandker, Shahidur R. (2012). Seasonality of Income and Poverty in Bangladesh. *Journal of Development Economics*, 97(2): 244-256. doi:http://dx.doi.org/10.1016/j.jdeveco.2011.05.001,

Khandker, Shahidur R., Khaleque, M. A. & Samad, Hussain. (2011). *Can Social Safety Nets Alleviate Seasonal Deprivation? Evidence from Northwest Bangladesh.* The World Bank. Policy Research Working Paper. No. 5865.

Khandker, Shahidur R. & Mahmud, Wahiduddin. (2012). *Seasonal Hunger and Public Policies: Evidence from Northwest Bangladesh.* Washington, D.C.: The 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.

Kimball, Miles S. (1990). Precautionary Saving in the Small and in the Large. *Econometrica*, 58(1): 53-73. Retrieved from http://www.jstor.org/stable/2938334

Koszegi, Botond & Rabin, Matthew. (2006). A Model of Reference-Dependent Preferences. *The Quarterly Journal of Economics*, 121(4): 1133-1165.

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

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

Lewis, W. A. (1955). *The Theory of Economic Growth*. London: Allen & Unwin.

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

Martinez,Claudia A.,Yang,Dean. (2005). Remittances and Poverty in Migrants' Home Areas: Evidence from the Philippines. In C. Ozden, & M. Schiff (Eds.), *International Migration, Remittances, and the Brain Drain* (). Washington, D.C.: World Bank.

McKenzie, David. (2012). *Learning about Migration through Experiments.* Centre for Research and Analysis of Migration. CReAM Discussion Paper. No. 07/12. Retrieved from http://www.cream-migration.org/publ_uploads/CDP_07_12.pdf;

McKenzie, David, Gibson, John & Stillman, Steven. (2007). *A Land of Milk and Honey with Streets Paved with Gold: Do Emigrants have Over-Optimistic Expectations about Incomes Abroad.* The World Bank. Policy Research Working Paper. No. 4141. doi:10.1596/1813-9450-4141

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.

McKenzie, David & Yang, Dean. (2010). *Experimental Approaches in Migration Studies.* Bonn: Institute for the Study of Labor (IZA). Discussion Paper. No. 5125.

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

Meredith, Jennifer, Robinson, Jonathan, Walker, Sarah & Wydick, Bruce. (2011). *Keeping the Doctor Away: Experimental Evidence on Investment in Preventative Health Products.* from http://people.ucsc.edu/~jmrtwo/HealthProducts.pdf

Miller, Grant & Mobarak, A. M. (2013). *Gender Differences in Preferences, Intra-Household Externalities and Low Demand for Improved Cookstoves. Working paper.* from http://faculty.som.yale.edu/mushfiqmobarak/stove_long.pdf

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.* National Bureau of Economic Research. No. Working Paper 14850.

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

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

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.

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

Rodrik, Dani. (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.

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.

Spinnewijn, Johannes. (2012). *Heterogeneity, Demand for Insurance and Adverse Selection.* Center for Economic Policy and Research. CEPR Discussion Paper. No. DP8833. Retrieved from http://ssrn.com/abstract=2013824

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.

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

Vereshchagina, Galina & Hopenhayn, Hugo A. (2009). Risk Taking by Entrepreneurs. *American Economic Review*, 99(5): 1808-1830. doi:10.1257/aer.99.5.1808

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.* Washington, D.C.: Environment for Development Intiative and Resources for the Future. No. EfD Discussion Paper 09-13.

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

Table 1. Randomization Balance on Observables at Baseline

	Incentivized		Non-Incentivized		Diff I v NI	P-value
	Cash	Credit	Control	Info		
Consumption of Food	805.86	813.65	818.68	768.64	15.84	0.638
	(19.16)	(40.91)	(31.76)	(18.00)	(33.57)	
Consumption of Non-Food	248.98	262.38	248.4	237.35	12.23	0.278
	(5.84)	(6.74)	(9.28)	(7.99)	(11.20)	
Total Consumption	1054.83	1076.03	1067.08	1005.99	28.06	0.465
	(21.11)	(42.08)	(34.55)	(22.77)	(38.29)	
Total Calories (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)	
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)	
Consumption of Meat Products	25.04	18.24	27.13	20.71	-1.97	0.594
	(2.58)	(2.0)	(3.24)	(2.90)	(3.69)	
Consumption of Milk & Eggs	11.74	9.77	9.96	10.77	0.48	0.675
	(0.79)	(0.80)	(1.12)	(1.19)	(1.13)	
Consumption of Fish	42.17	39.86	41.36	45.98	-2.56	0.496
	(1.83)	(1.79)	(2.76)	(2.89)	(3.74)	
Consumption of 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)	
Consumption of 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)	
Consumption of 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)	
Consumption of 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 in Cash (conditional on positive savings)	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 Consumption 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	Cash	Credit	**Not Incentivized**	Diff (I-NI)
Migration Rate in 2008	58%	59.0%	56.8%	36%	22***
	(1.4)	(1.87)	(2.06)	(1.96)	(2.43)
Migration Rate in 2009	47%	45%	49%	37%	10***
	(1.41)	(1.92)	(2.12)	(2.0)	(2.46)
Migration Rate in 2011	44%	40%	50%	36%	8***
	(1.33)	(3.04)	(3.04)	(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: Effects of Migration before December 2008 on Consumption in 2008 Amongst Remaining Household Members

Dependent Variable	ITT			ITT	ITT	IV	IV	OLS	Mean
	Cash	Credit	Info						
Consumption of Food	61.876**	50.044*	15.644	48.642**	44.183*	280.792**	260.139**	102.714***	689.9
	(29.048)	(28.099)	(40.177)	(24.139)	(23.926)	(131.954)	(128.053)	(17.147)	
Consumption of Non-Food	34.885***	27.817**	22.843	20.367**	16.726*	115.003**	99.924*	59.085***	260.9
	(13.111)	(12.425)	(17.551)	(9.662)	(9.098)	(56.692)	(51.688)	(8.960)	
Total Consumption	96.566***	76.743**	38.521	68.359**	60.139**	391.193**	355.115**	160.696***	950.8
	(34.610)	(33.646)	(50.975)	(30.593)	(29.683)	(169.431)	(158.835)	(22.061)	
Total Calories (per person per day)	106.819*	93.429	-85.977	142.629***	129.901***	842.673***	757.602***	317.495***	1976.1
	(62.974)	(59.597)	(76.337)	(47.196)	(48.057)	(248.510)	(250.317)	(41.110)	
Calories from Protein (per person per day)	2.852*	2.588	-0.509	2.977**	2.657**	17.442**	15.573**	6.777***	44.0
	(1.557)	(1.571)	(2.089)	(1.287)	(1.273)	(7.064)	(6.830)	(0.992)	
Consumption of Meat Products	12.325**	6.577	8.163	5.618	5.599	31.857	34.302	3.905	24.2
	(5.489)	(5.402)	(6.667)	(3.755)	(3.726)	(21.549)	(21.399)	(3.923)	
Consumption of Fish	8.979*	12.618**	8.977	6.297	5.193	34.652	28.775	8.901**	67.3
	(4.743)	(5.998)	(6.076)	(4.407)	(4.142)	(24.941)	(22.909)	(3.778)	
Consumption of Children's Education	6.146*	7.658**	1.546	6.110**	4.299*	30.848**	21.487	-3.677	14.5
	(3.297)	(3.441)	(3.938)	(2.485)	(2.405)	(14.144)	(13.536)	(2.355)	
Sub-district Fixed Effects?	yes	yes	yes	yes	yes	yes	yes	yes	
Additional controls	no	no	no	no	yes	no	yes	no	

Robust standard errors in parentheses, clustered by village. *** p<0.01, ** p<0.05, * p<0.1. Each row is a different dependent variable (in column 1). In the IV columns, these dependent variables are regressed on "Migration", which is a binary variable equal to 1 if at least one member of the household migrated and 0 otherwise. The last column reports sample mean of the dependent variable in the control group. All consumption (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 5 and 7 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 4: Effects of Migration in 2008 on Consumption in 2009 Amongst Remaining Household Members

Dependent Variable	ITT			ITT	ITT	IV	IV	OLS	Mean
	Cash	Credit	Info						
Consumption of Food	31.437	16.047	-37.521	42.691**	33.108*	225.232**	183.990**	-26.029*	913.98
	(23.587)	(23.226)	(28.255)	(17.073)	(16.849)	(100.891)	(92.927)	(14.855)	
Consumption of Non-Food	3.471	30.840*	-4.411	18.514	11.997	98.188	59.937	-2.369	326.99
	(16.629)	(17.871)	(21.943)	(13.034)	(13.080)	(72.243)	(70.778)	(10.865)	
Total Consumption	34.908	46.887	-41.932	61.205**	45.105*	323.420**	243.927*	-28.398	1240.97
	(31.113)	(33.736)	(40.345)	(24.655)	(24.668)	(144.313)	(134.898)	(21.342)	
Total Calories (per person per day)	77.706	1.832	-91.558	87.148**	73.146*	466.907**	412.931**	-33.656	2057.67
	(54.683)	(60.426)	(55.842)	(37.337)	(37.850)	(217.023)	(206.676)	(28.901)	
Calories from Protein (per person per day)	1.341	-0.062	-2.396*	1.862**	1.472*	9.898**	8.301*	-0.891	46.49
	(1.213)	(1.280)	(1.314)	(0.822)	(0.827)	(4.778)	(4.484)	(0.713)	
Consumption of Meat Products	-4.164	4.615	-4.090	1.980	1.630	9.572	7.376	-4.513	26.56
	(5.048)	(5.151)	(6.109)	(3.152)	(3.177)	(17.267)	(17.148)	(2.880)	
Consumption of Fish	5.090	-4.495	-3.045	2.083	1.519	11.072	9.998	2.600	66.45
	(4.926)	(5.037)	(5.936)	(3.513)	(3.642)	(19.095)	(19.492)	(2.670)	
Consumption of Children's Education	-0.156	-0.585	-3.828	1.546	0.140	7.660	1.057	-5.635***	16.89
	(2.742)	(2.774)	(2.976)	(1.768)	(1.615)	(8.861)	(7.910)	(1.680)	
Sub-district Fixed Effects?	yes	yes	yes	yes	yes	yes	yes	yes	
Additional controls	no	no	no	no	yes	no	yes	no	

Robust standard errors in parentheses, clustered by village. *** p<0.01, ** p<0.05, * p<0.1. Each row is a different dependent variable (in column 1). In the IV columns, these dependent variables are regressed on "Migration", which is a binary variable equal to 1 if at least one member of the household migrated and 0 otherwise. The last column reports sample mean of the dependent variable in the control group. All consumption (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 5 and 7 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 Destination (Data for Migrants Only; Non-

	All Migrants	Incentivized	Not Incentivized	Diff	Obs
Total Savings by household	3,490.47	3,506.59	3,434.94	71.65	951
	(97.22)	(110.83)	(202.80)	(232.91)	
Total Earnings by household	7,777.19	7,451.27	8,894.40	-1443.129**	952
	(244.77)	(264.99)	(586.14)	(583.83)	
Savings per day	56.76	56.46	57.79	-1.33	905
	(1.15)	(1.29)	(2.56)	(2.77)	
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)	(3.80)	(7.62)	(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	75.00	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	9.00	5.75	3.83	19.02	6039.72
	(1.82)	(1.66)	(1.55)	(1.42)	(1.78)	(317.52)
Transport	40.93	11.00	1.33	1.09	15.34	4993.81
	(2.21)	(1.81)	(0.76)	(0.77)	(1.63)	(203.12)
Other	20.97	5.00	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 at Destination	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. Shows the proportion of workers in each occupation by destination, average total earnings by sector across destinations, and average total earnings by destination across sectors. Based on migration for work episodes between September 1, 2008 to April 13, 2009. Occupation at the destination is based on the question, "In which sector were you employed (agriculture, industry, etc)?" Bogra and Tangail, which employ most migrant workers in the agriculture sector, are potato-growing areas which do not follow the same crop and seasonal cycle as rice-growing Rangpur.

Table 7. Treatment Effects in 2011 Accounting for Basis Risk in the Insurance Program

Dep. Var.: Migrated in 2011	(1)	(2)	(3)	(4)
Conditional Credit	0.156***	0.170***	0.172***	0.180***
	(0.077)	(0.075)	(0.073)	(0.072)
Rainfall Insurance	0.139***	0.149***	0.228***	0.224***
	(0.056)	(0.055)	(0.055)	(0.056)
Unconditional Credit	0.099	0.102	0.135***	0.134***
	(0.065)	(0.062)	(0.062)	(0.061)
Went to Bogra before Baseline		0.180***		0.149***
		(0.042)		(0.080)
Identified as Farmer at Baseline			0.172***	0.159***
			(0.027)	(0.029)
Went to Bogra before x Rain insurance		0.083		0.223
		(0.103)		(0.137)
Farmer at Baseline x Rain Insurance			-0.120***	-0.106***
			(0.044)	(0.046)
Went to Bogra x Farmer				-0.015
				(0.088)
Went to Bogra x Farmer x Rain				-0.203
				(0.153)
District Fixed Effects?	Yes	Yes	Yes	Yes
Constant	0.214***	0.197***	0.126***	0.120***
	(0.064)	(0.061)	(0.063)	(0.061)
Observations	2,051	2,050	2,043	2,043
R-squared	0.041	0.053	0.065	0.072
p-value for F-test: Conditional credit = Rainfall Insurance	0.842			
p-value of Responsive to Rainfall Insurance for those going to Bogra		0.0275		
p-value of Responsive to Rainfall Insurance for non-farmers			0.0825	
p-value of Responsive to Rainfall Insurance for non-farmers going to Bogra				0.00145
p-value of Responsive to Rainfall Insurance for farmers not going to Bogra				0.0577
p-value of Responsive to Rainfall Insurance for farmers going to Bogra				0.382

Robust standard errors 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.

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

Dep. Var.: Migration in 2009	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Did any member of the household migrate in 2008?	0.392***	0.410***	0.392***	0.464***	0.393***	0.436***	0.392***	0.476***
	(0.02)	(0.145)	(0.02)	(0.133)	(0.021)	(0.132)	(0.02)	(0.13)
Number of friends and relatives who migrated			0.007	-0.006				
			(0.01)	(0.022)				
Number of friends who migrated					-0.012	-0.048		
					(0.025)	(0.049)		
Number of relatives who migrated							0.01	0.007
							(0.011)	(0.027)
Constant	0.097***	0.088	0.095**	0.062	0.098***	0.078	0.095**	0.052
	(0.037)	(0.083)	(0.038)	(0.078)	(0.037)	(0.076)	(0.038)	(0.077)
Observations	1818	1818	1818	1818	1797	1797	1797	1797
R-squared	0.207	0.206	0.207	0.201	0.208	0.206	0.209	0.202

*** $p<0.01$, ** $p<0.05$, * $p<0.1$ Robust standard errors in parentheses.

Table 9. Differences in Characteristics Between Migrants in Treatment and in Control Group

Panel A: Percentage of Migrants that Know Someone at Destination

	Incentive	Non incentive	Diff
First Episode	47%	64%	17***
	(1.85)	(3.30)	(3.8)
Any Episode	55%	62%	6.3*
	(1.80)	(3.23)	(3.70)

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

	Incentive	Non incentive	Diff
First Episode	27%	44%	17***
	(1.64)	(3.41)	(3.55)
Any Episode	31%	44%	12.8***
	(1.67)	(3.30)	(3.56)

Panel C: Percentage of Migrants Traveling Alone

	Incentive	Non incentive	Diff
First Episode	30%	32%	1.6
	(1.70)	(3.20)	(3.6)
Any Episode	37%	37%	0.44
	(1.75)	(3.20)	(3.65)

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

Table 10. Destination Choices of Re-Migrants

Dep. Var.: Migrated in 2009 to:	2009 - Dhaka		2009 - Bogra		2009 - Tangail		2009 - Munshigonj	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Migrated in 2008 to Dhaka	0.413***	0.679*						
	(0.052)	(0.348)						
Migrated in 2008 to Bogra			0.333***	0.051				
			(0.061)	(0.177)				
Migrated in 2008 to Tangail					0.463***	0.108		
					(0.057)	(0.184)		
Migrated in 2008 to Munshigonj							0.233***	0.304*
							(0.050)	(0.185)
Constant	0.317***	0.213	-0.014	-0.002	0.027	0.073	0.059	0.038
	(0.068)	(0.148)	(0.012)	(0.008)	(0.050)	(0.054)	(0.037)	(0.060)
Observations	589	589	589	589	589	589	589	589
R-squared	0.195	0.132	0.205	0.032	0.305	0.081	0.155	0.085
1st F-test		1.139		4.338		2.116		0.980
1st pvalue		0.345		0.000166		0.0412		0.456
1st partial R2		0.0119		0.0561		0.0616		0.0217
Hansen J0		4.272		7.142		8.882		3.920
R2 overall		0.132		0.0317		0.0814		0.0849
Dep. Var.: Migrated in 2011 to:	2011 - Dhaka		2011 - Bogra		2011 - Tangail		2011 - Munshigonj	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Migrated in 2008 to Dhaka	0.327***	0.655**						
	(0.055)	(0.318)						
Migrated in 2008 to Bogra			0.280***	0.068				
			(0.061)	(0.166)				
Migrated in 2008 to Tangail					0.376***	0.285		
					(0.092)	(0.265)		
Migrated in 2008 to Munshigonj							0.275***	0.108
							(0.059)	(0.236)
Constant	0.248***	0.127	0.076	0.098	0.079	0.098	0.138	0.182
	(0.070)	(0.126)	(0.097)	(0.085)	(0.175)	(0.174)	(0.096)	(0.120)
Observations	480	480	480	480	480	480	480	480
R-squared	0.179	0.067	0.127	0.032	0.181	0.117	0.220	0.061
1st F-test		0.986		4.649		2.706		1.781
1st pvalue		0.452		8.24e-05		0.0100		0.0905
1st partial R2		0.0166		0.0775		0.0554		0.0354
Hansen J0		7.374		4.322		16.50		4.131
R2 overall		0.0668		0.0319		0.117		0.0611

*** $p<0.01$, ** $p<0.05$, * $p<0.1$ Robust standard errors in parentheses. 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 11. Parameters Used for Calibration

Parameter	Calibration	Notes
$u(c)$	$\dfrac{(c-s)^{1-\sigma}}{1-\sigma}$	HARA utility function.
s	*250* Taka per hh member per month	Enough for about 600 Calories per hh member per month
π_G	*0.5*	The portion of induced migrants that remigrate
F	*250* Taka per hh member per month	600 Taka for bus fare, plus 6 days of foregone labor at 60 Taka per day. Spread over 4 hh members
m	*550* per household member per month	Solution to: $\pi_G(m+I) = 350$ where 350 is our LATE estimate and I is the size of our incentive.
$\mu(y)$	$N(700,7)$ per household member, per month	Designed to look like the distribution of the bottom half of the population
Time Period	6 months	We assume the choice to migrate can be made after planting for either of the agricultural seasons.
δ	0.99	
I (incentive size)	200 Taka per household member	Assumes a households size of 4

Table 12. Expectations about finding a job and earnings (Non-experimental: Asked of 2008 migrants)

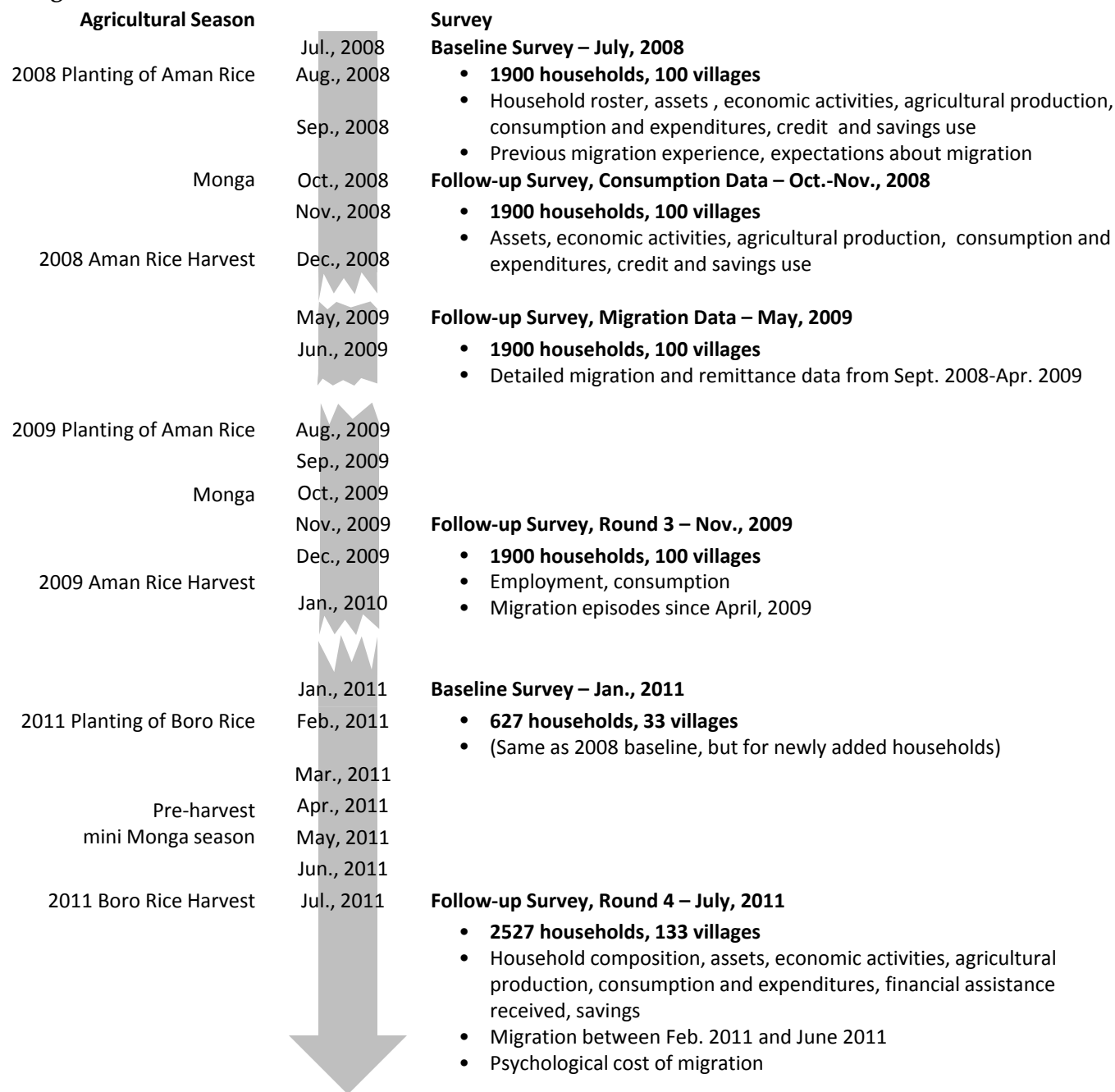|	Incentivized	Not incentivized	Diff
Expectations about finding a job			
Too optimistic (job search took more time than expected)	0.18	0.14	0.05
	(0.01)	(0.02)	(0.03)
Too pessimistic (job search took less time than expected)	0.24	0.27	-0.03
	(0.02)	(0.03)	(0.03)
As expected	0.58	0.59	-0.01
	(0.02)	(0.03)	(0.04)
Expectations about earnings at the destination			
Too optimistic (earned less than expected)	0.42	0.39	0.03
	(0.02)	(0.03)	(0.04)
Too pessimistic (earned more than expected)	0.26	0.27	0.00
	(0.02)	(0.03)	(0.03)
As expected	0.32	0.34	-0.02
	(0.02)	(0.03)	(0.04)
Expectations about the Severity of Monga			
Monga (1-100 scale)	78.71	77.05	1.04
	(0.58)	(0.89)	(1.66)

Standard errors in parentheses.

Figure 1. Seasonality in Consumption and Price in Rangpur and in Other Regions of Bangladesh
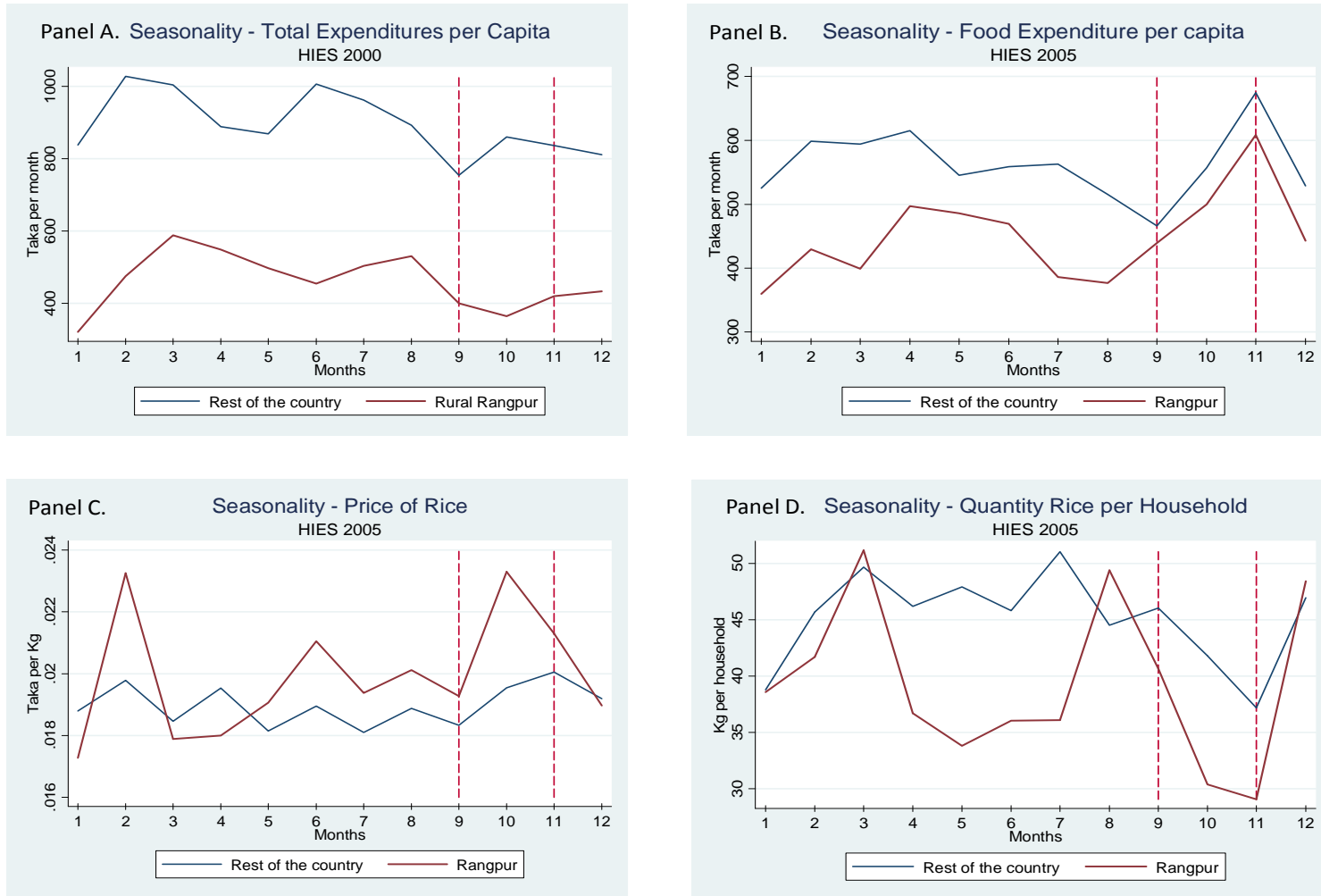
Source: Bangladesh Bureau of Statistics 2005 Household Income and Expenditure Survey

Figure 2. Trial Profile and Timeline

Agricultural Season		Survey	Experimental Design

Agricultural Season

2008 Planting of Aman Rice — Jul., 2008 / Aug., 2008 / Sep., 2008

Monga — Oct. 2008 / Nov., 2008

2008 Aman Rice Harvest — Dec., 2008

May, 2009 / Jun., 2009

2009 Planting of Aman Rice — Aug., 2009 / Sep., 2009

Monga — Oct., 2009

Nov., 2009 / Dec., 2009

2009 Aman Rice Harvest — Jan., 2010

Jan., 2011

2011 Planting of Boro Rice — Feb., 2011

Mar., 2011

Pre-harvest — Apr., 2011

mini Monga season — May, 2011 / Jun., 2011

2011 Boro Rice Harvest — Jul., 2011

Survey

Baseline Survey – July, 2008
- **1900 households, 100 villages**
- Household roster, assets , economic activities, agricultural production, consumption and expenditures, credit and savings use
- Previous migration experience, expectations about migration

Follow-up Survey, Consumption Data – Oct.-Nov., 2008
- **1900 households, 100 villages**
- Assets, economic activities, agricultural production, consumption and expenditures, credit and savings use

Follow-up Survey, Migration Data – May, 2009
- **1900 households, 100 villages**
- Detailed migration and remittance data from Sept. 2008-Apr. 2009

Follow-up Survey, Round 3 – Nov., 2009
- **1900 households, 100 villages**
- Employment, consumption
- Migration episodes since April, 2009

Baseline Survey – Jan., 2011
- **627 households, 33 villages**
- (Same as 2008 baseline, but for newly added households)

Follow-up Survey, Round 4 – July, 2011
- **2527 households, 133 villages**
- Household composition, assets, economic activities, agricultural production, consumption and expenditures, financial assistance received, savings
- Migration between Feb. 2011 and June 2011
- Psychological cost of migration

Experimental Design

First Experiment – August, 2008

Households (Villages)	
Cash	703 (37)
Credit	589 (31)
Information	304 (16)
Control	304 (16)
Total	**1900 (100)**

Group Formation Requirement	
Individual	476
Assigned Group	408
Self-Formed Group	408

Destination is assigned for half the sample in each group, randomized at the household level

Second Experiment – February, 2011

Households (Villages)	
Rainfall Insurance	456 (24)
Rice Price Insurance	456 (24)
Unconditional Credit	285 (15)
Conditional Credit	285 (15)
Control	665 (35)
Total	**2147 (113)**

Figure 3. Value functions of migrating and non-migrating households

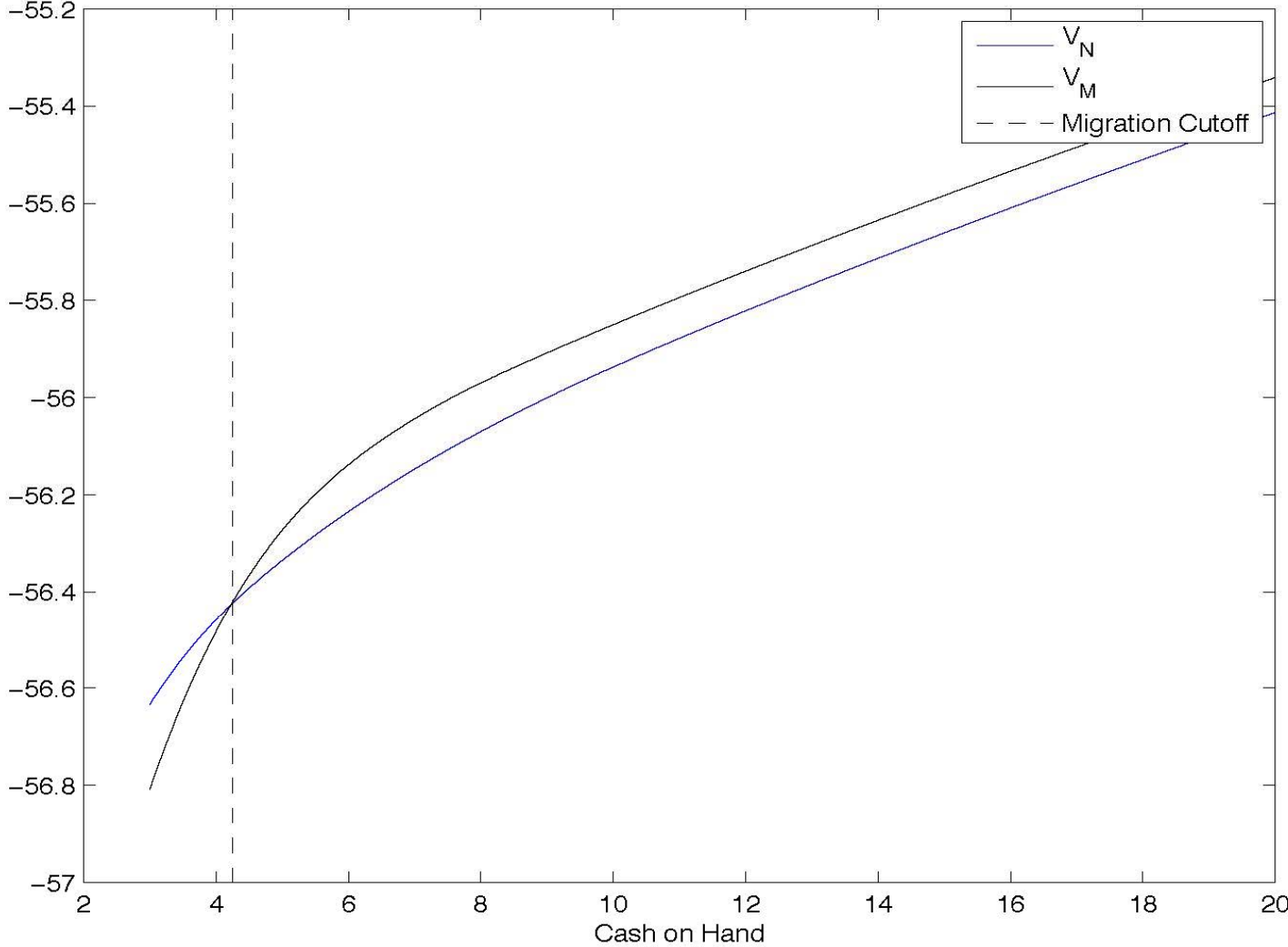**Figure 4. Policy functions (consumption as a function of cash on hand) for households bad at migrating and households restricted from migrating**
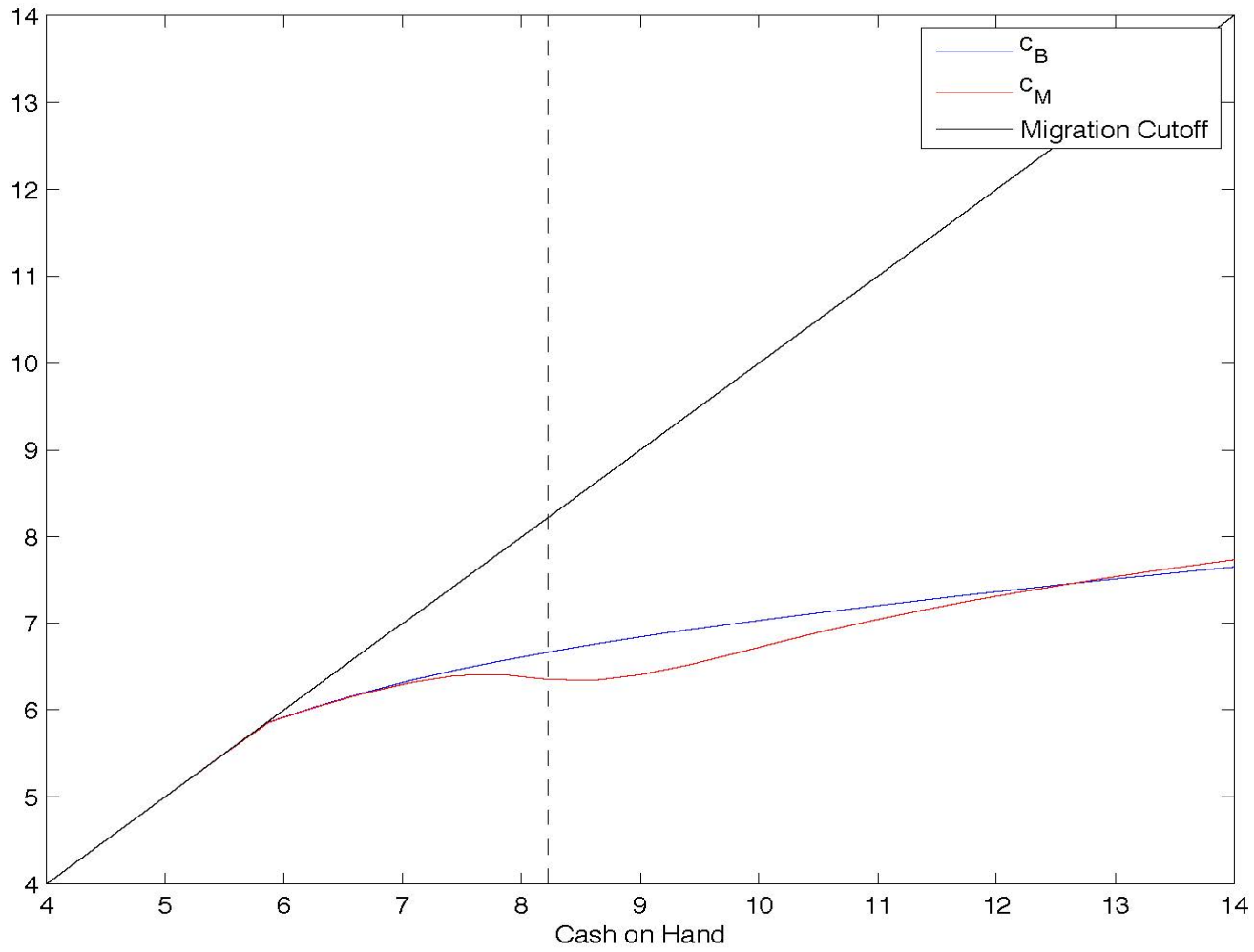
Figure 5. Simulated Cahs on Hand and Consumptions for Varying Levels of Wealth
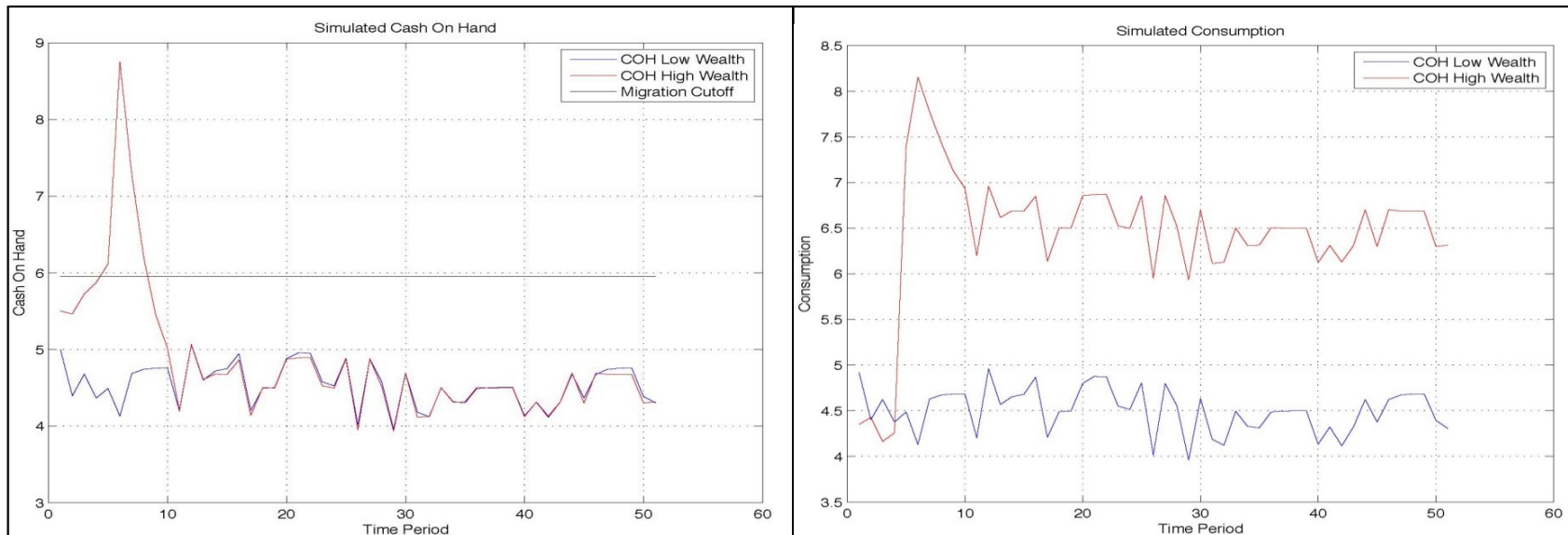
Panel A

Panel B

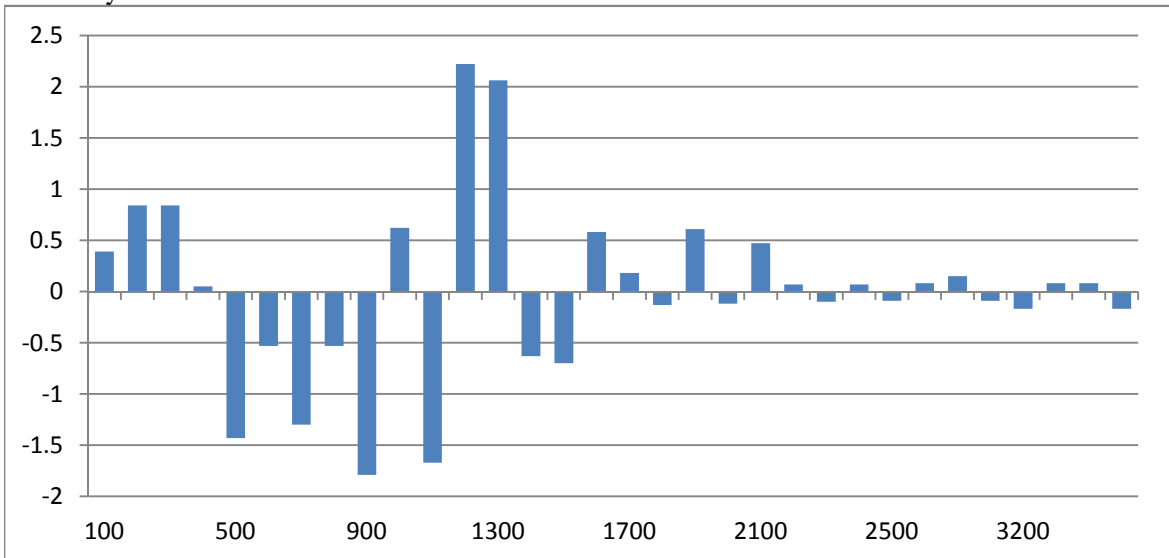**Figure 6. Distribution of Consumption in Control Villages subtracted from Distribution of Consumption in Treatment Villages**

Panel A. Risk: If the Migration Incentive was not paid out, and the migration cost had to be borne by the household

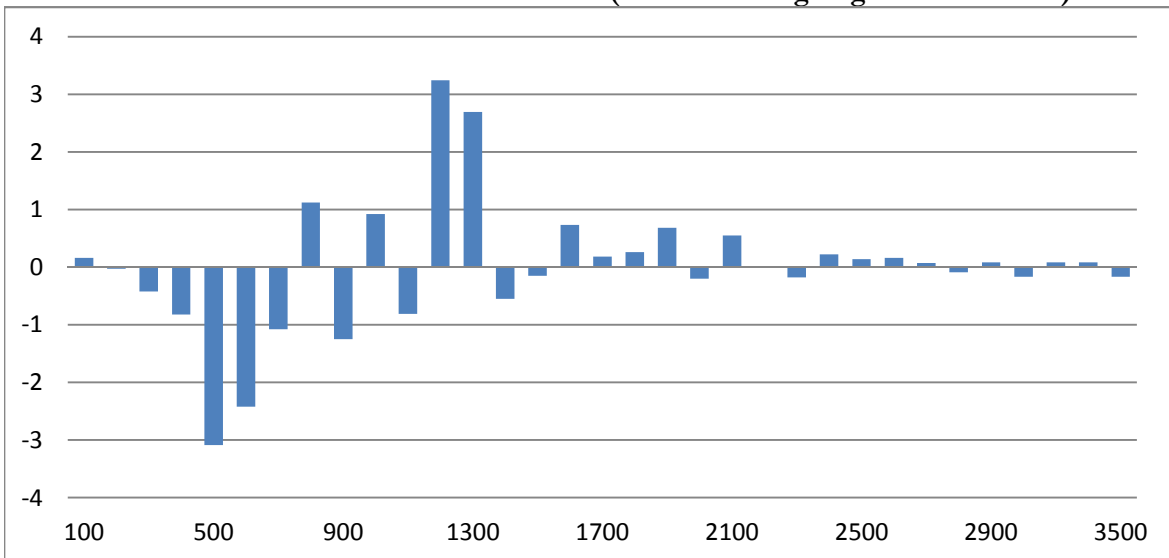**Panel B. Treatment minus Control Distribution (not subtracting migration incentive)**

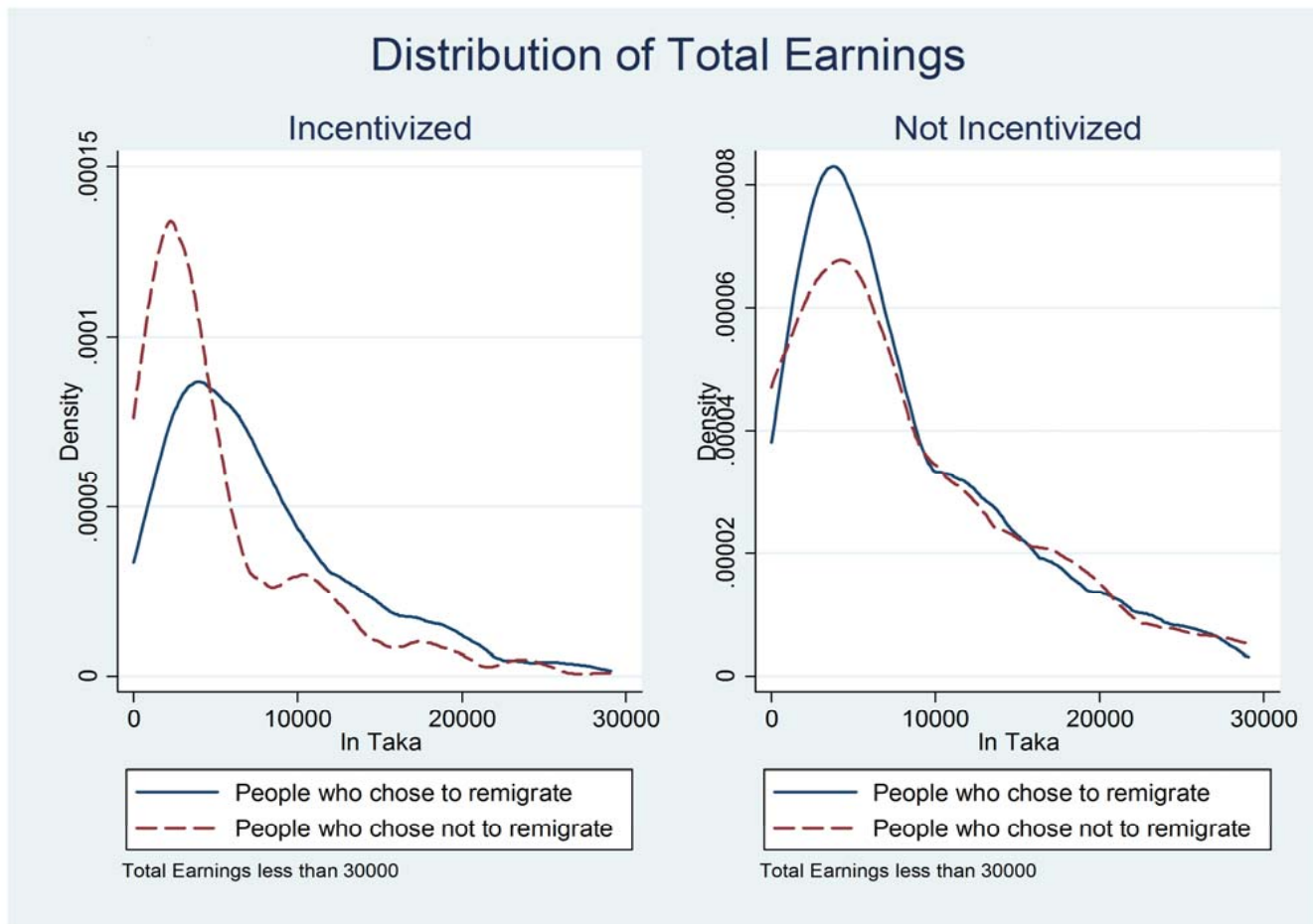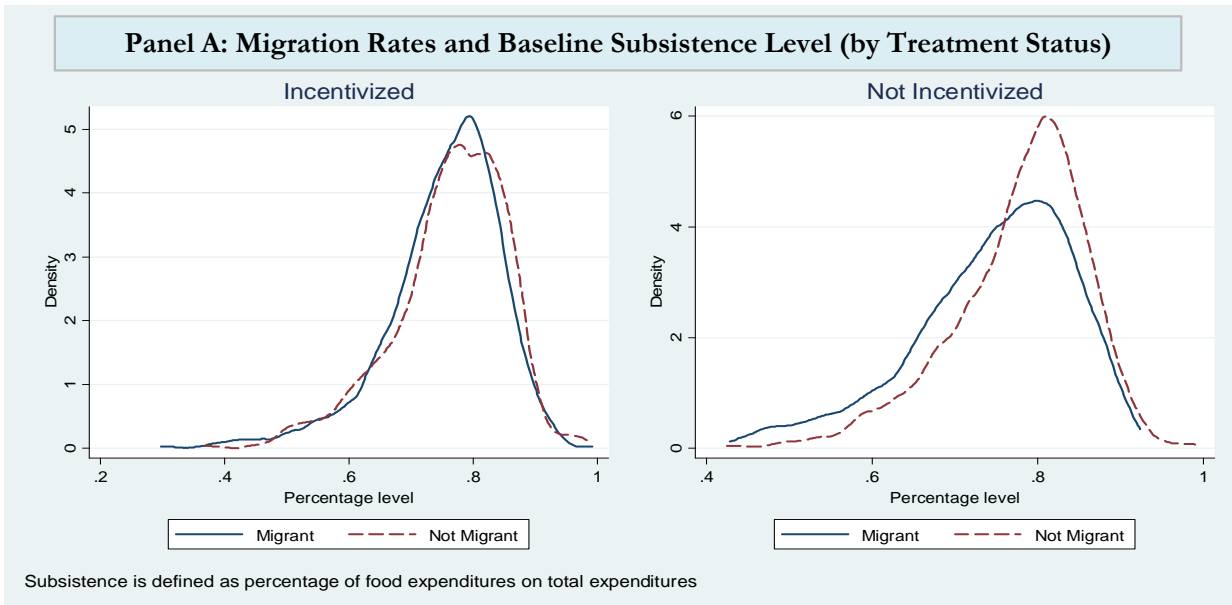**Figure 7. Migration experience in 2008 by re-migration status in 2009.**

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

Panel A: Migration Rates and Baseline Subsistence Level (by Treatment Status)

Subsistence is defined as percentage of food expenditures on total expenditures

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.

Panel C: Migration Rates across the Distribution of Food Expenditures/Total Expenditures

Food Expenditures / Total Expenditures (Subsistence Level at Baseline)
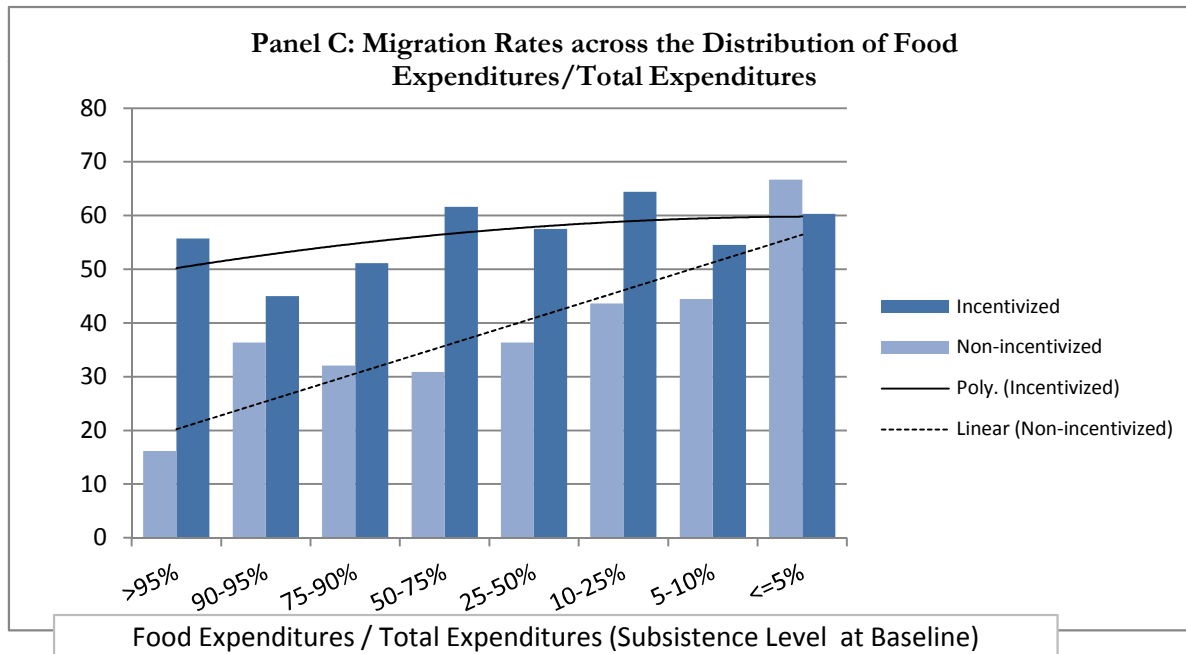
Figure 9. Static Model with Myopic Agents
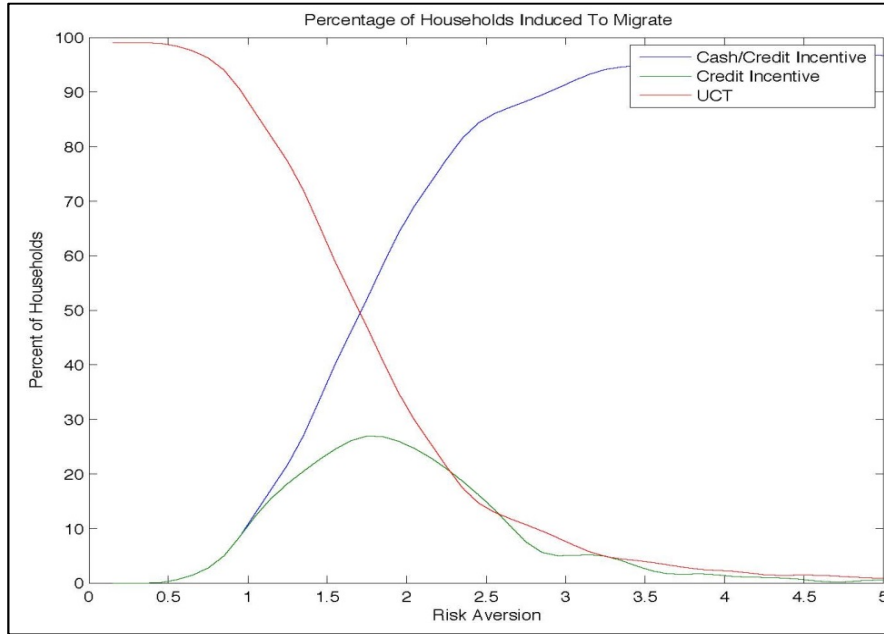
Panel A   Panel B

Figure 10. Forward-looking Agents, but no Savings

Panel A

Panel B

Figure 11. Full Model with Buffer Stock Savings and Possibility of Saving up for Migration

Panel A   Panel B

Figure 12.

Portion of Population Induceable
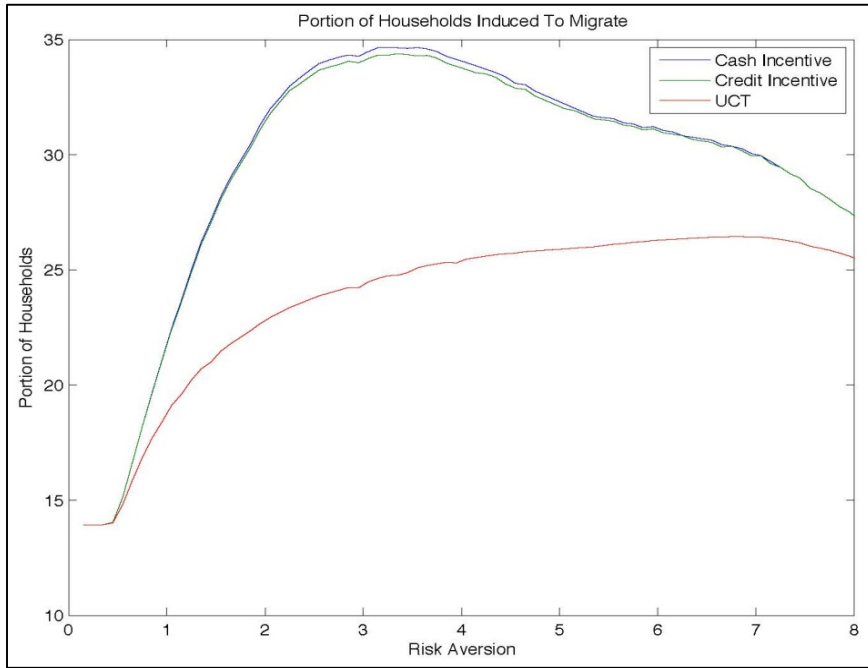
Appendices for Bryan, Chowdhury and Mobarak (2013)

Appendix 1, 2, 3

Appendix Tables

Appendix Figures

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:

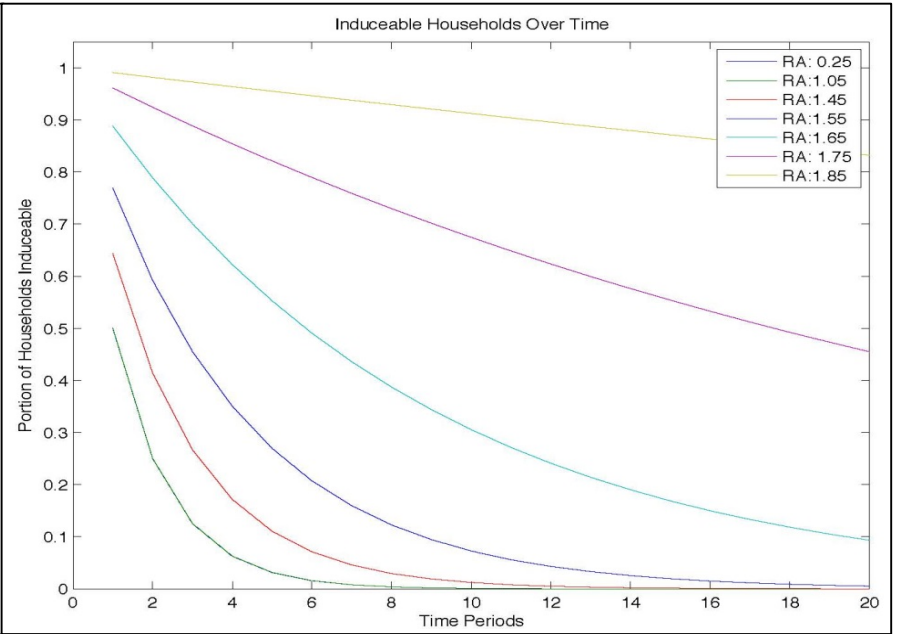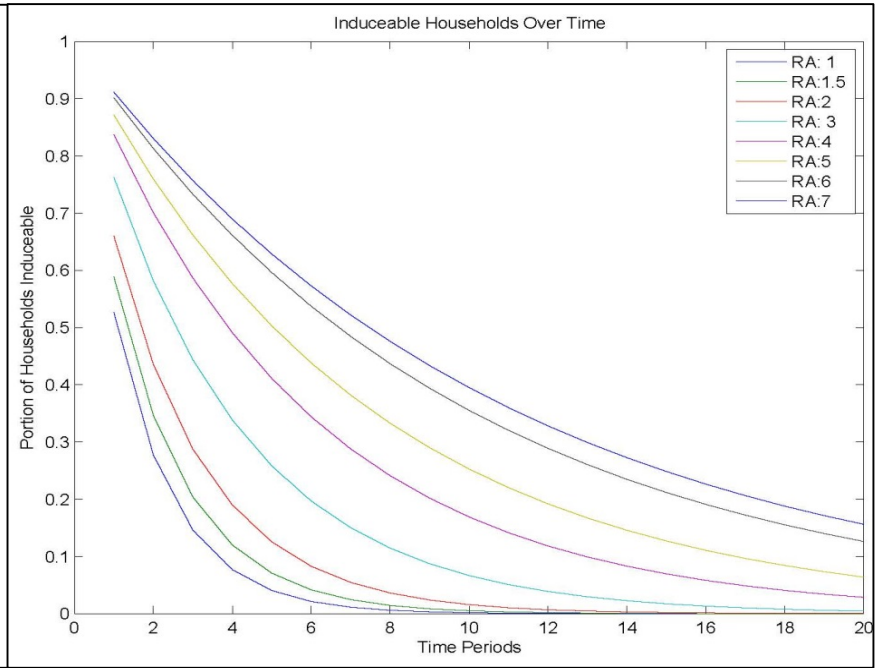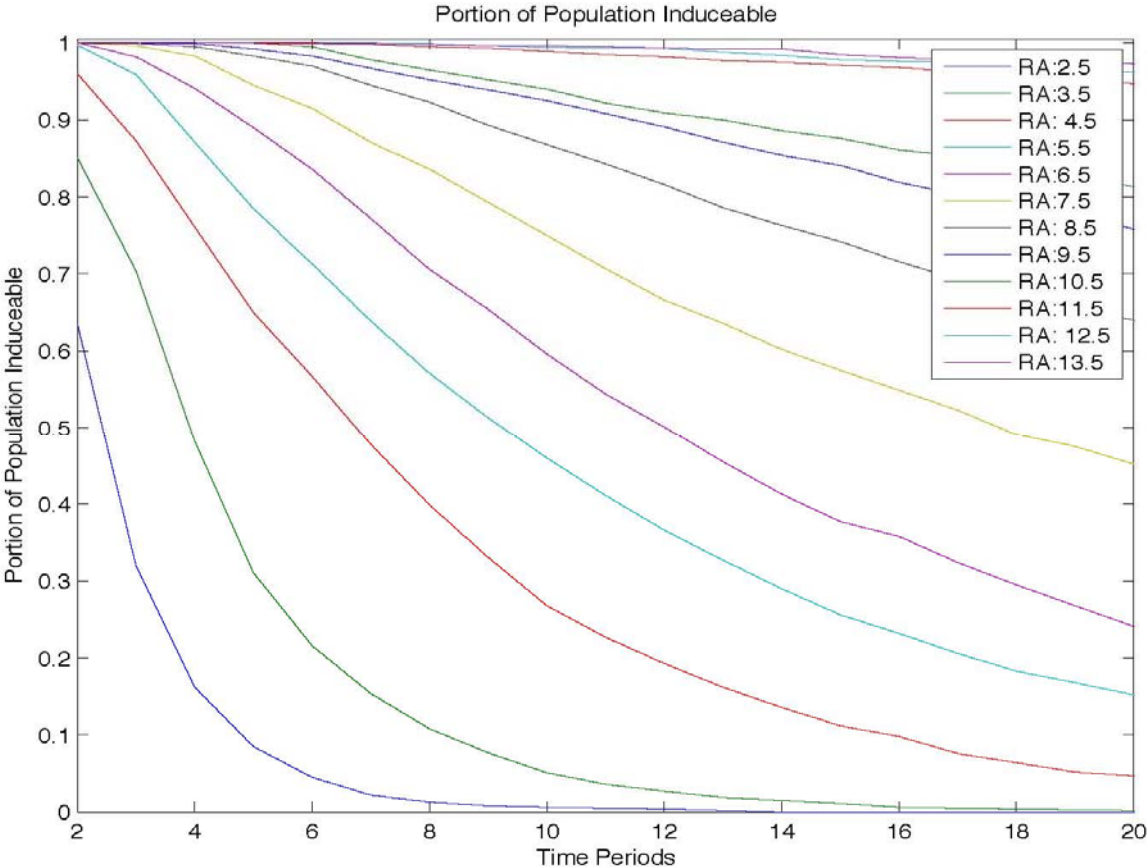**Information (16 villages/304 households):** Potential migrants were provided with information on the types of jobs available in each of four areas: Bogra, Dhaka, Munshigonj and Tangail. In addition, they were told the likelihood of finding such a job, and the average daily wage in each job. This information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

Appendix 2 : Description of Treatments in 2011

In 2011 we conducted one more round of randomized interventions in the same sample of 1900 households (in 100 villages), plus 247 new households in 13 new randomly selected villages from the same two districts (Kurigram and Lalmonirhat). The treatments (most of which encouraged migration, like the 2008 experiments) were randomized at the village level. They were offered in February, 2011, just before the onset of the 2011 "mini-Monga season," which is the pre-harvest lean season associated with the lesser of the two annual rice harvests. The treatments were therefore designed to encourage migration during this lean season. The same organization as in 2008 – PKSF, and their local NGO partners – implemented the treatments. We collected follow up data on all households in 133 villages in July-August 2011.

Controls: All 16 Control villages from the 2008 experiments were retained as a control group in 2011. We also chose not to intervene again in 19 villages that were offered the credit treatment in 2008. These 19 villages are labeled "Impure Control" in the regression table, and they allow us to study the long-run effects of offering migration credit in 2008.

Credit conditional on migration: Sample households in 15 villages received the same zero-interest loan conditional on a household member migrating, as offered in 2008. The credit amount was raised to Tk.800 (~US$10.8) to reflect inflation in the cost of travel since 2008. Households were required to pay back in a single installment in July, at the end of the lean season.

Unconditional credit: To test one of the implications of our model, we offered an *unconditional* zero-interest loan of Tk.800 to sample households in 15 villages. The loan repayment terms were the same as the conditional credit, and no conditionality was attached to the loan.

Conditional Credit with destination rainfall insurance: Sample households in 24 villages were offered the same zero interest Tk.800 (~US$10.8) credit conditional on migration, but the repayment terms were conditioned on rainfall outcomes in one popular migration destination: Bogra. Too much rainfall (and flooding) is a risk in Bangladesh, and can lower migrant earnings, particularly for outdoor work like rickshaw-pulling and construction site work. We purchased 10 years of daily rainfall data 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 loan contract specified that if rainfall in Bogra for March/April 2011 remained "normal" (4 days or less), the migrants would have to pay back Tk. 950 (~US$12.83). For 5-9 days of rainfall, the repayment requirement would be Tk.714 (~US$9.64). For 10 or more days of rainfall, the repayment requirement was Tk.640 (~US$8.64). The amounts were chosen to make the insurance contract actuarially fair, given historical rainfall data.

Note that this is a loan contract, but the repayment rules introduce a feature of index insurance against too much rainfall.[1] The treatment design takes advantage of the fact that the contract offers differential basis risk for households that differ along identifiable baseline characteristics: those who had a propensity for traveling to Bogra, and non-farmers. Basis risk from the index contract is lower for these two groups.

All treatments described above were proportionally balanced across the Information, Cash and Credit treatments from 2008 (and Control villages from 2008 were retained as long-term controls as described above). In some other sample villages from 2008, we conducted other treatments that are not relevant for the analysis conducted in this paper, and we therefore do not discuss those treatments here.

---

[1] 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 and Yang 2009).

Appendix 3 : Risk Aversion, Insurance and Basis Risk

This appendix provides a simple model of basis risk based on Clarke (2011) and uses it to argue that our 2011 insurance experiment can be used to test whether migration is risky and migrants are risk averse.[2]

There are two payoff relevant states *{L,H}* which lead to income at the destination $y_L \leq y_H$. We assume $pr(H) = \pi_y$. There are two rainfall states $\{R_L, R_H\}$ and rainfall insurance makes a payment of p in state R_H and costs c in state R_L. We denote $pr(R_L) = \pi_R$. This setup leads to four possible states of the world $\{LR_L, LR_H, HR_L, HR_H\}$. Following Clarke we parameterize basis risk with a variable $r = pr(LR_L)$ – that is, the probability that income is low but that the insurance contract does not payout and is in fact costly.[3] This implies that the remaining probabilities are

$$\{pr(LR_H) = 1 - \pi_R - r; pr(HR_L) = \pi_R - r; pr(HR_H) = \pi_y - \pi_R + r\}$$

We assume that r depends on the characteristics of the migrator. In particular, we assume:

1. Basis risk is larger for farmers than for non-farmers $r_F > r_{NF}$; and

2. Basis risk is smaller for those that are more likely to migrate to Bogra: $r^B < r^{NB}$.

We make the first assumption because the insurance contract pays in a high rain situation. High rain is likely to reduce income of day laborers who work, for example, pulling rickshaws. For agricultural laborers, however, high rain is potential advantageous as it is likely to increase work. We make the second assumption because the rainfall data is collected in Bogra and will be less accurate in other destinations. This leads to the possibility that we record high rainfall, but there is in fact low rainfall in, for

---

[2] See also Bryan (2012) for an application of the model presented here.

[3] Recall that our insurance project pays out in the high rainfall state.

example, Dhaka. We make no assumption about the relative basis risk for those that are farmer going to Bogra versus non-farmers that are not going to Bogra.

We are interested in deriving the relative impact of the provision of insurance on the migration rate. To do so, we suppose that potential migrators all face the same (expected) income given migration (i.e. there is no heterogeneity in the migration process except for r), but that potential migrators are heterogeneous with respect to their returns to remaining at home.[4] In particular, we assume that the expected utility of remaining at home is migrator specific and given by h_i which we assume to be distributed according to F.

Given these assumptions, the portion of potential migrators that migrate without insurance is given by

$$F\left(\pi_y u(y_h) + (1 - \pi_y)u(y_L)\right),$$

and with insurance by

$$F\left(ru(y_L - c) + (1 - \pi_R - r)u(y_L + p) + (\pi_R - r)u(y_H - c) + (\pi_y - \pi_R + r)u(y_H + p)\right)$$

If F does not depend on the type of migrator except, perhaps, through purely horizontal shifts, then the change in the probability of migration (or equivalently the portion of the population migrating) is proportional to

$$\left(ru(y_L - c) + (1 - \pi_R - r)u(y_L + p) + (\pi_R - r)u(y_H - c) + (\pi_y - \pi_R + r)u(y_H + p)\right) -$$
$$\left(\pi_y u(y_H) + (1 - \pi_y)u(y_L)\right).$$

Given this setup, we say that migration is risky if $y_H > y_L$. The model implies the following:

---

[4] This is easily generalized and our regressions presented in the main text allow for differences in the return to migration for farmers, non-farmers and those that are going to Bogra.

Proposition 1 (Basis risk is only relevant if migration is risky). *The portion of people induced to migrate by insurance is decreasing in r if and only if migration is risky and migrators are risk averse.*

Proof. The "only if" follows because r drops out of (1) when migration is not risky. The "if" follows because an increase in r is a mean preserving spread, so the left hand side of (1) must be decreasing in r so long as migrators are risk averse.

This proposition, combined with our assumptions on r leads to the following joint test:

Hypothesis 1 (Basis risk implies migration is risky). *If migration is risky, then rainfall insurance will increase migration rates more for those that are migrating to Bogra and more for non-farmers.*

The nature of this hypothesis is that, if the model of basis risk is correct, and our assumptions about the relative amounts of basis risk are correct, then we can infer that migration itself is risky from the results of our insurance experiment.

References

Bryan, G. (2012). *Ambiguity and insurance.* Unpublished manuscript.

Clarke, D. J. (2011). *A theory of rational demand for index insurance.* (Department of Economics Discussion Paper Series No. 572).University of Oxford. Retrieved from http://www.economics.ox.ac.uk/index.php/department-of-economics-discussion-paper-series/a-theory-of-rational-demand-for-index-insurance

Appendix Table 1. First Stage: Migration as a Function of Treatments in 2008

	Migration in 2008	
Cash	0.191***	0.192***
	(0.049)	(0.045)
Credit	0.177***	0.174***
	(0.048)	(0.045)
Info	0.001	0.003
	(0.056)	(0.052)
Sub-district fixed effects?	yes	yes
Additional controls?	no	yes
Observations	1,870	1,826
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 baseline.

Appendix Table 2. Intensive and Extensive Margin Changes due to Incentive (Cash or Credit) Treatment

	2008	2009	2011
Total number of migration episodes per household	0.378***	0.047**	0.024*
	(0.0700)	(0.021)	(0.014)
Total number of migrants per household	0.188***	0.020*	0.021**
	(0.0348)	(0.010)	(0.010)
Changes on Intensive Margin			
Total number of migration episodes per household (among migrant households)	0.108	0.106	-0.015
	(0.104)	(0.068)	(0.046)
Total number of migrants per household (among migrant households)	-0.0172	-0.009	0.013
	(0.0231)	(0.018)	(0.015)
Total number of episodes per migrant	0.125	0.106	-0.031
	(0.0980)	(0.067)	(0.037)
Days away per migrant per episode	-8.245**	-2.625	2.793**
	(4.118)	(3.383)	(1.400)
Male	0.0154	-0.004	-0.010**
	(0.0150)	(0.007)	(0.004)
Age	2.637**	0.144	-0.213
	(1.108)	(1.003)	(0.796)
Migrant is head of household	0.0674**	-0.021	0.017
	(0.0318)	(0.028)	(0.020)

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 "incentivized" (cash and credit groups in 2008 and 2009; conditional, unconditional credit, cash or rainfall insurance in 2011).

Appendix Table 3. Effects of Migration in 2008 on Consumption in 2008; Sensitivity to Changes in Definition of Household Size

Dependent Variable	ITT			ITT	IV	OLS
	Cash	Credit	Info			

Panel A: number of household members is based on question Q7 in R2 follow-up survey ("status of household members")

	Cash	Credit	Info	ITT	IV	OLS
Consumption of Food	49.674**	48.292**	20.427	39.033*	222.288*	-7.835
	(23.752)	(23.015)	(36.787)	(21.745)	(124.365)	(15.422)
Consumption of Non-Food	35.320**	28.121**	20.817	21.721**	122.929*	32.930***
	(14.941)	(14.046)	(18.860)	(10.348)	(63.274)	(8.621)
Total Consumption	104.162***	86.081***	41.620	75.234**	429.585**	61.339***
	(32.672)	(31.318)	(49.635)	(30.031)	(176.462)	(20.343)
Total Calories (per person per day)	120.927**	111.339**	-66.444	148.964***	869.842***	102.951***
	(54.673)	(51.398)	(68.194)	(42.735)	(243.784)	(38.129)

Panel B: number of household members is based on Q9 in R2 follow-up survey ("currently present members")

	Cash	Credit	Info	ITT	IV	OLS
Consumption of Food	50.506*	46.669*	5.063	46.219*	267.336**	67.936***
	(26.961)	(26.185)	(38.967)	(23.648)	(133.310)	(17.226)
Consumption of Non-Food	29.778**	25.690*	18.536	18.774*	106.119*	45.519***
	(13.686)	(13.495)	(18.144)	(9.917)	(59.272)	(9.152)
Total Consumption	80.085**	71.211**	23.634	64.328**	368.937**	112.357***
	(31.663)	(31.784)	(49.575)	(29.958)	(171.948)	(22.179)
Total Calories (per person per day)	69.645	77.571	-117.409	130.875***	775.485***	218.266***
	(65.251)	(62.278)	(76.655)	(48.946)	(274.635)	(41.640)

Panel C: household size is based on the total number of household members at the time of the interview

	Cash	Credit	Info	ITT	IV	OLS
Consumption of Food	56.019*	49.215*	21.065	42.498*	243.791*	80.573***
	(28.385)	(27.493)	(40.053)	(24.070)	(132.883)	(16.898)
Consumption of Non-Food	32.313**	27.335**	25.281	17.586*	98.361*	49.524***
	(13.170)	(12.594)	(17.941)	(9.593)	(56.223)	(8.738)
Total Consumption	88.138**	75.440**	46.380	59.440*	337.769**	129.019***
	(34.016)	(33.216)	(51.202)	(30.518)	(170.467)	(21.769)
Total Calories (per person per day)	90.556	91.954	-69.585	125.294***	737.107***	252.609***
	(60.478)	(56.772)	(75.689)	(46.656)	(249.228)	(40.847)

Robust standard errors in parentheses, clustered by village. *** p<0.01, ** p<0.05, * p<0.1.

Continued: Appendix Table 3: Effects of Migration in 2008 on Consumption in 2008; Sensitivity to Changes in Definition of Household Size

Dependent Variable	ITT			ITT	IV	OLS
	Cash	Credit	Info			
Panel D: household size is based on the total number of household members present in the last 14 days						
Consumption of Food	65.320**	52.001*	16.532	50.952**	294.218**	114.443***
	(29.708)	(29.165)	(40.476)	(24.395)	(130.921)	(17.779)
Consumption of Non-Food	37.317***	28.879**	22.655	22.246**	126.026**	63.824***
	(13.105)	(12.307)	(17.403)	(9.709)	(56.518)	(9.154)
Total Consumption	102.441***	79.753**	39.221	72.541**	415.549**	177.147***
	(35.327)	(34.650)	(51.050)	(30.846)	(167.430)	(22.851)
Total Calories (per person per day)	115.229*	97.084	-83.808	147.739***	872.820***	350.271***
	(65.440)	(63.041)	(77.209)	(48.055)	(243.244)	(41.971)
Panel E: Total monthly consumption per household; no adjustment to household size						
Consumption of Food	68.356	58.472	-29.407	78.084	454.672	-22.104
	(125.876)	(126.579)	(171.409)	(104.435)	(584.120)	(59.784)
Consumption of Non-Food	81.562*	53.790	60.009	39.126	219.877	41.280
	(41.239)	(40.458)	(48.636)	(31.682)	(179.086)	(25.780)
Total Consumption	149.230	108.306	30.727	114.917	660.329	15.572
	(143.280)	(145.175)	(203.232)	(125.865)	(701.793)	(74.566)
Total Calories (per person per day)	-9.354	-21.278	-426.987	193.855	1,169.733	22.695
	(279.707)	(274.067)	(342.132)	(225.931)	(1,245.768)	(166.249)

Robust standard errors in parentheses, clustered by village. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 4. Effects of Migration in 2008 on Savings, Earnings and Changes in Children's Middle Upper Arm Circumference (MUAC)

Dep. Var.:	Total Savings by household		Total Earnings by household		MUAC (mm)		Change in MUAC (mm)	
	ITT	IV	ITT	IV	ITT	IV	ITT	IV
Incentives (Cash or Credit)	591.617***		585.653		1.929		0.744	
Treatment	(170.718)		(708.002)		(1.315)		(0.951)	
Migration (before Dec 2008),		3,287.602***		3,281.877		11.059		4.474
instrumented by treatment		(869.377)		(3,773.748)		(7.944)		(5.348)
Controls?	No	No	No	No	No	No	No	No
Observations	1,851	1,851	1,851	1,851	1,854	1,854	1,836	1,836
R-squared	0.052	0.285	0.026	0.103	0.031	-0.034	0.017	-0.005
Mean of Control	2272	2272	14244	14244	205.2	205.2	-3.971	-3.971

Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$ Total earnings include earnings from migration and earnings at the origin from all sources, including (1) total earnings for daily wage-earners and in-kind; (2) self-employment; (3) livestock; fishery; forestry.

Appendix Table 5. Covariance of income per capita across rounds.

	Consumption in R2	Consumption in R3	Consumption in R3
Consumption per capita in R1	0.102***		0.067***
	(0.014)		(0.012)
Consumption per capita in R2		0.445***	
		(0.027)	
Constant	881.546***	765.099***	1,094.635***
	(18.215)	(25.513)	(15.676)
Sub-district FE?	no	no	no
Observations	1,855	1,782	1,798
R-squared	0.027	0.131	0.017

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

Appendix Table 6. Summary Statistics on Households Savings

	Baseline		Follow-up 2008		Follow-up 2011		Total	
	Mean	St.dev	Mean	St.dev	Mean	St.dev	Mean	St.dev
Share with positive current savings	0.53	0.50	0.57	0.50	0.34	0.48	0.49	0.50
Total value of current cash savings for all HHs	745.45	1,629.28	787.04	1,616.97	768.33	2,280.19	798.83	1,885.58
Total value of current cash savings for HHs with reported savings	1,416.36	2,023.58	1,385.29	1,942.77	2,233.72	3,442.41	1,624.94	2,427.08
Share with liquid assets	0.42	0.49	0.59	0.49	0.81	0.39	1,556.14	3,018.54
Total value of liquid assets for all HHs	339.35	1,154.88	494.58	1,292.40	1,390.12	3,115.53	0.60	0.49
Total value of liquid assets for HHs with reported assets	812.05	1,676.18	844.30	1,599.04	1,709.12	3,374.84	1,269.59	2,712.63
1 if purchased assets in last 12 months (all HHs)	0.01	0.09	0.01	0.09	0.20	0.40	757.45	2,185.75
Value of purchased assets in the last 12 months	6.26	89.65	9.37	195.36	122.89	1,476.58	0.07	0.26
Total savings (current + liquid assets) for all HHs	1,084.80	2,057.72	1,281.62	2,185.67	2,157.30	4,028.99	41.36	549.42
Total savings (current + liquid assets) for HHs with reported savings or assets	1,547.39	2,307.55	1,588.02	2,330.90	2,530.66	4,254.20	1,979.27	3,279.01
Obs	1900		1871		2413		5777	

Cash savings are the total of any cash holdings by all household members (held in any location). Liquid asset value is the reported value of all non-property assets, including stocks, bonds, other financial assets and jewelry.

Appendix Table 7. Going Back to the Same Employer in 2011

	Full sample
Incentivized in 2008	0.047*
	(0.027)
Constant	0.266***
	(0.020)
Observations	2,771
R-squared	0.003

Notes. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The dependent variable in "Full sample" is equal to 1 if a respondent reports going to the same employer in 2011 as before; 0 otherwise. The dependent variable in "Migrant only" is equal to 1 if a respondent reports going to the same employer in 2011 as before; it is equal to 0 if a migrant reports going to a different employer as before (of migrants who travelled to the same place as before).

Appendix Table 8a. Proportion of 2011 migrants who first met employer before or after migration incentive (2006-2007 vs 2008-2009), Incentivized in 2008 only [Migrant Only Sample; Non-experimental]

	2007	2008	Difference	P-value	Obs
First met employer in 2007 vs 2008	0.42	0.58	-0.17	0.0941	103
	(0.05)	(0.05)			
	2006-7	**2008-9**	Difference	P-value	Obs
First met employer in 2006-7 vs 2008-9	0.43	0.57	-0.13	0.0567	201
	(0.04)	(0.04)			

Standard errors in parentheses. This table shows the proportion of migrants who were incentivized in 2008, who re-migrated in 2011, returned to the same place and met their employer between 2006 and 2009.

Appendix Table 8b. Proportion of 2011 migrants who first met employer before or after incentivization (2006-2007 vs 2008-2009), Full Sample [Migrant Only Sample; Non-experimental]

|	Not Incentivized	Incentivized	Difference	P-value	Obs
First met employer in 2008 (rather than 2007)	0.50	0.58	-0.08	0.2589	189
	(0.05)	(0.05)			
	Not Incentivized	**Incentivized**	Difference	P-value	Obs
First met employer in 2006-07 rather than 2008-09	0.54	0.57	-0.03	0.5672	363
	(0.04)	(0.04)			

Standard errors in parentheses. This table shows the proportion of all migrants from 2008 who re-migrated in 2011, returned to the same place and met their employer between 2006 and 2009.

Appendix Table 9. First stage of Instrumental Variables Regression for Destination Choices

| Dep. Var.: Migrated in 2008 to:	Dhaka	Bogra	Tangail	Munshigonj
Cash	-0.032	0.125**	-0.052	0.010
	(0.088)	(0.051)	(0.075)	(0.083)
Credit	0.035	0.085*	0.017	-0.056
	(0.088)	(0.048)	(0.077)	(0.083)
Info	0.009	0.052	0.016	0.019
	(0.102)	(0.049)	(0.088)	(0.094)
Group formation - self-formed	-0.045	0.022	-0.022	-0.011
	(0.046)	(0.053)	(0.054)	(0.051)
Group formation - assigned	-0.001	0.053	-0.041	0.008
	(0.058)	(0.057)	(0.051)	(0.049)
Group formation - two people	-0.048	-0.018	0.059	0.054
	(0.050)	(0.052)	(0.066)	(0.072)
Destination assigned	-0.020	-0.059*	-0.078*	-0.007
	(0.044)	(0.033)	(0.045)	(0.037)
Assigned to Dhaka	0.054			
	(0.068)			
Assigned to Bogra		0.234***		
		(0.066)		
Assigned to Tangail			0.305***	
			(0.084)	
Assigned to Munshigonj				0.163**
				(0.080)
Constant	0.427***	-0.075*	0.142*	0.295
	(0.148)	(0.043)	(0.072)	(0.187)
Observations	589	589	589	589
R-squared	0.092	0.103	0.197	0.097

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors in parentheses.

Appendix Figure 1. Distribution of consumption per person per month by baseline consumption decile
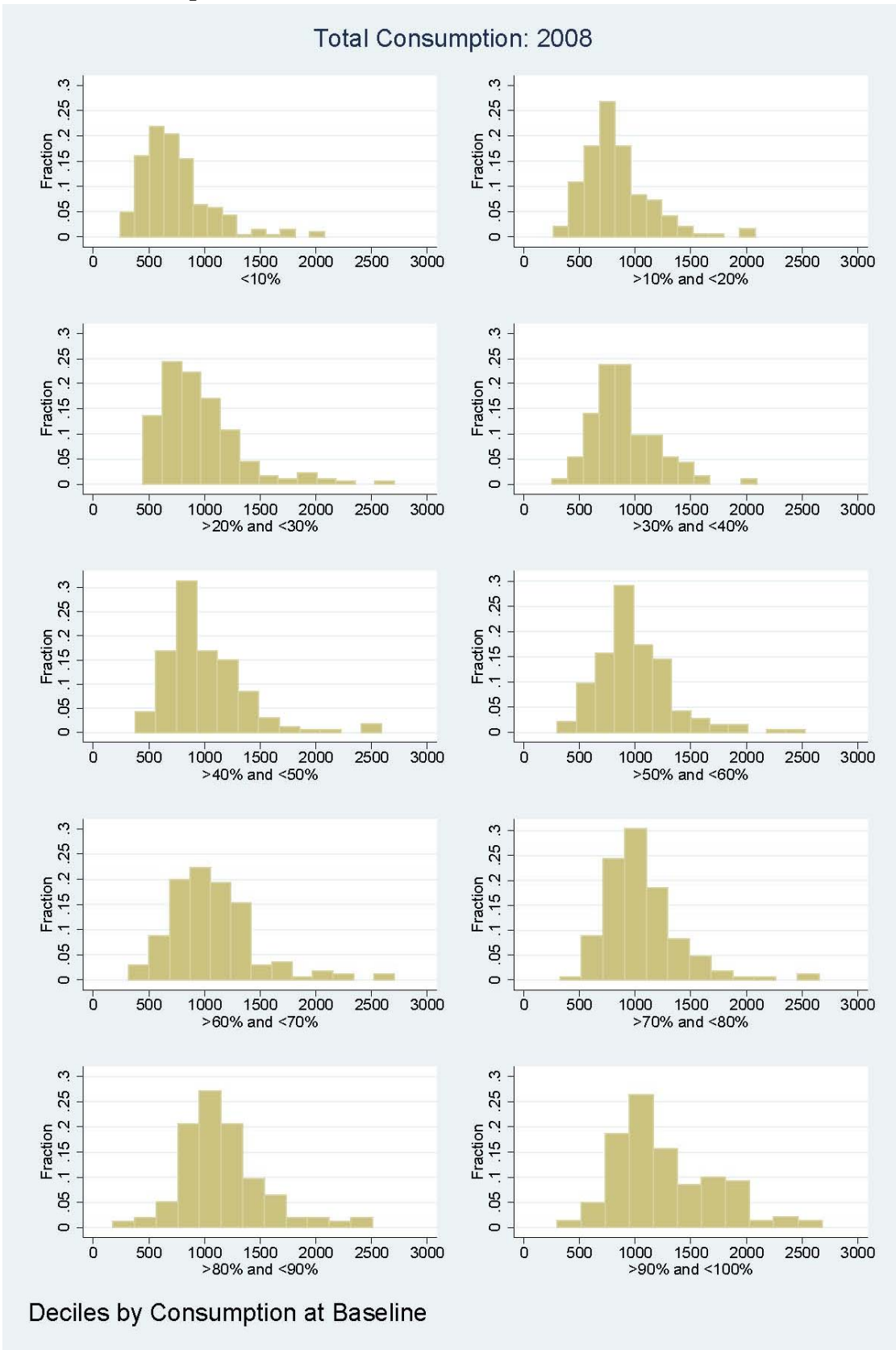
Consumption reflects total expenditures per person per month.

Appendix Figure 2. Distribution of Calories per Person per Day in 2008
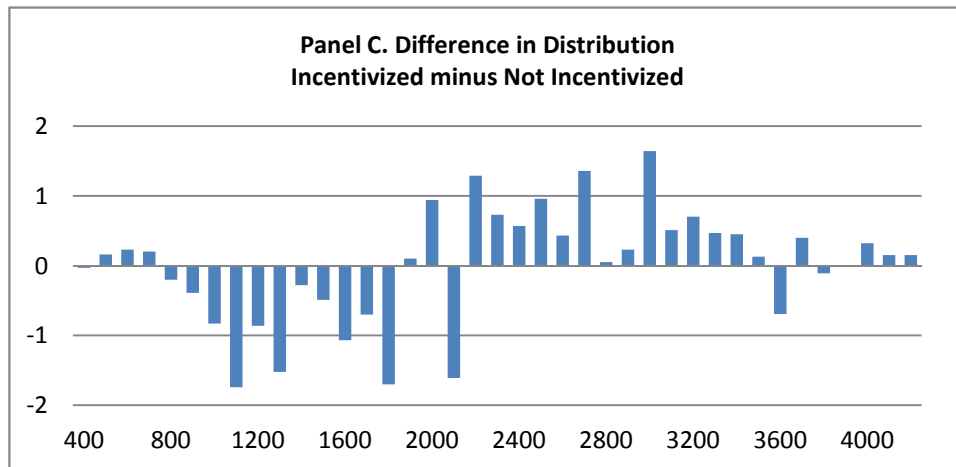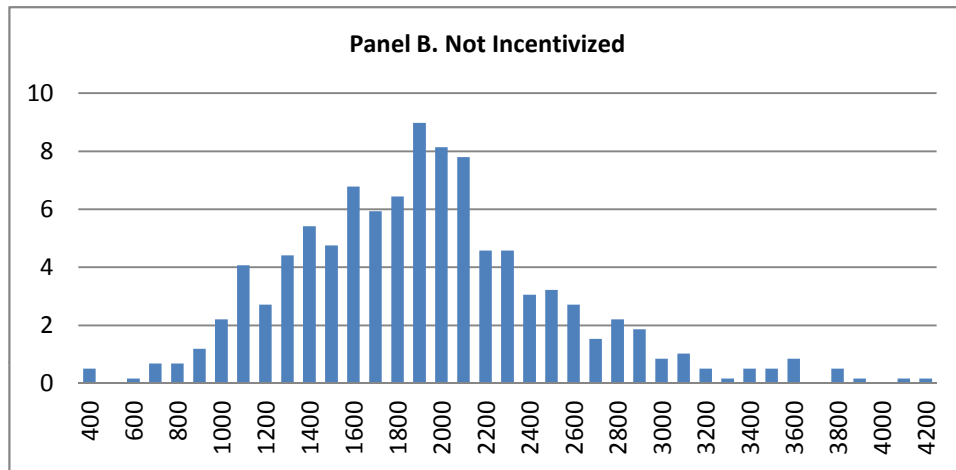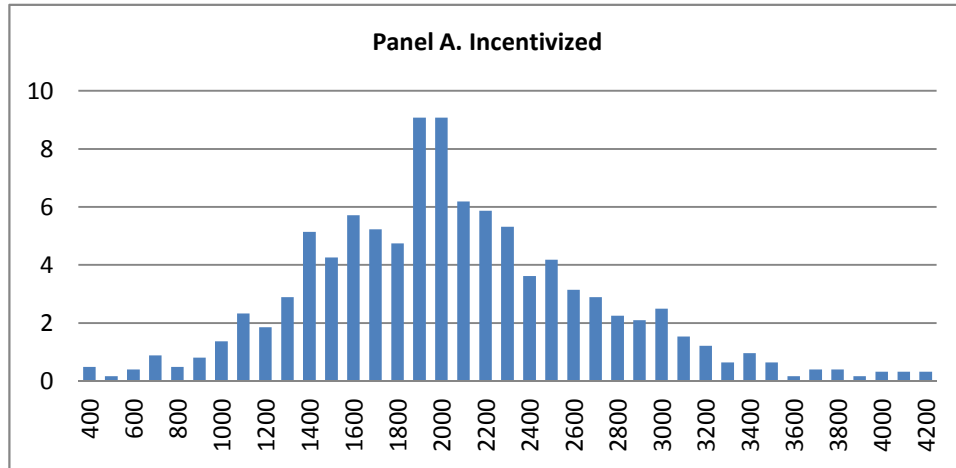
Notes: These histograms show the distribution of calories per person per day at baseline. Household size is based on the total number of household members at the time of the interview