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UNDERINVESTMENT IN A PROFITABLE TECHNOLOGY: THE CASE OF SEASONAL MIGRATION IN BANGLADESH

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UNDERINVESTMENT IN A PROFITABLE TECHNOLOGY: THE CASE OF SEASONAL MIGRATION IN BANGLADESH

BY GHARAD BRYAN, SHYAMAL CHOWDHURY, AND AHMED MUSHFIQ MOBARAK[1]

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 temporarily 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 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, technology adoption, Bangladesh, risk.

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. 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.[2] The proximate causes of the famine season are easily

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[2]Seasonal poverty has been documented in Ethiopia (Dercon and Krishnan (2000)), where poverty and malnourishment increase 27% during the lean season, Mozambique and Malawi

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understood—work opportunities are scarce between planting and harvest in agrarian areas, and grain prices rise during this period (Khandker and 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 assign 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 by difficult selection issues (Akee (2010), Grogger and Hanson (2011)).[3] Most closely related to our work are 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.[4]

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.

(Brune, Gine, Goldberg, and Yang (2011)), where people refer to a "hungry season," Madagascar, where Dostie, Haggblade, and Randriamamonjy (2002) estimated that 1 million people fall into poverty before the rice harvest, Kenya, where Swift (1989) distinguished between years that people died and years of less severe shortage, Francophone Africa (the soudure phenomenon), Indonesia (Basu and Wong (2012)) ('musim paceklik' or 'famine season' and 'lapar biasa' or 'ordinary hunger period'), Thailand (Paxson (1993)), India (Chaudhuri and Paxson (2002)), and inland China (Jalan and Ravallion (2001)).

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

[4]A related literature studies the effects of exogenous changes in destination conditions on remittances, savings, and welfare at the origin (Martinez and Yang (2005), Aycinena, Martinez, and Yang (2010), Chin, Karkoviata, and Wicox (2010), Ashraf, Aycinena, Martinez, and Yang (2014)).

These large effects on migration rates, consumption, and re-migration suggest that a policy of encouraging migration may have substantial benefits. However, to understand the settings to which these results might apply, and optimal policy responses, it is necessary to confront an important puzzle: why did our subjects not already engage in such highly profitable behavior? An answer to this question would allow a characterization of settings in which encouragement to adopt new technologies or behaviors is likely to lead to similar positive outcomes, and provide some policy guidance. The 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 underpredict 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 and Williamson (1998)). Ardington, Case, and Hosegood (2009) provided similar evidence of constraints preventing profitable out-migration in rural South Africa.

The second part of our paper rationalizes the experimental results using a simple benchmark model in which experimenting with a new activity is risky, and rational households choose not to 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. This last aspect of our model means that it is important for individuals to experience migration for themselves; they cannot learn about returns from others. 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, Parente, and Rogerson (2002), Caselli (2005), Restuccia, Yang, and Zhu (2008), Vollrath (2009), Gollin, Lagakos, and Waugh (2011), McMillan and 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 and Laffont (1979), Banerjee and 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 and Gollin (2003)), but adoption and diffusion of the new technologies were slow, partly due to low levels of experimentation and the resultant slow learning (Munshi (2004), BenYishay and Mobarak (2013)). 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 and Rosenzweig (2011)).[5] Similarly, to counter the surprisingly low adoption rates of effective health products (Mobarak, Dwivedi, Bailis, Hildemann, and Miller (2012), Meredith, Robinson, Walker, and Wydick (2013)), we may need to give households the opportunity to experiment with the new technology (Dupas (2014)), perhaps with free trial periods and other insurance schemes. Aversion to experimentation can also hinder entrepreneurship and business start-ups and growth (Hausmann and 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 further predictions of the model. To distinguish our explanation from failure to migrate due to a liquidity constraint, we show that migration is more responsive to incentives (e.g., credit *conditional* on migration) than to *un*conditional credit. We also implement another new treatment providing insurance for migration, and this offer induces just as many households to migrate. Further, they respond to the insurance program 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 shows that, quantitatively, our model does not offer a fully satisfying explanation for the migration phenomena.[6] Once agents

[5]The inability to experiment due to uninsured risk has been linked to biases toward low-risk low-return technologies that stunt long-run growth (Mobarak and Rosenzweig (2013)), and to reduced investments in agricultural inputs and technologies such as new high-yield variety seeds and fertilizer (Rosenzweig and Wolpin (1993), Dercon and Christiaensen (2011)).

[6]We adapt the highly influential buffer stock saving model that is the backbone of much modern macroeconomic modeling. For example, see Kaboski and Townsend (2011).

in our model are allowed to save up to migrate, they do so rapidly. The model implies that, for reasonable levels of risk aversion, there should be very few households that have not tried migrating, and therefore very few households that would be induced to migrate by our interventions. We conclude that the level of risk aversion required to quantitatively account for our data appears to be implausibly high.

In the light of these results, we believe that our work leads to three main conclusions. First, our experimental results show that migration in this setting is very profitable, and in some sense underutilized. Second, our qualitative exploration of the model shows that the three components of risk, incomes close to subsistence, and learning about the returns to migration are important elements in explaining the low utilization. Our positive results are likely to be replicable in settings with these three characteristics. Third, our quantitative results show that we do not fully understand the migration choices of these households: there is some important aspect of their choices that we are not capturing. This final challenge leads us to briefly consider some departures from full information and rationality and other market imperfections (such as savings constraints). Ultimately, however, we lack the data to determine what ingredient would provide a fully satisfying characterization of the behavior we observe, and leave this to future research. Because we cannot fully rationalize the behavior, we advocate care in interpreting our model: any additional element that is needed to match the data may change the conclusions from our baseline model.

The next two sections describe the context and the design of our interventions. We present program evaluation results 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 northwestern Bangladesh. The Rangpur region is home to roughly 7% of the country's population, or 9.6 million people. Fifty-seven percent of the region's population (or 5.3 million people) live below the poverty line.[7] 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,

[7]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.

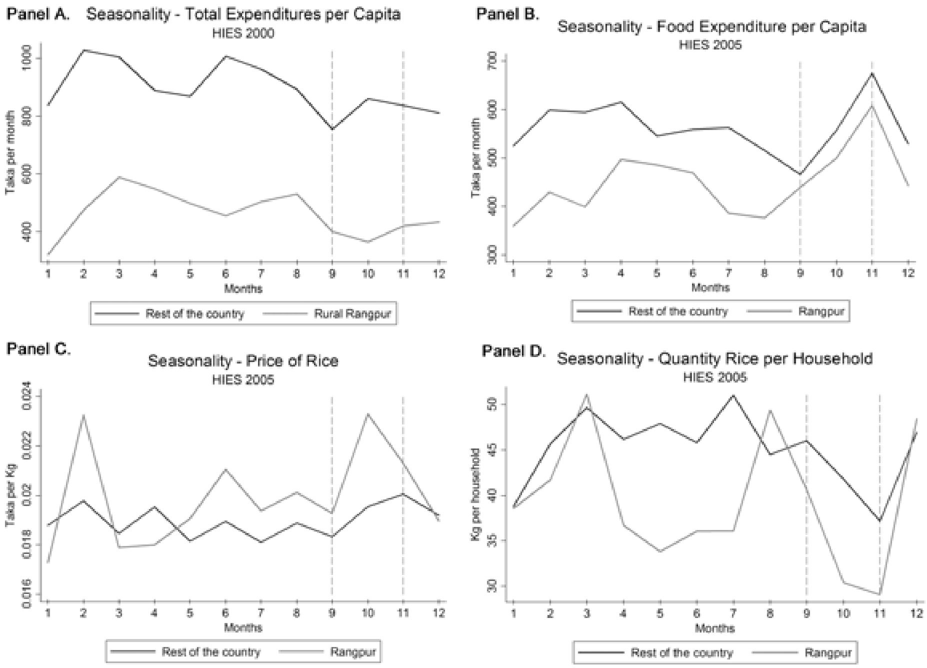


FIGURE 1.—Seasonality in consumption and price in Rangpur and in other regions of Bangladesh. *Source*: Bangladesh Bureau of Statistics 2005 Household Income and Expenditure Survey.

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 Aman harvest, which is the main rice crop in Bangladesh (Khandker and 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 toward food while waiting for the harvest.

The lack of job opportunities and low wages during the pre-harvest season and the coincident increase in grain prices combine to create a situation of seasonal deprivation and famine (Sen (1981), Khandker and Mahmud (2012)).[8] 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 con-

[8]Amartya Sen (1981) noted 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.

sumption below subsistence for Rangpur households, where agricultural wages are already the lowest in the country (Bangladesh Bureau of Statistics (2011)).

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 and Duflo (2007)).

Second, inter-regional variation in income and poverty between Rangpur and the rest of 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)) noted 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) pointed 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 microcredit 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 toward 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.[9] In

[9]Seventy-one percent 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,

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.[10] 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. Sixteen 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 preselected destinations, the likelihood of getting such a job, and approximate wages associated with each type of job and destination (see Appendix A for details). Seven hundred three households in 37 randomly selected villages were offered cash of 600 Taka (~US$8.50) at the origin conditional on migration, and an additional bonus of 200 Taka (~US$3) if the migrant reported to us at the destination during a specified time period. We also provided exactly the same information about jobs and wages to this group as in the information-only treatment. Six hundred 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.[11]

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 as-

and our sample is therefore representative of the poorer 56% of the rural population in the two districts.

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

[11]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) 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 round-trip travel cost was reported to be 450 Taka, or 529 Taka including the cost of food and other incidentals during the trip.

signed 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 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 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 learned 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 were collected over three rounds: in July 2008 (baseline), December 2008, and December 2009. Migration behavior data were collected in December 2008, May 2009, December 2009, and July 2011, which jointly cover three seasons in 2008, 2009, and 2011.

Table I shows that there was pretreatment 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 on migration in 2009 and 2011 (one and three 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 (ITT) and local average treatment effect (LATE) estimates for consumption in December 2008, and

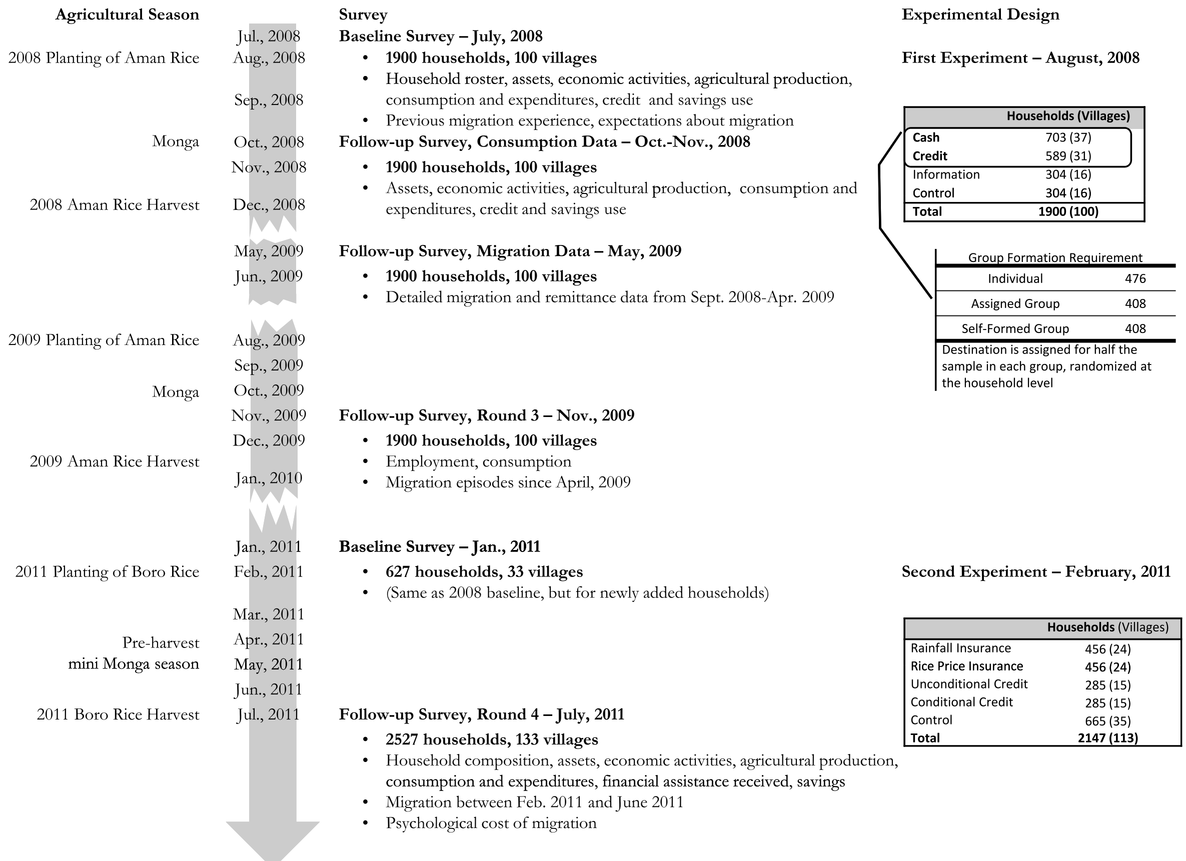


FIGURE 2.—Trial profile and timeline.

TABLE I

RANDOMIZATION BALANCE ON OBSERVABLES AT BASELINE[a]

	Incentivized		Non-Incentivized			
	Cash	Credit	Control	Info	Diff. (I − NI)	*p*-Value
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	2081.19	2079.51	2099.3	2021.31	20.25	0.585
(per person per day)	(20.34)	(22.76)	(30.44)	(32.56)	(36.99)	
Calories from protein	45.66	45.3	46.26	44.75	−0.01	0.992
(per person per day)	(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 and 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	1345.55	1366.37	1418.29	1611.05	−160.56	0.255
(conditional on positive savings)	(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	0.25	0.24	0.25	0.22	0.01	0.628
1 = Educated	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	
Number of males	1.19	1.22	1.18	1.18	0.03	0.515
Age > 14	(0.02)	(0.02)	(0.03)	(0.03)	(0.04)	
Number of children	1.01	1.05	1.08	1.15	−0.09	0.093
Age < 9	(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)	

(*Continues*)

TABLE I—*Continued*

	Incentivized		Non-Incentivized			
	Cash	Credit	Control	Info	Diff. (I − NI)	p-Value
Subjective expectation: Monga occurrence this year	78.79 (0.77)	78.62 (0.88)	78.38 (1.15)	75.72 (1.35)	1.66 (2.32)	0.47
Subjective expectation: Will get social network help in Dhaka	58.53 (1.07)	60.82 (1.21)	58.38 (1.64)	57.40 (1.61)	1.68 (2.04)	0.41
Subjective expectation: Can send remittance from Dhaka	52.53 (1.13)	52.90 (1.25)	52.42 (1.78)	51.15 (1.72)	0.91 (2.40)	0.70
Ratio of food expenditure over total consumption in round 1	0.77 (0.003)	0.75 (0.09)	0.77 (0.01)	0.77 (0.004)	−0.01 (0.01)	0.21
Average skill score received by network	6.53 (0.05)	6.49 (0.27)	6.24 (0.07)	6.20 (0.07)	0.27 (0.23)	0.24
Applied and refused for credit or did not apply because of insufficient collateral	0.03 (0.01)	0.04 (0.004)	0.04 (0.01)	0.04 (0.01)	−0.00 (0.01)	0.75
Received credit from NGO, family and friends, or money lender	0.68 (0.02)	0.65 (0.02)	0.70 (0.03)	0.60 (0.03)	0.02 (0.04)	0.55
Migration to Bogra in round 1	0.11 (0.01)	0.10 (0.01)	0.16 (0.02)	0.12 (0.02)	0.03 (0.03)	0.30

[a]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. Differences and p-values are derived from linear regression the variable of interest as the dependent variable, a binary variable equal to 1 if treatment group and 0 else as the independent variable; 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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

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 II 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,[12] and for capturing the migration experiences of those who left and/or returned later. The two sets of reports were

[12]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.

TABLE II
PROGRAM TAKE-UP RATES[a]

	Incentivized	Cash	Credit	*Not Incentivized*	Info	Control	*Diff. (I − NI)*
Migration rate in 2008	58.0% (1.4)	59.0% (1.9)	56.8% (2.1)	36.0% (2.0)	35.9% (2.8)	36.0% (2.8)	22.0*** (2.4)
Migration rate in 2009	46.7% (1.4)	44.6% (1.9)	49.1% (2.1)	37.5% (2.0)	34.4% (2.8)	40.5% (2.9)	9.2*** (2.5)
Migration rate in 2011[b]	39% (2.1)			32% (2.5)			7.0** (3.3)

[a]Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Diff. Incentivized − Not Incentivized tests the difference between migration rates of incentivized and non-incentivized households, regardless of whether they accepted our cash or credit. No incentives were offered in 2009.

[b]For re-migration rate in 2011, we compare migration rates in control villages that never received any incentives to the subset of 2008 treatment villages that did not receive any further incentives in 2011. Note that migration was measured over a longer period (covering the main monga season) in 2008 and 2009, and a different time period (the mini-monga season) in 2011.

quite consistent with each other, and the first row of Table II shows the more complete 2008 migration rates obtained in May 2009.

In Table II, 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.[13] Providing information about wages and job opportunities at the 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 this information, 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 is 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

[13]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, Khaleque, and Samad (2011)). Our result appears very consistent 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.

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 second and third rows of Table II compare 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 2008. Strikingly, the migration rate in 2009 was 9 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 re-migration amongst a subset who were induced to migrate in 2008. 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 7 percentage points higher in the villages randomly assigned to the cash or credit treatment in 2008.[14]

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.[15] Second, the persistence of re-migration from 2009 to 2011 (with four potential migration seasons in between) suggests that households learned 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. Consump-

[14]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.

[15]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.

tion is a broad and useful measure of the benefits of migration, aggregating as it does the impact of migrating on the whole family (Deaton (1997)), and takes into account the monetary costs of investing (although it neglects non-pecuniary costs). Consumption can be comparably measured for migrant and non-migrant families alike, and it 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 III 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 subdistrict j in 2008, and φ_j are fixed effects for subdistricts. 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 III 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. Panel A studies the effects on 2008 consumption, while Panel B examines consumption measured in December 2009, after the next monga season. 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.

Panel A shows that both the cash and credit treatments—which induced 21–24% more migration—result in statistically significant increases in food and non-food consumption in 2008. Total consumption 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.

Since both cash and credit treatments led to greater migration (Table II), 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.

TABLE III

EFFECTS OF MIGRATION BEFORE DECEMBER 2008 ON CONSUMPTION AMONGST REMAINING HOUSEHOLD MEMBERS[a]

	ITT								
	Cash	Credit	Info	ITT	ITT	IV	IV	OLS	Mean
				Panel A: 2008 Consumption					
Consumption of food	61.876**	50.044*	15.644	48.642**	44.183*	280.792**	260.139**	102.714***	726.80
	(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***	274.46
	(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***	1000.87
	(34.610)	(33.646)	(50.975)	(30.593)	(29.683)	(169.431)	(158.835)	(22.061)	
Total calories	106.819*	93.429	−85.977	142.629***	129.901***	842.673***	757.602***	317.495***	2090.26
(per person per day)	(62.974)	(59.597)	(76.337)	(47.196)	(48.057)	(248.510)	(250.317)	(41.110)	

(*Continues*)

TABLE III—*Continued*

	ITT								
	Cash	Credit	Info	ITT	ITT	IV	IV	OLS	Mean
				Panel B: 2009 Consumption					
Consumption of food	34.273	22.645	−30.736	43.983**	34.042*	230.811**	186.279*	1.687	872.69
	(23.076)	(23.013)	(29.087)	(17.589)	(18.110)	(100.536)	(96.993)	(14.687)	
Consumption of non-food	3.792	31.328*	−8.644	21.009*	14.877	110.324*	74.216	6.133	323.31
	(16.186)	(18.135)	(20.024)	(11.954)	(12.031)	(65.333)	(63.792)	(10.312)	
Total consumption	38.065	53.973	−39.380	64.992***	48.919*	341.135**	260.495**	7.820	1196.01
	(30.728)	(34.057)	(39.781)	(23.958)	(24.713)	(137.029)	(131.851)	(21.044)	
Total calories	83.242	23.995	−81.487	95.621**	78.564*	510.327**	434.602**	20.361	2001.27
(per person per day)	(52.766)	(62.207)	(60.141)	(39.187)	(40.600)	(221.010)	(216.670)	(28.392)	
Controls?	No	No	No	No	Yes	No	Yes	No	

[a] 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.

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.[16] 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 A.I 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 A.II 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.[17] However, the treatment does make it more likely that older, heads of households become more likely to migrate.

[16]Since the incentive arm included the information script, it may in theory have altered the behavior of households that did not migrate, which would threaten the exclusion restriction. We have verified that the information-only treatment had no effect (relative to control) on savings in 2008 or migration in 2009, or a broader range of outcomes. It is therefore unlikely that the information component of the incentive treatment had independent effects that violate the exclusion restriction. Furthermore, all incentive treatment effects we report in this section are robust to omitting the information-only group entirely, and comparing only to the pure control arm.

[17]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.

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 (baseline income has a negative coefficient in the first-stage regression in Appendix Table A.I). 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, De Weerdt, and Dercon (2011) for Tanzania. Finally, none of the results discussed above are sensitive to changes in baseline control variables.

In terms of magnitude of effects, monthly consumption among migrant families increases 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 on transportation costs two months prior to the consumption survey.

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 III $*$ 4 household members $*$ 2 months) during the monga by incurring a migration cost 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 both increases 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). Panel B of Table III 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. 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 lead to long-run consumption gains.[18] Appendix Table A.IV examines effects on a few detailed categories of consumption. We focus on protein consumption, because this is a marker of welfare in very poor populations. We see that migration leads to some statistically and quantitatively significant increase in the consumption of protein, especially from meat and fish (but not milk and eggs). For the Bangladesh context, this reflects a shift toward a higher quality diet, as meat and fish are considered more attractive, "tasty" sources of protein. Educational expenditures on children also increase significantly, but there is no significant change in medical expenditures for males or females. There are no changes in female labor force participation, school attendance, or agricultural investment.

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, and these are not experimental estimates; they merely help to calibrate the consumption results. Table IV 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 A.V 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

[18]Since the migration decision is serially correlated, measurement error in 2009 migration dates can also bias our estimates. Appendix Table A.III shows the results of a number of other sensitivity checks on the consumption results by varying the definition of household size (the denominator). The results are robust.

TABLE IV

MIGRANT EARNINGS AND SAVINGS AT DESTINATION (DATA FOR MIGRANTS ONLY; NON-EXPERIMENTAL)[a]

	All Migrants	Incentivized	Not Incentivized	Diff.	Observations
Total savings by household	3490.47	3506.59	3434.94	71.65	951
	(97.22)	(110.83)	(202.80)	(232.91)	
Total earnings by household	7777.19	7451.27	8894.40	−1443.129**	952
	(244.77)	(264.99)	(586.14)	(583.83)	
Savings per day	56.76	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)	
One-way travel cost per episode	264.55	264.12	266.00	−1.88	953
	(3.41)	(3.80)	(7.62)	(8.16)	

[a]Standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The "Diff." columns tests statistical differences between incentivized and non-incentivized groups, with robust standard errors clustered at the village level 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 120,000 for earnings, individuals savings per day less than 500 and individuals earnings 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.

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.

Appendix Table A.VI provides further descriptive statistics on 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

We have documented three facts: (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. (3) A large portion of the households that were incentivized to migrate continued to send a seasonal migrant in subsequent years.

These three facts taken together suggest that the people of Rangpur, and perhaps others in the developing world, are failing to capitalize on an extremely profitable opportunity, and suggest a potential role for policy in facilitating migration. Two issues, however, need to be addressed: first, because these results are unlikely to generalize to all settings, it is important to understand the circumstances in which we expect to see migration outcomes similar to the ones documented above; and second, it is important to understand the optimal policy response to our findings. To address these issues, this section provides a simple model that can help to rationalize the findings. The model can be used to characterize settings in which our experimental results are likely to replicate and can be used to think about optimal policy.

The model we provide emphasizes three key elements: risk, subsistence, and learning about the profitability of migration. These elements help to explain why a household would not migrate despite positive returns, and also the strong re-migration rates. Further, our model also incorporates the empirically realistic assumptions that households face credit constraints and can save, both for migration and to buffer against income shocks.

To assess the empirical fit of the model, we undertake two exercises. First, we use the model to frame a deeper discussion of the data in Section 6 and argue that several patterns in the data are qualitatively consistent with our simple framework. Second, in Section 7, we 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. Here we find that the model is not able to quantitatively replicate our experimental findings for reasonable parameter values. We argue that there are two main reasons for this failure: first, forward-looking migrants should foresee the strong positive re-migration rate and hence the long-term risk reduction advantages of migration; second, given the profitability of migration, households should be saving up in order to experiment.

We interpret the results of this section as follows. First, our qualitative analysis strongly suggests that risk, subsistence, and the need to experience migration are important elements in explaining our experimental results. We predict these three elements will be present in other settings where migration is profitable but underutilized. Second, our quantitative results suggest that the behavior we document (low migration rates but high returns) remains somewhat of a puzzle: there is some element that we do not understand. In Section 8, we provide some discussion of what this element may be, but because we lack the data to come to a definitive conclusion, we leave the resolution of the puzzle to future work. Such work could help us to get a stronger understanding of the two questions that motivate this section: where do we expect to see positive unrealized returns, and what exactly should policymakers do in response to these returns?

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.[19] We refer to this as background income and assume the process is independent and identically distributed (i.i.d.).[20] 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.[21] Therefore, consumption is less than cash on hand ($c \leq x$) in any period.

The household faces uncertainty. With probability π_G the household is type G—good at migrating—and receives a 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. There are two possible interpretations of π_G. The first is that each migrator has a set characteristic, which determines whether he or she is good at migrating, and is revealed at the destination. The second is that π_G is the probability

[19]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. In practice, 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.

[20]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.

[21]Households have access to microfinance from a range of sources, but the conditions associated with such loans (only for female borrowers, to be used for entrepreneurial ventures, requiring bi-weekly repayment) imply that households are still credit constrained for consumption smoothing or migration.

of finding a connection at the destination within a reasonable search time.[22] In either case, we think of type (once it is revealed) 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. 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.[23]

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

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

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

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

With this formulation, we are assuming that the household can migrate before it makes its consumption decision; this means that a household that knows 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

[22]The two alternatives differ in one key aspect: what are the choices open to a household that has migrated in the past and found itself to be a bad migrator? The first alternative implies they will never migrate again, because they know they are bad at migrating, but the second implies that they may migrate again and take another draw to see if they can find a connection. We write the model in this section with the first possibility in mind because it is simpler. However, when we do our calibration we assume that a household that is found to be bad can continue to migrate and have another draw, in line with the second interpretation. This errs on the side of letting the model fit the data, because more households will be affected by the incentive. We also favor the second interpretation when we consider the interpretation of our insurance experiments in Section 6.2.1.

[23]We thank an anonymous referee for clarification on this point and also the term experience good. Direct experience with migration may also be required if it is difficult to receive credible reports on employment conditions at the destination. McKenzie, Gibson, and Stillman (2007) provided some evidence that migrators report incorrect information.

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 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

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

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 is, the less likely is migration for any particular level of cash on hand.[24]

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 and Hopenhayn (2009)). Because

[24]See Eeckhoudt, Gollier, and Schlesinger (1996) and the literature cited there for a discussion of when background risk leads to a reduction in risk taking.

the model is a simple combination of well-known models, we provide only a brief description of its main implications; a longer discussion can be found in Appendix B. First, the model 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. 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 increasing the value of the program conditional on migration and lowers $\tilde{x}$ 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.

Second, the model implies a poverty trap of sorts. We show in the Appendix that, for some parameter values and for low enough cash on hand, households that have the opportunity to migrate engage in exactly the same savings behavior as households that cannot. This implies that these households do not engage in any saving up for migration. If the income process μ is such that cash on hand is always low, then such a household will not save up for the profitable investment, while wealthier households will. This implies that a poverty trap is possible in this model. In Section 7, we ask whether such a trap can be sustained for empirically plausible parameter assumptions. In the Appendix, we also simulate the model for 50 periods and show that, for specific parameter values, it is possible for a household with low starting cash on hand to not migrate, while a household that starts out wealthier saves up and migrates. In general, our simulations show the intuitive comparative static that households with a lower mean income ($E_{\mu}y$) or with a lower starting cash on hand are less likely to cross the migration threshold for any finite time period. Again, this shows that the model can generate a poverty trap over a finite time period.

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. Our aim is to show that risk, subsistence, and learning/experience are important explanations of our experimental findings.

6.1. *Descriptive Evidence on Income Variability and Buffering*

Here we provide evidence that background income is indeed variable, as assumed in the model. We incorporate this variability into the model because it seems to be empirically important, and makes it hard for our model (or any model) to match the empirical results. In particular, our incentive presumably works, at least in part, by increasing cash on hand past the threshold required to migrate. But, the data suggest that income (and, therefore, cash on hand) will be higher by the size of the incentive regularly, just by pure chance. This is primarily why we do not think that a pure liquidity constraint—the inability to raise bus fare—provides a good description of the setting.

We provide two pieces of evidence in favor of income variability. First, our consumption data show a great deal of variability. 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 A.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 earlier rounds' consumption in Appendix Table A.VII. 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 and 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.[25]

Second, we show that behavior is in line with the theoretical implications of background risk. If households are prudent (i.e., $u''' > 0$) and impatient ($\delta > R$), both of which seem likely in our setting,[26] then high income-variability should lead to buffer stock savings. Appendix Table A.VIII describes savings behavior in our sample. 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.

[25]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.

[26]The existence of savings constraints in developing countries (Dupas and Robinson (2013)) makes $\delta > R$ reasonable. There are by now many theoretical and empirical arguments suggesting that prudence is a reasonable assumption for the utility function. See Gollier (2004) for a discussion of the theory, Deaton (1991) for an early argument in favor of the empirical reasonableness of assuming prudence in low income countries, Paxson (1992) for evidence in favor of the buffering behavior implied by prudence, and Carroll (2001) for a discussion of the empirical relevance of prudence and buffering in developed countries.

Our model also suggests, given the assumption of a subsistence constraint, that households close to subsistence should hold very little savings. About 50% of households close to subsistence (consuming less than 1000 calories) do not hold any savings, and average savings in this group is about half of the value for the rest of the sample. Appendix Table A.IX displays results of a Tobit regression of savings on calories consumed and shows a strong positive correlation in both the baseline and the follow-up surveys: every extra 100 calories of consumption per day is associated with 18–34 extra Taka in savings.

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 3 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. There are two ways to think about this. First, if we assume that the incentive was consumed within the one month of December (a reasonable assumption given the date of disbursement and the fact that some households would have had very low consumption without the incentive), this provides an estimate of the outcome of migration in the absence of our treatment. Second, our incentive covered the cost of migration, but in the absence of the incentive, this money would have to be found by a migrating household. Subtracting the cost of migration (roughly 600 Taka as argued below) from monthly consumption gives household consumption levels if migration costs have to be borne within one month.

In panel A of Figure 3, 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).

We quantify this excess risk of falling below subsistence (where the migrant cannot afford 1000 calories) in Appendix Table A.X under different assumptions for the amount of money that has to be paid out-of-pocket (just the bus fare of 450, or the cost of travel including incidentals of 529, or the migration incentive of 600–800), and the length of time over which the incentive was consumed, or the migration cost can be spread. There appears to be significant excess risk of falling below subsistence even if the migration cost can be spread over 2–2.5 months. As we note above, those that are close to subsistence in the data are less likely to have savings, suggesting that they would not be able to

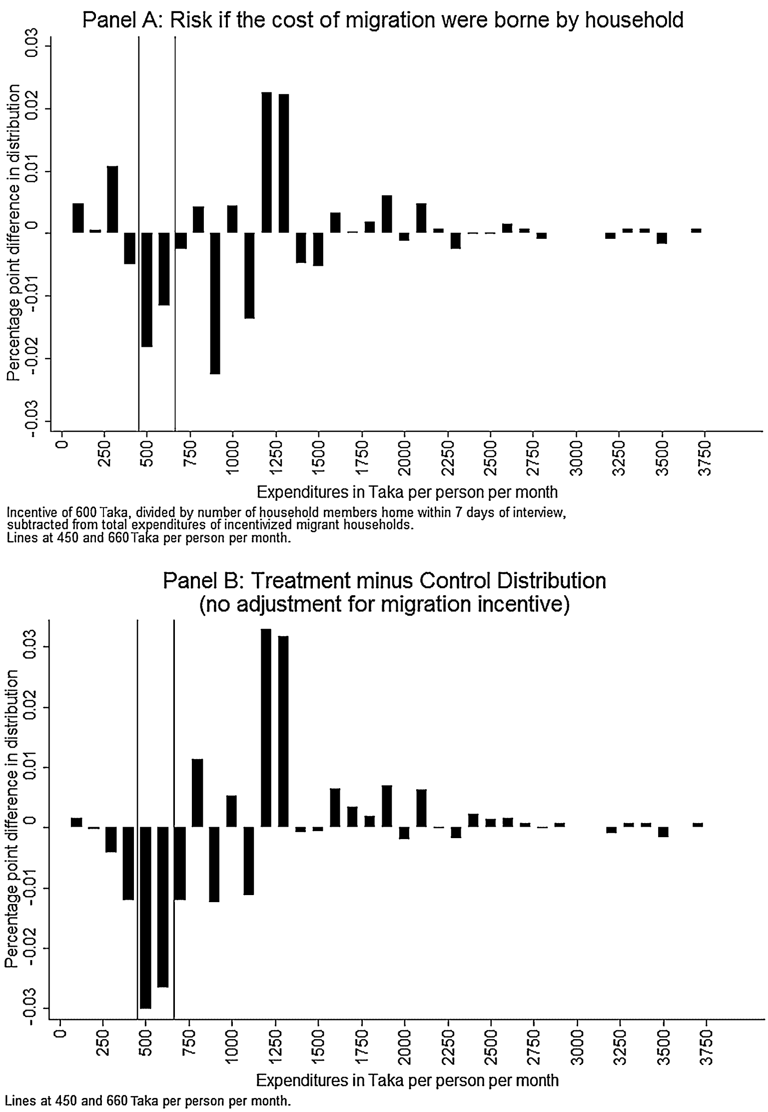


FIGURE 3.—Distribution of consumption in control villages subtracted from distribution of consumption in treatment villages.

spread the cost of migration over time and further suggesting that the choice to migrate is associated with a risk of a very bad outcome. Finally, panel B of Figure 3 shows that the risk all but disappears when we account for the incentive and suggests that households at greatest risk were the ones induced to migrate by our incentive, a result we will explore more precisely below.

6.2.1. *Experiments on Migration Insurance*

Motivated by our first two years of findings and the model, we conducted new experiments in 2011 to directly test whether households perceive migration to be risky. Appendix C describes the sampling frame and intervention design. To study risk, the specific treatment was to offer an 800 Taka loan up-front conditional on migration, but the loan repayment requirement was explicitly conditional on rainfall conditions measured at one of the popular migration destinations: Bogra. Excessive rainfall is an important external event that adversely affects labor demand and non-farm work opportunities at the destination. Rain makes it more difficult to engage in daily wage labor at outdoor construction sites (e.g., breaking bricks); it both increases the cost of pulling rickshaws and lowers the demand for rickshaw transport. In terms of the model, high rainfall reduces 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), and reduces the return to migration, m.

Appendix D 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 measured rainfall is within normal range so that the insurance does not pay out. In terms of the 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.[27] Appendix D 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 risk averse. The insurance contract is based on rainfall measured at one specific destination (Bogra), which allows us to clearly define the basis risk: households that had a pre-existing affinity to Bogra face lower basis risk than others.[28]

There are two sources of exogenous variation in pre-existing household preferences for Bogra: (a) some people were randomly assigned to migrate to Bogra during our August 2008 incentive interventions, and (b) some people had reported sending a migrant to Bogra in our July 2008 baseline survey, conducted before any migration intervention was introduced. Table V shows the migration responsiveness to our 2011 insurance treatment, paying particular attention to heterogeneous treatment effects among those with the affinity to Bogra (who faced lower basis risk).

[27] Related to footnote 21 and the discussion in the text, we are assuming here that π_G is the probability of finding a connection at the destination within a reasonable search time.

[28] We use the basis risk variation to test for riskiness because our insurance contract is valuable even without risk, as it also includes a credit element. During our baseline survey in July 2008 (before any interventions were introduced in these villages), we asked all households the destinations to which they had migrated in the past. This produces a clean indicator for households that entered our sample with a pre-existing affinity for Bogra, and therefore provides exogenous variation in the basis risk created by our insurance contract design.

TABLE V

TREATMENT EFFECTS IN 2011 ACCOUNTING FOR BASIS RISK IN THE INSURANCE PROGRAM[a]

Bogra Variable:	Assigned to Travel to Bogra		Went to Bogra Before 2008
Dep. Var.: Migrated in 2011	Full Sample	Full Sample	Full Sample
Impure control	0.064	0.043	0.045
	(0.048)	(0.046)	(0.045)
Conditional credit	0.156**	0.191**	0.162**
	(0.077)	(0.093)	(0.074)
Rainfall insurance	0.139**	0.084	0.143**
	(0.056)	(0.065)	(0.055)
Unconditional credit	0.099	0.080	0.110*
	(0.065)	(0.075)	(0.065)
Bogra		−0.096	0.142**
		(0.087)	(0.065)
Bogra × Rain insurance		0.216	0.122
		(0.136)	(0.115)
Constant	0.214***	0.200***	0.198***
	(0.064)	(0.071)	(0.062)
Observations	2051	1569	2050
R-squared	0.041	0.051	0.055
District fixed effects?	Yes	Yes	Yes
Mean of assigned to Bogra	0.0835		
p-value for F-test:			
Conditional credit = Rainfall insurance	0.842		
p-value of 0 = Rainfall insurance + Bogra ∗ Rain		0.0552	0.0217

[a]Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Regressions 2 and 3 control for interactions between Bogra and other randomized treatments.

The first column shows that the rainfall insurance contract induced the same amount of migration overall as a simpler (conditional) credit contract very similar to the credit contract offered in 2008. The second column shows that households randomly assigned to travel to Bogra in 2008 are 30 percentage points more likely to migrate under the rainfall insurance treatment in 2011 (p-value 0.05), whereas others are only 8.4 percentage points more likely to migrate. The 21.6 percentage point difference between the two groups has a p-value of 0.11, which indicates that basis risk plays a role in decision-making. The third column replaces the random assignment to Bogra with an indicator for households with the historical propensity to travel to Bogra, and finds that such households are 26.5 percentage points more likely to migrate in response to the rainfall insurance treatment (p-value 0.02), compared to a 14.3 percentage point effect among others. The 12.2 percentage point difference is not statistically significant.

All these results suggest that basis risk affected people's propensity to migrate in response to the insurance intervention. This is reasonably strong evidence that households perceive migration to be risky, and 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 (idiosyncratic), 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 relatively 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 subtreatments 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 from others, 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 VI. If there is learning from others, we expect to see $\hat{\gamma} > 0$, because of the strong positive returns to migration. Table VI shows strong persistence in own migration: that inducing migration in

TABLE VI

LEARNING FROM OWN EXPERIENCE AND OTHERS' EXPERIENCES IN 2009 RE-MIGRATION DECISION[a]

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***	0.486***	0.393***	0.436***	0.392***	0.476***
	(0.02)	(0.145)	(0.02)	(0.136)	(0.021)	(0.132)	(0.02)	(0.13)
Number of friends and relatives who migrated			0.007	−0.001				
			(0.01)	(0.025)				
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**	0.050	0.098***	0.078	0.095**	0.052
	(0.037)	(0.083)	(0.038)	(0.080)	(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	0.198	0.208	0.206	0.209	0.202

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

2008 with the randomized treatments leads those same induced migrants to remigrate 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 A.XI 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 and Sood (2012)). This would make it difficult for migrants to "learn" from other villagers to resolve the uncertainty.[29]

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 16.5% 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.[30] 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.[31] Our model implies that the incentive will only have an impact on those that do not know

[29]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 referrer's own job in danger, or require the referrer to look after a new migrant, perhaps providing some risk sharing and sharing housing.

[30]Appendix Table A.XII 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.

[31]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.

TABLE VII

DIFFERENCES IN CHARACTERISTICS BETWEEN MIGRANTS IN TREATMENT AND IN CONTROL GROUP[a]

	Incentive	Non-Incentive	Diff.
Panel A: Percentage of Migrants That Know Someone at Destination			
First episode	47%	64%	17***
	(1.84)	(3.30)	(3.8)
Any episode	57%	66%	8.3**
	(1.83)	(3.63)	(3.82)
Panel B: Percentage of Migrants That Had a Job Lead at Destination			
First episode	27%	44%	17***
	(1.64)	(3.41)	(3.55)
Any episode	32%	46%	14.5***
	(1.72)	(3.43)	(3.69)
Panel C: Percentage of Migrants Traveling Alone			
First episode	30%	32%	1.6
	(1.70)	(3.20)	(3.6)
Any episode	38%	39%	0.65
	(1.79)	(3.35)	(3.79)

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

their status and so we expect to see more migrators without a connection in the incentive group. Table VII 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. The IV estimates in Appendix Table A.XIIIa (where destination choices are instrumented by the random assignment of destinations) 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. Effects are positive for all four destinations,

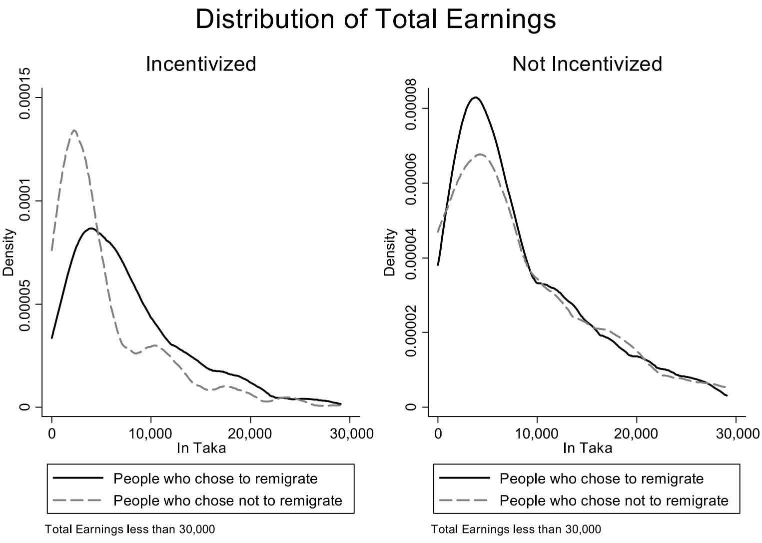


FIGURE 4.—Migration experience in 2008 by re-migration status in 2009.

which is evidence in favor of location-specific learning, or the accumulation of connections at the destination 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 4 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 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 A.1, we see that a substantial portion of households are close to subsistence. Appendix Figure A.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.

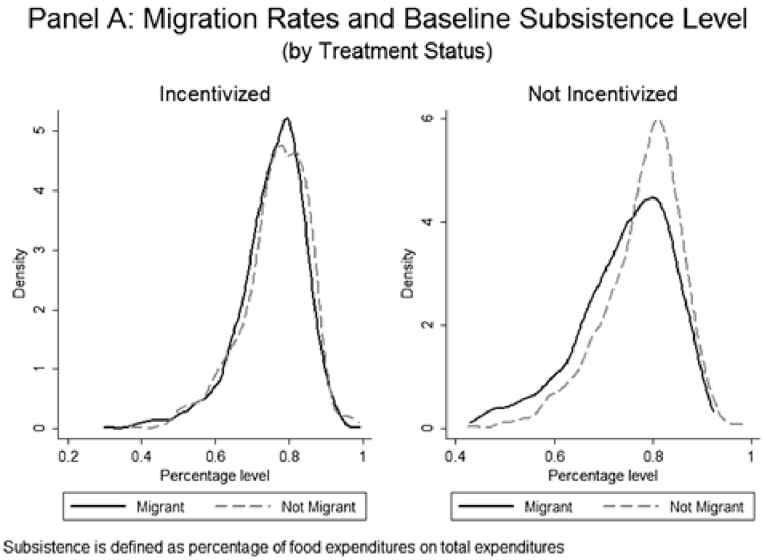
Our model suggests that if aversion to the risk of falling below subsistence is an important deterrent to migration, then: (a) people close to subsistence should not be migrating in the control group, and (b) our treatment should have the largest effect on households that are close to subsistence: they should be the ones induced to migrate by our incentive. The three panels in Figure 5 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.[32] 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 cash and credit incentives. 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.

[32]The ratio of food to total expenditures has less measurement error than caloric intake or total expenditure as a measure of proximity to subsistence because (a) caloric requirements vary greatly across families with different age, gender compositions, and activity levels, and (b) prices used to calculate expenditures vary across families, introducing noise (which is eliminated when computing the ratio). We therefore prefer the ratio measure for our regression, even though we describe subsistence in our sample in Appendix Figure A.2 using more intuitive measures like calories consumed. The concept behind this measure is motivated by the literature on food consumption, which shows that the elasticity of calories to income is modest even among the very poor (e.g., Subramanian and Deaton (1996)). Jensen and Miller (2008) used a similar measure of subsistence.



Panel B: Migration Decision as a Function of Baseline Subsistence	
Incentivized	−0.223
	(0.186)
Ratio of food expenditure over total expenditure round 1	−0.828***
	(0.211)
Interaction: Ratio of food to total ∗ Incentivized	0.566**
	(0.246)
Constant	0.686***
	(0.189)
Observations	1856
R-squared	0.189

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

FIGURE 5.—Heterogeneity in migration responsiveness to treatment by subsistence level.

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, and $M(x) = G(x)$ if the household is a good migrator,

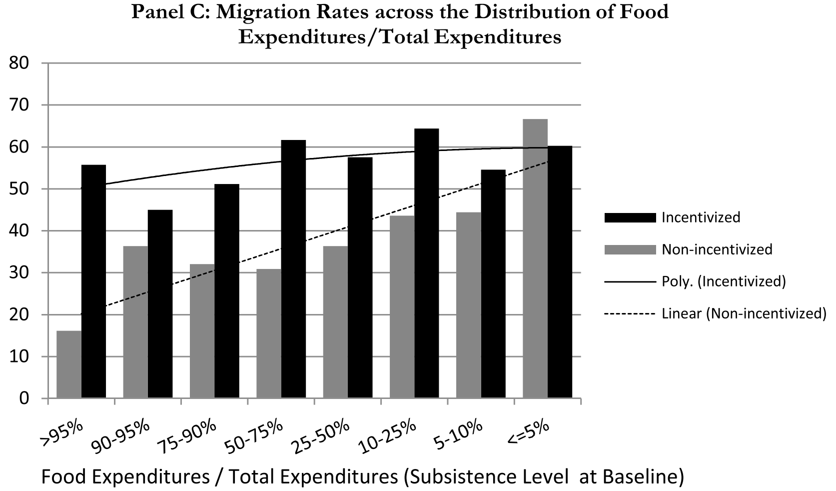


FIGURE 5.—*Continued*.

and consider a household that has a loan of value L and is required to repay Z. The household will repay the loan if and only if

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

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. This setup is easily able to generate the experimental finding that cash and credit have the same impact. The cash treatment differs from credit treatment in making a payout when the migrator is good, but this has a low value, as it occurs when consumption would be high anyway. The credit contract also differs from cash because it costs P to use the limited liability, which provides insurance in the bad state. This cost P is, however, very small relative to the benefit because the household that uses it is close to subsistence. Both these arguments imply that, for concave enough utility functions, the cash and credit have almost identical effects, a fact which we can also demonstrate in our calibrations below.

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 as 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 only increases household's utility when it migrates, while the unconditional credit also increases the payoff to staying at home.[33] 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 the first column of Table V, and indicate that, consistent with our model but inconsistent with a cash constraint model, the unconditional transfer has a smaller impact than the conditional transfer.[34]

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 an experience good. The model also (qualitatively) 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 VIII 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. Our interpretation of this result is that the qualitative evidence above suggests that the model captures several key aspects of the setting, and gives guidance to where we would expect to be able to replicate our experimental findings. However, because the model fails to quantitatively explain the experimental results, there remains some element of household behavior or the environment that our model does not capture, which would result in the observed under-migration. We therefore view the model as providing a starting point for future work on what prevents investments in profitable technologies like seasonal migration.

[33]In terms of the discussion in Appendix B, the conditional credit payment moves only the V^M curve, while the unconditional credit raises the V^N curve as well.

[34]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.

TABLE VIII

PARAMETERS USED FOR CALIBRATION

Parameter	Calibration	Notes
$u(c)$	$\frac{(c-s)^{1-\sigma}}{1-\sigma}$	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 re-migrate
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
$\mu(y)$	$N(700, 70)$ 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

Three calibration 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, reflecting the possibility that the subsistence constraint binds, and the household has no choice but to finance migration out of contemporaneous consumption. Third, we assume that income at home is distributed $N(700, 70)$.[35] This is an attempt to estimate the income distribution of the lowest 50% of households in the sample. We argue that the results of this section are not sensitive to this choice.

[35]The use of the normal distribution implies that the model does not in fact contain a poverty trap: as time goes to infinity, all households will eventually receive a shock so large that they decide to migrate. In practice, this is not a problem for our calibrations for two reasons. First, we truncate the distribution in the upper tail because we have to make a discrete approximation to make the calibration feasible. Second, we work with short time periods—always less than 20 years—implying that this theoretical possibility does not seem important in practice.

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, or the possibility of saving up to migrate (although households do consider the benefit of repeat migration when making their choices).

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 does not yet know its type. In the model, only induceable 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.[36]

Here we present results from calibrating the full model; Appendix E (in Supplemental Material (Bryan, Chowdhury, and Mobarak (2014))) presents further discussion. The left panel of Figure 6 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. The left panel shows that, ignoring the dynamic effects of migration, the model predicts that, with a risk aversion of $\sigma \approx 1.5$, our incentive would induce about 20% of households to migrate—consistent with our experimental findings. Further, consistent with our experimental results, the cash and credit incentives have the same effect. However, while the unconditional cash transfer has a smaller effect, as predicted, at low levels of risk aversion the UCT continues to induce about 17% of the population to migrate, higher than observed in the data. Despite this quantitative inconsistency, this form of the model is broadly consistent with our experimental results.

The right hand panel, however, shows that once we allow for savings up and repeat migration, the model is no longer able to rationalize the data. With a risk aversion level of about 11, 40% of the population is induceable after 8 seasons (or 4 years), which corresponds to a 20% treatment effect if the model

[36]In all the results presented below, we depart slightly from the baseline 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. See also the discussion in footnote 21.

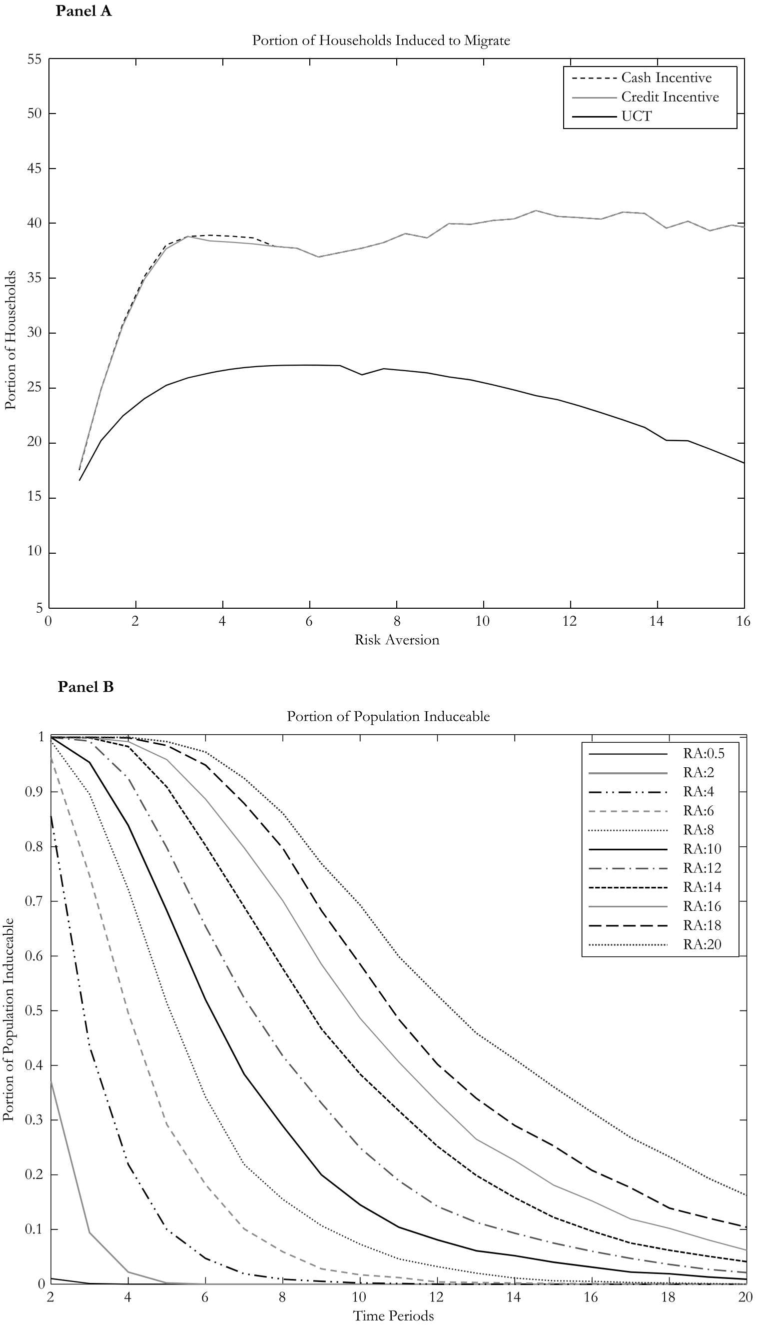


FIGURE 6.—Full model with buffer stock savings and possibility of saving up for migration.

applies to the poorest half of the sample. If we allow 10 prior years of migration activity, the model suggests that even $\sigma \approx 20$ is insufficient to rationalize the results.[37] We discuss plausible values for risk aversion in the next section, but believe that most scholars would think 20 to be implausible, especially in our model, where the subsistence constraint means that behavior will exhibit even more risk aversion. Because it seems reasonable to assume that households in our treatment areas have faced a similar migration choice for some time, and quite reasonably for 10 prior years, we conclude that the model cannot quantitatively account for our experimental findings: there should not be large numbers of people living in the villages who can be induced to migrate.

These results are robust to our assumption regarding the distribution of income. We have simulated the right panel of Figure 6 for standard deviations from 40 to 140, and the results are almost identical. As discussed in Appendix B, there are several impacts of increasing the degree of background risk, and the simulations suggest that, for our parameter values, these effects cancel each other out.

We can also use the 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.

In Appendix E, we discuss two alternative forms of the model: first, 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—that is, they are myopic past the current migration period; and second, a model in which we assume that there is no savings, but allow households to be forward looking. The results allow us to better understand why the model cannot match the data and reveal two main results. First, forward-looking behavior is very important in driving the results: if the households are not forward looking and do not save, then the model is able to match the data, but if the households are forward looking and cannot save, then we cannot match the experimental results without risk aversion levels of greater than 5,

[37]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 10,000 households has not migrated after t periods. Another way to summarize the results is to say that the distribution of cash on hand implied by the model is insufficiently close to subsistence to support the experimental results.

which may be implausibly high. Second, allowing savings has two partially offsetting effects. On one hand, savings reduces the value of migration because it allows the household to self-insure through buffering, reducing the need to experiment with migration. This effect means that, in a myopic model, allowing savings allows us to more easily rationalize the data. On the other hand, savings allows households to save up to migrate. As we see above, once this effect is incorporated, it becomes extremely hard to rationalize the data—households should be saving for an opportunity that is this profitable.

8. EXTENSIONS

While the qualitative evaluation of the model shows 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 to our treatments, we have to extend the model in some way. It could be that households underestimate the benefits of migration, or they are unable to save up for migration, or they are insufficiently forward-looking. In this section, we briefly summarize extensions that we have considered, and provide further details in Appendix F (in Supplemental Material). We do not have the data to determine conclusively which extensions are the most important. We therefore see this section as an extended call for more work, and we provide some suggestions based on our model and data, regarding approaches that are unlikely to work. We consider a large number of possibilities partly to highlight the uncertainty and the need for additional experimentation before moving to policy prescriptions.

The thrust of our calibration argument is that households would have to be very risk averse to generate our data. However, if we allow σ to be very high, then the model can rationalize most of the data.[38] The literature has not arrived at a consensus on "reasonable" values for σ: Holt and Laury (2002) stated that someone with $\sigma > 1.37$ should "stay in bed," while papers in the equity premium literature (e.g., Kandel and Stambaugh (1991)) argued 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) showed 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 should 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.

[38]As we note above, however, high risk aversion is not a complete panacea, as it would imply very high savings rates, which we do not observe in the data.

Changing model parameters presents a second obvious way to improve the fit of the model. We have experimented with several different specifications of the returns to migration, including allowing for risk in m. This does not substantially improve the ability of the model to fit the data. We also considered the effect of lowering the discount factor (δ) or allowing for depreciation of the status of being a good migrator. We considered only reasonable levels of δ and depreciation consistent with the high re-migration rates, and while these possibilities allow for a better fit, high risk aversion levels are still required to match the experimental findings.

A third explanation is that migration is unpleasant and there is high non-pecuniary disutility from migration. We estimate the value of this disutility from our data (see Appendix F for details), but this is unlikely to account for our experimental results. The central issue is that while high disutility may prevent migration, it would also reduce responsiveness to our interventions.

We also examined whether incorrect beliefs about the returns to migration could drive the results.[39] To assess this possibility, 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. If households have biased beliefs, we would expect that those in the control group, who were already migrating and had a chance to learn, would have roughly correct beliefs, while those in the treatment group would have beliefs biased toward the overly pessimistic.[40] Results presented in Appendix Table A.XIV are not consistent with biased beliefs: treatment group migrants do not have significantly different beliefs from control group migrants.

The slightly different character of our results for the model with and without savings points to the possible conclusion that it is savings behavior 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 and Ozier (2012)), or they may simply have 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 and Robinson (2013)). 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, as we argue above, even without savings, forward-looking households should not

[39]One possible cause of bias is that non-migrators have access to incorrect information. McKenzie, Gibson, and Stillman (2007) argued that migrant households provide incorrect information because they do not want to have to share resources, or job connections and accommodation at the destination.

[40]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.

be induceable in great numbers.[41] There is, therefore, 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 and Rosenzweig (2009)).

Finally, 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. 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 be applied to rationalize the data for some values of β. The version of this model discussed in Duflo, Kremer, and Robinson (2011) provided 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. Second, Koszegi and Rabin's (2006) 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, 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, Montenegro, and Pritchett (2008), Rosenzweig (2006), Gibson and McKenzie (2010), Clemens (2011), Rodrik (2007), and Hanson (2009) that offering migration opportunities has large effects on welfare, even rela-

[41]This is shown in Appendix Figure A.7 (see Supplemental Material).

tive 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 understudied phenomenon[42]—are also large.

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 situations 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 and Williamson (1998)) and poorer South Africans (Ardington, Case, and Hosegood (2009)).

Our quantitative work implies that we cannot provide a fully satisfying explanation for why people in Rangpur had not saved up to migrate.[43] 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 People's Republic of Bangladesh (2005), Khandker, Khaleque, and Samad (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 general equilibrium effects in destination labor markets if the government were to contemplate scaling up such a program.[44]

[42]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)).

[43]Several other papers document very high rates of return to small capital investments in developing countries (Udry and Anagol (2006), de Mel, McKenzie, and Woodruff (2008), Bandiera, Barankay, and Rasul (2011), Duflo, Kremer, and Robinson (2011), Fafchamps, McKenzie, Quinn, and Woodruff (2011)), and this literature must also confront the same question of why households do not save to invest in these high-return activities.

[44]There is mixed evidence in the literature on whether these effects are substantial (Ottaviano and Peri (2012), Borjas (2003), Borjas and 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.

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 and 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.[45] 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.

APPENDIX A: 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.

[45]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.

> 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 go to any of the above locations 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 Tk. 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 Tk. 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 Tk. 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 Tk. 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 B: THEORY APPENDIX

In this appendix, we describe the behavior of agents in our baseline model using the value functions, policy functions, and simulated time series of choices. The appendix documents the facts about the model presented in Section 5.1.

Appendix Figure A.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:

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

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).

Appendix Figure A.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. Appendix Figure A.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 simulation shows that the wealthier household 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.[46] 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.

APPENDIX C: 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

[46]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.

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 preharvest 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.[47] 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.

[47]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 D: APPENDIX TABLES AND FIGURES

TABLE A.I

FIRST-STAGE: MIGRATION AS A FUNCTION OF TREATMENTS IN 2008[a]

	Migration in 2008	
Cash	0.169***	0.178***
	(0.045)	(0.044)
Credit	0.164***	0.165***
	(0.044)	(0.044)
Info	−0.012	−0.000
	(0.044)	(0.044)
Sub-district fixed effects?	Yes	Yes
Additional controls?	No	Yes
Observations	1868	1824
R-squared	0.101	0.145
1st F-test	12.74	12.58
1st p-value	0.000	0.000
1st partial R2	0.027	0.028

[a]Robust standard errors in parentheses, clustered by village. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Table displays first-stage results for the regressions displayed in columns 6–7, row 1, of Table III. First stage for other dependent variables varies slightly depending on the sample used (varies only if observations are missing). The dependent variable is a binary variable equal to 1 if at least one member of household migrated. Additional controls included in column 2 are: 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 A.II

INTENSIVE AND EXTENSIVE MARGIN CHANGES DUE TO INCENTIVE (CASH OR CREDIT)[a]

	2008	2009	2011
Total number of migration episodes per household	0.385***	0.186***	0.019
	(0.070)	(0.071)	(0.026)
Total number of migrants per household	0.190***	0.074**	0.071*
	(0.034)	(0.035)	(0.036)
Changes on Intensive Margin			
Total number of migration episodes per household (among migrant households)	0.111	0.110	−0.001
	(0.104)	(0.069)	(0.053)
Total number of migrants per household (among migrant households)	−0.017	−0.009	0.015
	(0.023)	(0.018)	(0.015)
Total number of episodes per migrant	0.127	0.110	−0.021
	(0.097)	(0.067)	(0.041)
Days away per migrant per episode	−11.722**	−2.705	3.336**
	(5.283)	(3.987)	(1.432)
Male	0.016	−0.004	−0.010**
	(0.015)	(0.007)	(0.004)
Age	2.625**	0.128	−0.153
	(1.106)	(1.012)	(0.832)
Migrant is head of household	0.070**	−0.027	−0.004
	(0.032)	(0.028)	(0.019)

[a]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).

TABLE A.III

EFFECTS OF MIGRATION IN 2008 ON CONSUMPTION IN 2008; SENSITIVITY TO CHANGES IN DEFINITION OF HOUSEHOLD SIZE[a]

	ITT					
Dependent Variable:	Cash	Credit	Info	ITT	IV	OLS
Panel A: Household Size Is Based on Question Q7 in R2 Follow-Up Survey ("Status of Household Members")						
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	120.927**	111.339**	−66.444	148.964***	869.842***	102.951***
(per person per day)	(54.673)	(51.398)	(68.194)	(42.735)	(243.784)	(38.129)
Panel B: Household Size Is Based on Question Q9 in R2 Follow-Up Survey ("Currently Present Members")						
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	69.645	77.571	−117.409	130.875***	775.485***	218.266***
(per person per day)	(65.251)	(62.278)	(76.655)	(48.946)	(274.635)	(41.640)

(*Continues*)

TABLE A.III—*Continued*

	ITT					
Dependent Variable:	Cash	Credit	Info	ITT	IV	OLS
Panel C: Household Size Is Based on the Total Number of Household Members at the Time of the Interview						
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	90.556	91.954	−69.585	125.294***	737.107***	252.609***
(per person per day)	(60.478)	(56.772)	(75.689)	(46.656)	(249.228)	(40.847)
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	115.229*	97.084	−83.808	147.739***	872.820***	350.271***
(per person per day)	(65.440)	(63.041)	(77.209)	(48.055)	(243.244)	(41.971)

(*Continues*)

TABLE A.III—*Continued*

	ITT					
Dependent Variable:	Cash	Credit	Info	ITT	IV	OLS
	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	−9.354	−21.278	−426.987	193.855	1169.733	22.695
	(279.707)	(274.067)	(342.132)	(225.931)	(1245.768)	(166.249)

[a]This table explores whether measurement error in migration dates and in household size net of migrants biases our estimates of the effects of migration on per capita consumption. We conduct a number of sensitivity checks below by varying the definition of household size (the denominator in the dependent variables measuring consumption). 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, based on four household members) 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. Robust standard errors in parentheses, clustered by village. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A.IV

EFFECTS OF MIGRATION BEFORE DECEMBER 2008 ON CONSUMPTION AMONGST REMAINING HOUSEHOLD MEMBERS[a]

	ITT								
	Cash	Credit	Info	ITT	ITT	IV	IV	OLS	Mean
	Panel A: 2008 Consumption								
Calories from protein (per person per day)	2.852*	2.588*	−0.509	2.977**	2.657**	17.442**	15.573**	6.777***	46.51
	(1.557)	(1.571)	(2.089)	(1.287)	(1.273)	(7.064)	(6.830)	(0.992)	
Consumption of meat	12.325**	6.577	8.163	5.618	5.599	31.857	34.302	3.905	28.26
	(5.489)	(5.402)	(6.667)	(3.755)	(3.726)	(21.549)	(21.399)	(3.923)	
Consumption of milk and egg	−0.468	−1.365	0.026	−0.904	−1.318	−5.127	−7.237	1.764	13.06
	(2.256)	(2.334)	(2.401)	(1.563)	(1.544)	(9.107)	(9.052)	(1.679)	
Consumption of fish	8.979*	12.618**	8.977	6.297	5.193	34.652	28.775	8.901**	71.48
	(4.743)	(5.998)	(6.076)	(4.407)	(4.142)	(24.941)	(22.909)	(3.778)	
Consumption of childrens' education	6.146*	7.658**	1.546	6.110**	4.299*	30.848**	21.487	−3.677	18.17
	(3.297)	(3.441)	(3.938)	(2.485)	(2.405)	(14.144)	(13.536)	(2.355)	
Consumption of clothing and shoes	0.806	3.199	0.163	1.854	1.581	10.425	8.532	9.987***	38.83
	(2.075)	(1.986)	(2.493)	(1.547)	(1.496)	(8.907)	(8.439)	(1.675)	
Female in HH worked for wages in last 4 months	−0.006	−0.053	−0.055	−0.001	−0.003	0.003	−0.010	−0.016	0.21
	(0.038)	(0.034)	(0.036)	(0.026)	(0.024)	(0.142)	(0.136)	(0.022)	
Children aged 5–18 attended school	0.005	0.007	0.002	0.005	−0.005	0.019	−0.024	−0.001	0.18
	(0.026)	(0.028)	(0.037)	(0.019)	(0.018)	(0.082)	(0.071)	(0.025)	
Agricultural investment	−10.044	−1.163	−8.853	−1.530	1.973	−8.573	9.039	−4.833	40.75
	(27.110)	(24.590)	(30.420)	(16.498)	(15.701)	(93.891)	(90.518)	(20.330)	

(*Continues*)

TABLE A.IV—*Continued*

	ITT								
	Cash	Credit	Info	ITT	ITT	IV	IV	OLS	Mean
	Panel B: 2009 Consumption								
Calories from protein	1.507	0.400	−2.060	2.004**	1.569*	10.627**	8.701*	0.391	44.81
(per person per day)	(1.150)	(1.273)	(1.349)	(0.830)	(0.864)	(4.688)	(4.567)	(0.694)	
Consumption of meat	−2.330	5.010	−2.065	2.141	1.897	10.395	8.961	−2.300	26.72
	(4.558)	(4.587)	(5.343)	(2.953)	(2.926)	(15.922)	(15.436)	(2.919)	
Consumption of milk and egg	−0.565	1.074	−5.386*	2.866	2.507	14.295	12.960	0.506	20.27
	(3.096)	(3.194)	(3.126)	(2.029)	(2.090)	(11.418)	(11.417)	(1.451)	
Consumption of fish	4.802	−4.198	−2.118	1.616	0.982	8.639	7.136	4.231	64.73
	(5.191)	(5.221)	(6.337)	(3.562)	(3.719)	(19.128)	(19.562)	(2.616)	
Consumption of childrens' education	−0.169	−0.604	−3.753	1.498	0.106	7.423	0.898	−5.666***	18.15
	(2.743)	(2.775)	(2.979)	(1.766)	(1.611)	(8.782)	(7.802)	(1.683)	
Consumption of clothing and shoes	0.945	0.698	0.140	0.760	0.418	3.754	2.462	2.665***	37.08
	(1.283)	(1.316)	(1.315)	(0.780)	(0.815)	(4.396)	(4.365)	(0.851)	
Controls?	No	No	No	No	Yes	No	Yes	No	

[a]Robust standard errors in parentheses, clustered by village. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Each row is a different dependent variable (listed 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 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. Female wage labor and children 5–18 attending school are proportions by household based on members home or accounted for at the time of the interview. 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 A.V

EFFECTS OF MIGRATION IN 2008 ON SAVINGS, EARNINGS AND CHANGES IN CHILDREN'S MIDDLE UPPER ARM CIRCUMFERENCE (MUAC)[a]

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) treatment	591.617***		585.653		1.929		0.744	
	(170.718)		(708.002)		(1.315)		(0.951)	
Migration (before Dec. 2008), instrumented by treatment	3287.602***		3281.877		11.059		4.474	
	(869.377)		(3773.748)		(7.944)		(5.348)	
Controls?	No	No	No	No	No	No	No	No
Observations	1851	1851	1851	1851	1854	1854	1836	1836
R-squared	0.052	0.285	0.026	0.103	0.031	−0.034	0.017	−0.005
Mean of control	1999	1999	13,842	13,842	204.6	204.6	−4.601	−4.601

[a] 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.

TABLE A.VI

2008 MIGRANT CHARACTERISTICS BY DESTINATION AND BY SECTOR[a]

Sector	Dhaka	Munshigonj	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-agriculture 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	1694
Total earnings at destination	5005.06	3777.30	2897.88	2491.07	5160.60	
	(185.92)	(156.0)	(145.72)	(123.19)	(188.69)	

[a]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 A.VII

COVARIANCE OF INCOME PER CAPITA ACROSS ROUNDS[a]

	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***	1094.635***
	(18.215)	(25.513)	(15.676)
Sub-district FE?	No	No	No
Observations	1855	1782	1798
R-squared	0.027	0.131	0.017

[a]Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A.VIII

SUMMARY STATISTICS ON HOUSEHOLDS SAVINGS[a]

	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	1629.28	787.04	1616.97	768.33	2280.19	792.19	1885.67
Total value of current cash savings for HHs with reported savings	1416.36	2023.58	1385.29	1942.77	2233.72	3442.41	1623.78	2436.98
Share with liquid assets	0.42	0.49	0.59	0.49	0.81	0.39	0.60	0.49
Total value of liquid assets for all HHs	339.35	1154.88	494.58	1292.40	1390.12	3115.53	764.94	2206.22
Total value of liquid assets for HHs with reported assets	812.05	1676.18	844.30	1599.04	1709.12	3374.84	1280.17	2736.26
1 if purchased assets in last 12 months (all HHs)	0.01	0.09	0.01	0.09	0.20	0.40	0.07	0.26
Value of purchased assets in the last 12 months	6.26	89.65	9.37	195.36	122.89	1476.58	41.85	554.70
Total savings (current + liquid assets) for all HHs	1084.80	2057.72	1281.62	2185.67	2157.30	4028.99	1552.70	3032.10
Total savings (current + liquid assets) for HHs with reported savings or assets	1547.39	2307.55	1588.02	2330.90	2530.66	4254.20	1981.93	3299.21
Observations	1900		1871		2413		5688	

[a]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.

TABLE A.IX

TOTAL SAVINGS AS A FUNCTION OF CALORIES[a]

	Baseline			Follow-Up 2008		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Calorie consumption per day per capita	0.343** (0.152)	0.375** (0.154)	0.285* (0.165)	0.181** (0.0737)	0.185*** (0.0700)	−0.0171 (0.0958)
Total expenditures per capita			0.190 (0.134)			0.433*** (0.168)
Constant	−933.7*** (333.4)	−793.1* (409.4)	−820.5** (409.2)	−422.2* (218.9)	−193.9 (307.6)	−233.9 (314.7)
Sigma	2518*** (201.0)	2491*** (197.0)	2489*** (196.6)	2401*** (247.0)	2385*** (246.5)	2382*** (246.7)
Observations	1893	1893	1892	1854	1854	1854
District fixed effects?	No	Yes	Yes	No	Yes	Yes

[a]Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Tobit analysis, left censored at 0.

TABLE A.X

EXCESS RISK OF STARVATION FROM PAYING MIGRATION COST[a]

Risk Level:	Starvation Level (<450 Taka per Person per Month)			
Amount Subtracted From Total Expenditures:	450	529	600	800
Migration cost spread over...				
1 month	36%	52%	59%	88%
1.5 months	16%	19%	27%	52%
2 months	1%	12%	16%	27%
2.5 months	−2%	−2%	3%	16%
3 months	−4%	−4%	−2%	12%
3.5 months	−13%	−4%	−4%	1%
4 months	−13%	−13%	−4%	−2%
% treat group when month = 1	4.70%	5.24%	5.47%	6.48%
% control group	3.45%	3.45%	3.45%	3.45%

[a]Amount subtracted from total expenditures per month per person: 450 is the average cost of a roundtrip bus ticket. 529 Taka is the average cost of migration reported by migrants including bus fare and incidentals. 600 is the base amount of the incentive given. 800 is the base amount of the incentive plus the 200 bonus upon arrival at the new city, only for those who reported to us at the destination.

Risk level: 450 Taka per person per month is the minimum needed to consume 1000 calories per day—the minimum for survival.

TABLE A.XI

GOING BACK TO THE SAME EMPLOYER IN 2011[a]

	Full Sample
Incentivized in 2008	0.047*
	(0.027)
Constant	0.266***
	(0.020)
Observations	2771
R-squared	0.003

[a]Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The dependent variable is equal to 1 if a respondent reports going to the same employer in 2011 as before; 0 otherwise.

TABLE A.XIIa

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][a]

	2007	2008	Diff.	p-Value	Observations
First met employer in 2007 vs. 2008	0.42	0.58	−0.17	0.0941	103
	(0.05)	(0.05)			
	2006–2007	2008–2009	Diff.	p-Value	Observations
First met employer in 2006–2007 vs. 2008–2009	0.43	0.57	−0.13	0.0567	201
	(0.04)	(0.04)			

[a]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.

TABLE A.XIIb

PROPORTION OF 2011 MIGRANTS WHO FIRST MET EMPLOYER BEFORE OR AFTER INCENTIVIZATION (2006–2007 VS. 2008–2009), FULL SAMPLE [MIGRANT ONLY SAMPLE; NON-EXPERIMENTAL][a]

	Not Incentivized	Incentivized	Diff.	p-Value	Observations
First met employer in 2008 (rather than 2007)	0.50	0.58	−0.08	0.2589	189
	(0.05)	(0.05)			
	Not Incentivized	Incentivized	Diff.	p-Value	Observations
First met employer in 2006–2007 rather than 2008–2009	0.54	0.57	−0.03	0.5672	363
	(0.04)	(0.04)			

[a]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.

TABLE A.XIIIa

DESTINATION CHOICES OF RE-MIGRANTS[a]

Panel A. Dep. Var.: Migrated in 2009 to:	Dhaka		Bogra		Tangail		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 p-value		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

(*Continues*)

TABLE A.XIIIa—*Continued*

Panel B. Dep. Va.: Migrated in 2011 to:	Dhaka		Bogra		Tangail		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 p-value		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

[a]Each coefficient entry in the table comes from a separate regression where migration to a specific destination in 2009 (Panel A) or in 2011 (Panel B) 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 (Panel A), or in 2011 (Panel B). 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 specific destinations, along with the other randomized treatments. Appendix Table A.XIIIb shows that initial destination assignment had a strong effect on destination choices in 2008. Sub-district fixed effect are included but not reported. The sample includes only households that sent a migrant in both 2008 and 2009 (or 2011). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Robust standard errors clustered by village in parentheses.

The hypothesis of destination specific learning implies that there should be more than one significant coefficient in the second stage estimates. 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 show that migration assignment leads to destination-specific re-migration to at least two different cities. 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 favor of location specific learning, or the accumulation of connections at the destination being an important driver of migration behavior.

TABLE A.XIIIb

FIRST STAGE OF INSTRUMENTAL VARIABLES REGRESSION FOR DESTINATION CHOICES FOR 2009 MIGRANTS[a]

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
F-statistic	1.139	4.338	2.116	0.980
prob > F	0.345	0.000166	0.0412	0.456
Partial $R2$	0.0119	0.0561	0.0616	0.0217
Hansen J statistic	4.272	7.142	8.882	3.920
R-squared	0.092	0.103	0.197	0.097

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

TABLE A.XIV

EXPECTATIONS ABOUT FINDING A JOB AND EARNINGS (NON-EXPERIMENTAL: ASKED OF 2008 MIGRANTS)[a]

	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)

[a]Standard errors in parentheses.

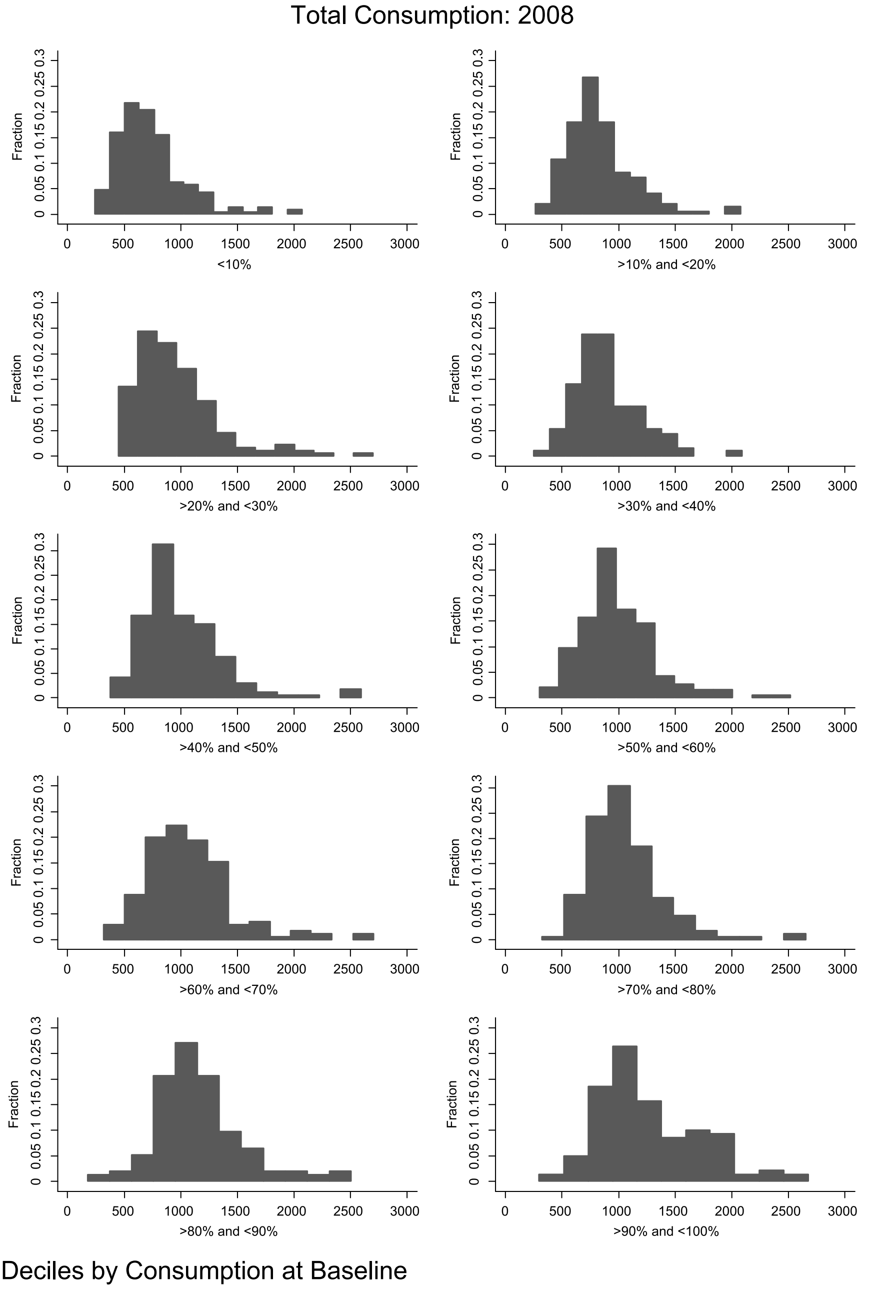


FIGURE A.1.—Distribution of consumption per person per month by baseline consumption decile.

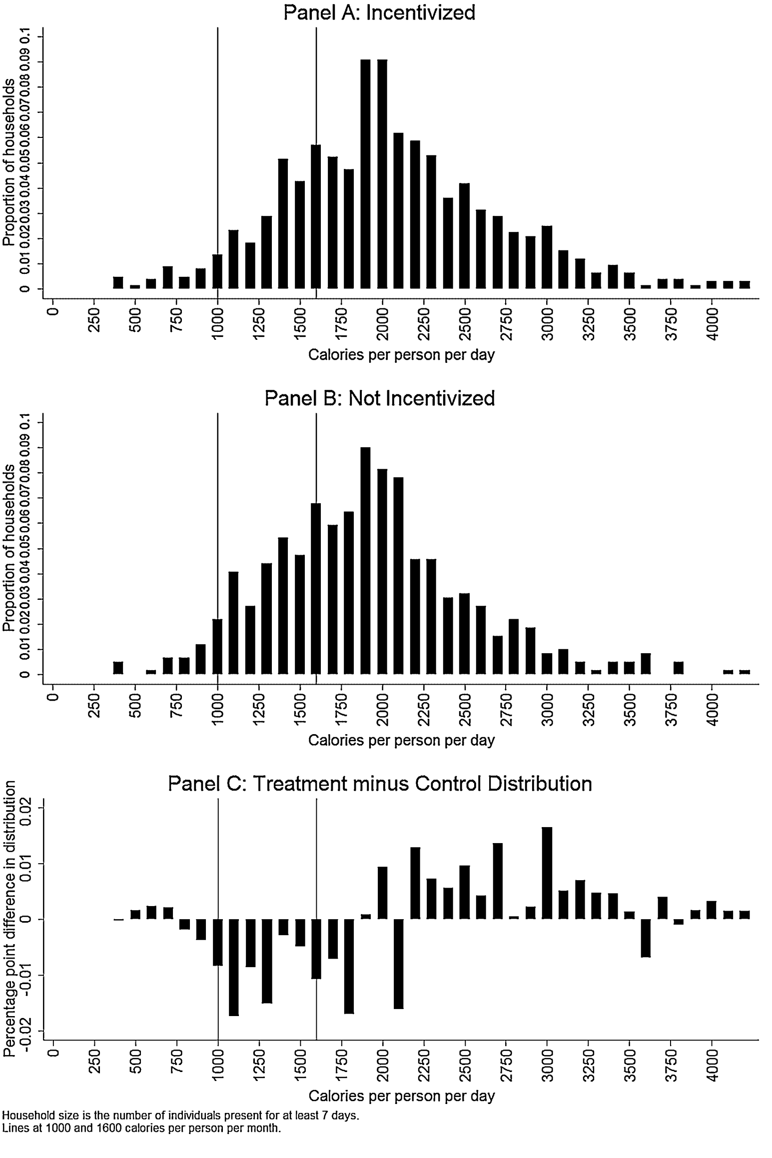


FIGURE A.2.—Distribution of calories per person per day in 2008.

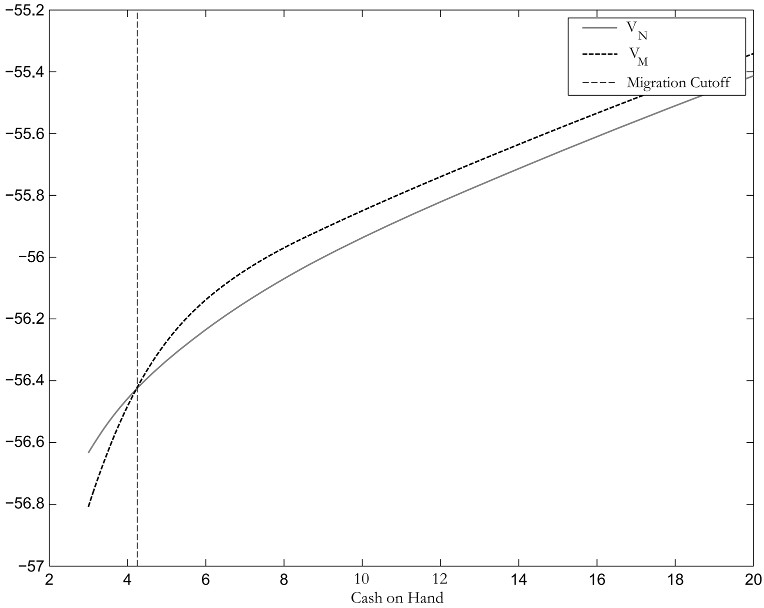


FIGURE A.3.—Value functions of migrating and non-migrating households.

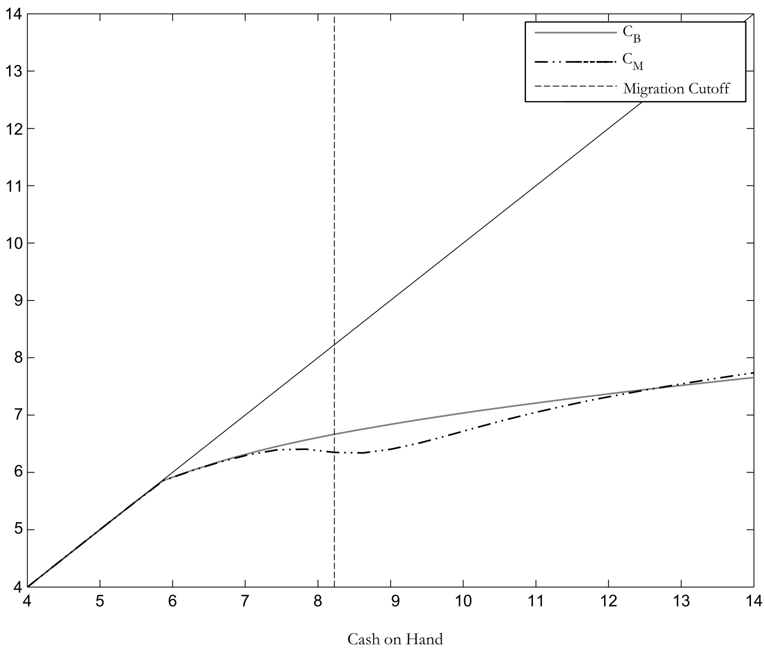


FIGURE A.4.—Policy functions (consumption as a function of cash on hand) for households bad at migrating and households restricted from migrating.

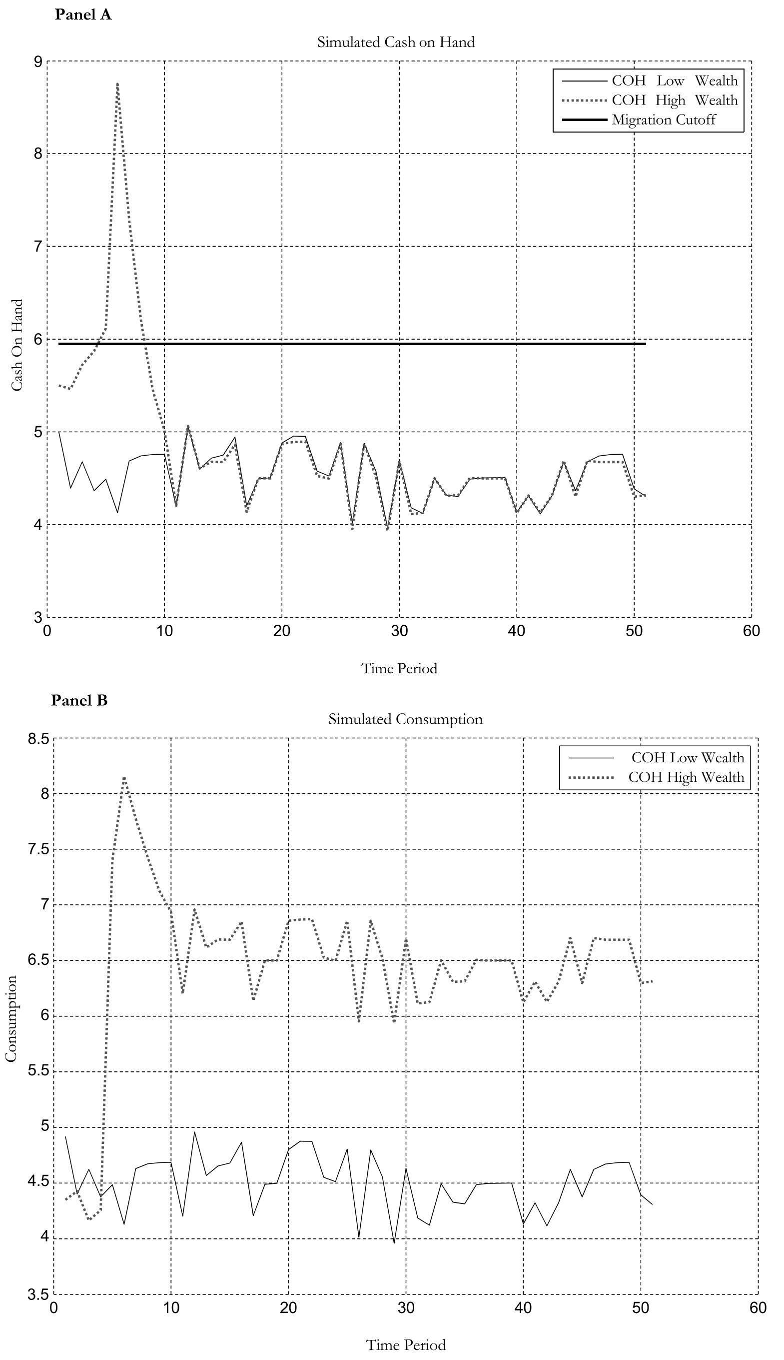


FIGURE A.5.—Simulated cash on hand and consumptions for varying levels of wealth.

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