Migration and Consumption Insurance in Bangladesh *

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Abstract

We investigate the relationship between seasonal migration and informal risk sharing in rural Bangladesh. We use data from a randomized controlled trial which provided incentives for households to migrate (Bryan et al., 2014). Using this experimental variation, we first provide evidence that these incentives substantially increased insurance against income shocks. We then investigate the mechanisms behind this effect by characterizing a dynamic model of migration and endogenous risk sharing. We estimate the model using data from control villages, and use treatment data to validate the model. We find that extending migration opportunities reduced the persistence of income shocks, which in turn relaxed constraints to risk-sharing and allowed for more insurance between households.

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1 Introduction

70% of the world's poor live in rural, agrarian areas. Agricultural activities are weather-dependent and risky, are rural livelihoods are both volatile across years and fluctuate seasonally with the crop cycle. Income diversification and consumption smoothing opportunities are valuable in such environments, but formal insurance markets are often incomplete or absent.

An established literature documents that in such environments, households manage risk through three primary mechanisms: They share risk with other members of their community (Munshi and Rosenzweig, 2016; Ferrara, 2003), they migrate to diversify income sources (Banerjee et al. (2007)), or they self-insure through savings. Each of these mechanisms is typically incomplete or imperfect: There are savings constraints (Dupas and Robinson, 2013), informal risk sharing is often incomplete and only provides partial insurance (e.g. Townsend (1994)), and migration may be costly or risky (Bryan et al., 2014). This paper studies the interaction between the two most common risk management techniques used by the rural poor: informal risk sharing with neighbors, and internal seasonal migration. We structurally estimate a model of informal insurance using a randomized experiment that lowered the cost of migration, to explore whether enhanced migration opportunities changes the nature and extent of risk sharing between community members.

Migration opportunities may change the viability of informal insurance schemes due to the "limited commitment" nature of risk sharing (Thomas and Worrall, 1988; Ligon et al., 2002; Kinnan, 2014). To insure idiosyncratic risk, the network must help a member facing an adverse shock in return for the promise of reciprocal aid in the future. The member receiving support cannot easily commit to helping back in the future, especially if a new migration opportunity helps them better diversify their own income sources and self-insure. In other words, lowering the cost of migration may tempt them to go at it alone, even if it means being cut off from the network forever. Thomas and Worrall (1988), and Ligon et al. (2002) have shown that networks may provide partial insurance in such circumstances, such that households with particularly good fortune are asked to transfer less, keeping them engaged in the network. The resulting partial insurance agreement will depend on the value of the outside option for the household. A new migration opportunity can change a household's welfare in autarky, thereby changing the extent of partial insurance that remains feasible (Morten (2017)). Furthermore, migration can exacerbate the asymmetric information problem in the network, making it more difficult for network members to monitor each other, creating moral hazard and undermining the strength of risk sharing ties (Kinnan, 2014). On the other hand, the spatial diversification associated with migration to the city allows the migrant to smooth aggregate shocks, and his network partners may receive some spillover benefits in the form of partial insurance against aggregate risk.

This paper models these interactions between migration and the sustainability of the informal insurance network, and estimates the impact of introducing the option to migrate on insurance and welfare. It has been difficult to answer this question because the decision to migrate is generally an endogenous choice. We circumvent this problem by estimating the model using a large-scale randomized experiment that successfully induced households in rural Bangladesh to seasonally migrate to cities during the pre-harvest lean season in their agrarian village. We have long-run panel data on income and consumption from treatment and control households both before and after the experiment, which allow us to infer the extent of risk sharing across households and estimate key parameters of the model.

This experiment was conducted in Northern Bangladesh, where year-to-year fluctuations in income are large, and seasonality is pronounced (Khandker (2012)). A program evaluation of the experiment (Bryan et al., 2014) suggests that the household's own returns to migrating to the city (during a lean, "hungry" period of the year when agricultural work in the village is scarce) are quite large, and that a small cash or credit inducement covering the round-trip cost of travel removes a constraint that was preventing profitable out-migration. The $8.50 transfer increased seasonal migration rates out of the village by 22 percentage points and increased consumption of the migrants' family members left behind in the village by 30%. One year after the subsidies were removed, migration remained 11 percentage points higher in the treatment villages. That program evaluation only focused on direct benefits accruing to migrant households, while this paper explores the indirect effects of migration opportunities that accrue to the network, through changes in the nature of risk sharing across households.

We first directly exploit the experiment to provide reduced form evidence on how migration opportunities affect village risk-sharing. We follow Townsend (1994) and test the extent to which village level shocks affect consumption. The results suggest that rural Bangladeshis are partially insured by their village co-residents, and that reducing migration costs improved the amount of risk sharing substantially. In other words, the dominant factor was the availability of this alternative income opportunity that changed the extent of diversification in the network, rather than the negative effects of improved outside options on risk sharing or an increase in moral hazard.

The second part of this paper structurally estimates a model of partial risk sharing to distinguish between different mechanisms underlying the effects we observe, and then simulate counterfactuals. Our model builds on the framework in Morten (2017) but extends the estimation by using exogenous variation in migration to trace out the casual effect on risk sharing. We follow the approach of Krueger and Perri (2010), where a planner minimizes the cost of offering promised level of utilities to all members in a risk sharing network, subject to participation constraints. Our model introduces an additional option to migrate after village income is revealed. A (temporary) migrant travels to town in search for work that provides uncertain returns. We estimate this model using the method of moments. We then use the model to simulate the effects of alternative policies on the amount of risk sharing. To estimate the structural model we use data from control villages only. With these structural parameter estimates, we then use the model to predict how the experiment would affect risk sharing and the variance of consumption in the treatment data.

Finally, we use data from new experiments run in these villages in 2014 (Akram et al., 2016) to validate the model's predictions out-of-sample. The new experiments varied the proportion of the village network that received migration subsidy offers simultaneously. Our model is able to replicate the experiment's central findings that providing migration subsidies to a larger proportion of the network improves risk sharing to a greater extent, and increases each household's out-migration propensity.

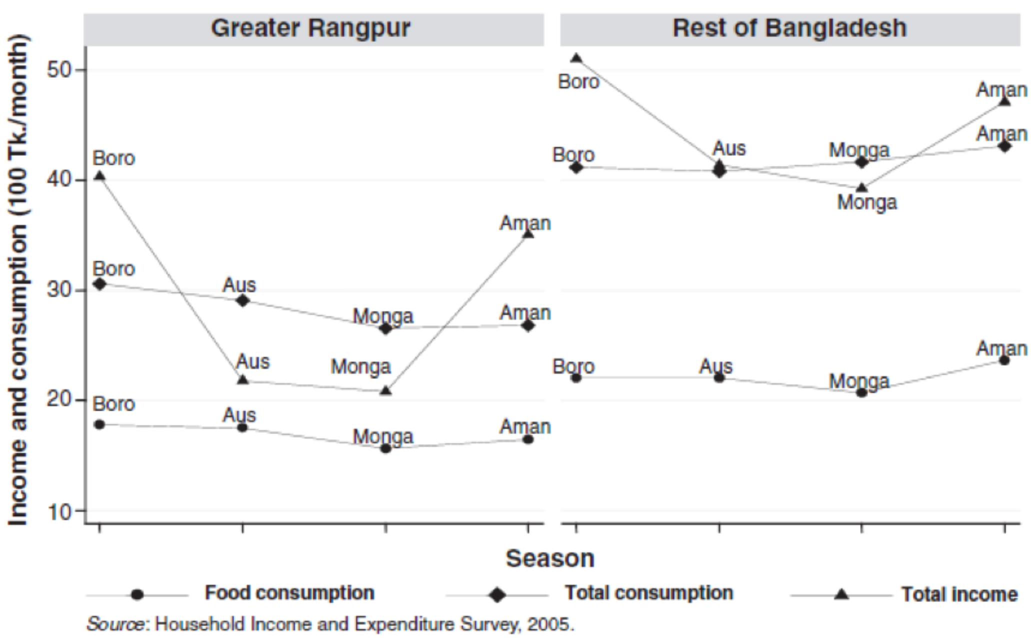
Our paper builds on an important literature on risk sharing. The earlier literature focused on tests for perfect risk sharing, such as Townsend (1994) and Udry (1994). The overwhelming conclusion has been that risk sharing is at best imperfect. Similar tests using US data (Cochrane, Attanasio and Davis amongst others) find similar results. Perfect insurance within the extended family has been rejected as well (Altonji et al. (1992)). The failure of full insurance has been attributed either to limited commitment or to moral hazard (see for example, Attanasio and Pavoni (2011)), which has given rise to a literature on partial insurance. Empirical tests for partial insurance in the context of industrialized countries have been proposed by Blundell et al. (2008) and Blundell et al. (2012), who conclude that while households in the US appear partially insured, this can mostly be attributed to self-insurance from savings. Attanasio et al. (n.d.) further show that the extended family provides no insurance beyond what BPP identify as originating from self-insurance. However, in the US, with its relatively advanced welfare system and ample labor market opportunities, outside options may be too good to support any substantial amount of insurance when there is limited commitment. This contrasts with the Bangladeshi context where autarky is unlikely to be a very attractive option. The key papers developing limited commitment models are Thomas and Worrall (1988), Ligon et al. (2002). Finally, Morten (2017) introduces migration in a model of limited commitment, which together with Krueger and Perri (2009) forms the basis of our approach.

We proceed as follows. In Section 2 we review the empirical setting. In Section 3 we undertake a reduced form analysis, building on the tests developed in Townsend (1994). Then, we characterize a model of migration and endogenous risk-sharing in Section 4. Section 5 describes our estimation strategy and discusses the results, and Section 7 briefly concludes.

2 Data

The empirical setting of the data is Rangpur, in northwest Bangladesh. The population of this area is 9.6 million, of which 5.3 million are below the poverty line. This is an area prone to a seasonal famine, known as *monga*. During the *monga* period, which occurs during September-November, prior to the harvest of the Aman rice harvest, consumption levels drop dramatically. This is shown in Figure 1.

Figure 1: Consumption, by season: Rangur and rest of Bangladesh



Source: Figure 5 from Khandker (2012)

The experiment, fully described in Bryan et al. (2014), was carried out in 2008, with multiple follow-up surveys and subsequent experiments. The 2008 experiment was conducted over two districts and covered 100 villages. In each village, 19 households were randomly selected from the set of households that reported (a) having low levels of landholding and (b) that a household member had to skip at least one meal during the prior monga season (56% of households satisfied both criteria).

The experiment itself was multi-pronged. There were three alternative treatments: a cash incentive (conditional on migrating), a credit incentive (conditional on migrating) and a provision of information treatment. Households in 37 villages were randomly assigned to the cash treatment; 31 were assigned to the credit treatment; 16 were assigned to the information treatment and 16 were control. The cash and credit treatment were each 600 taka ($8.50), approximately the cost of a return bus ticket and a few days food in the destination. The baseline was collected in July 2008, prior to that year's *monga*. In July 2011 the sample was expanded to a further 33 villages. In total, five rounds of data were collected and in most rounds this included detailed data on income, consumption, and migration episodes. Table 1 gives a summary timetable of the data collection including the dates for each round.

Table 1: Experimental Design and Data Collection Timeline

Round	Date	Observations	Treat/control
1	July 2008 (planting)	1900 HHs, 100 villages	
2[a]	Nov 2008 (Monga)	1900 HHs, 100 villages	Cash, credit, info, control
3[b]	Nov 2009	1900 HHs, 100 villages	
4	July 2011	2527 HHs, 133 villages	Rain insurance, price insurance, credit, conditional credit, control
5	Dec 2013	2527 HHs, 133 villages	Credit, job leads, control

[a] Income in rounds 1, 4, and 5 spans the previous 12 months. Income in round 2 spans the previous 4 months. [b] Income data was not collected in round 3.

This experiment had three main effects.[1] First, migration rates increased by 22 percentage points in treatment villages in the year in which financial incentives were offered. Second, treated villages were 8-10 percentage points more likely to migrate 1 and 3 years

[1] In what follows, the cash and credit treatments are bundled as "treatment". The information treatment was not successful at inducing migration and therefore we group these villages with control villages.

after the migration incentives were removed. Third, migration had positive returns on average: the consumption of family members left behind increased by approximately 30%. We refer the reader to Bryan et al. (2014) for a full description of the experimental effects. Some experimental sub-treatments also varied additional conditionalities attached to the migration subsidy, such as a requirement to form a group and migrate together. Our empirical section will explore whether these additional treatments and constraints changed the effect of migration subsidies on risk sharing.

Table 2: Summary statistics

	Round 1			Round 4			Round 5		
mean/sd	Total	Control	Treatment	Total	Control	Treatment	Total	Control	Treatment
Total income				11607	11322	11886	16530	15900	17127
				(6860)	(6940)	(6773)	(10876)	(10485)	(11206)
Village income	6702	6681	6712	11084	10809	11352	16080	15488	16640
	(4307)	(4402)	(4263)	(6602)	(6543)	(6652)	(10705)	(10307)	(11043)
Migration income				508	444	571	459	434	482
				(852)	(782)	(911)	(879)	(865)	(891)
Total consumption	12936	12916	12945	19824	19328	20304	20342	19722	20931
	(4260)	(4302)	(4242)	(8021)	(8019)	(7996)	(8779)	(8421)	(9071)
Food consumption	9741	9786	9720	13175	12868	13472	12756	12474	13024
	(3127)	(3090)	(3145)	(4878)	(4881)	(4860)	(4437)	(4329)	(4522)
Non-food consumption	3133	3069	3164	6439	6285	6589	7413	7072	7737
	(1743)	(1763)	(1732)	(4217)	(4285)	(4147)	(5863)	(5645)	(6048)
Household size	3.77	3.80	3.76	4.05	4.04	4.06	4.04	3.96	4.11
	(1.29)	(1.35)	(1.26)	(1.43)	(1.38)	(1.48)	(1.46)	(1.41)	(1.51)
Migrant household				0.41	0.39	0.44	0.39	0.38	0.40
				(0.49)	(0.49)	(0.50)	(0.49)	(0.48)	(0.49)
Number of households	1780	566	1214	2252	1112	1140	2167	1057	1110

Note: Income and consumption are annual per capita levels in Taka (approximately 75 Taka to the US$ in 2011). Consumption and calories include only non-migrant consumption and calories. Total income consists of wage, business, crop and other agriculture, asset income, other income such as lottery winnings or interest income, and migration income less migration costs. A migrant household is defined as a household that sent a migrant in the past four months. Round 1 did not collect migrant and migration income data.

Table 2 gives the summary statistics for the sample we use for estimation. Our main measures of interest are income obtained back home in the village, income obtained during migration episodes, and consumption. For each of these measures, we construct annual measures per person in the household.[2] Our measure of income consists of wage income, agricultural crop income, business income, and migration income. While income undoubtedly changes over the course of the year due to the seasonal nature of agriculture in Rangpur, our income data spans the full year, and we aggregate it up to the annual level. Because of this, our model does not have a seasonal component (e.g. a separate lean season and prime season), but rather focuses on income risk at the annual level.[3] Annual income was only collected in rounds 1, 4 and 5, hence we restrict our analysis to those years.[4] Our measure of consumption consists of 215 food items and 63 non-food items, some with a week recall and others with a bi-weekly or monthly recall. We annualize these measures to the yearly level in order to be consistent with our measure of income.[5] [6]

[2] We follow the household size definition used in Bryan et al. (2014), which includes all individuals who have been present in the house for at least seven days.

[3] Consistent with this, we find little evidence of aggregate *shocks* in the data. Rather, the Monga (lean) season is a predictable hungry period.

[4] Income was not collected in round 3, and round 2 only includes income from the previous 4 months.

[5] This also assumes that consumption is relatively smooth over the course of the year.

[6] As seen in Table 2 there is a gap between measured income and consumption. There are several possible explanations for this gap:

1. Seasonality: November is the leanest season, so we would expect consumption > income
2. Price indices used: The average price for rice in the production data is 11 Taka. In the consumption data, is 16 Taka. We adjust the price indices so that production is valued at the same prices as consumption.
3. Participation in social security programs. A large fraction of our sample receive public assistance. This may take the form of food aid, hence increasing consumption
4. Microfinance: almost all of the survey respondents are part of a microfinance group. We know from previous work that microfinance is often used for consumption.
5. Timing of survey questions: asked income expenditure over last year. Due to timing, possible that reporting most recent expenses for crop that hasn't harvested yet, and harvest from last year. This may lead to recall error in income.
6. Could just be missing a section of income: hard to collect well.

3 Reduced form evidence

Households in our sample have many potential ways to protect themselves against bad income realizations. As Table 3 shows, at baseline 29% of households report lending money to neighbors, family members, or friends, and the most common reasons were to help with food expenses (75%) and health expenses (11%). In addition, over half of the sample received credit from an NGO, and 15% received credit (with interest) from family or friends. Finally, over half the sample has positive savings. This high prevalence of direct transfers between households suggests that there is scope for the experiment to have affected the functioning of these networks.

Table 3: Savings, transfers, and credit at baseline

Baseline variable	Mean
Lend money to neighbors/family/friends	0.29
Amount lent in past 12 months (conditional)	429 Tk
Reason lent money: food	0.75
Reason lent money: health	0.11
Ever received credit from NGO	0.55
Ever received credit from family/friends (w/ interest)	0.15
Ever received credit from money lender (w/ interest)	0.15
Any current savings	0.53
Savings (conditional)	1416 Tk

In this section, we conduct direct tests of the effect of the experiment on several measures of transfers and risk-sharing. First, we examine how household responses to four hypothetical financial assistance scenarios differ between treatment and control villages in 2011. We find strong effects of treatment on the willingness to help others and ask for help from others. We then use detailed income and consumption data to test how the experiment affected the correlation between the two (in the spirit of Townsend (1994)).

Our results suggest that treated villages enjoyed better risk-sharing. Finally, we test the effect of the experiment on savings and find no significant changes to savings behavior, suggesting that the interpretation of the correlation tests is not a result of self-insurance, but instead a result of risk-sharing.

3.1 Effects on hypothetical financial assistance scenarios

In 2011, we collected data on each household's willingness to participate in a variety of informal and formal financial assistance arrangements. To measure the extent to which households could depend on others for assistance, households were asked whether family, friends, other villagers, NGOs, and moneylenders would be willing to help them financially, and if so whether the household would be willing to ask for help. In addition, to measure the extent to which households were willing to help others, households were asked whether family, friends, and other villagers would ask them for financial assistance, and if so whether the household would be willing to help.

Table 4 regresses these measures on treatment to test whether the experiment changed these beliefs in the financial arrangements between villagers. Each cell is a separate regression of the effect of treatment on whether the source denoted in the row would behave as described in the column. For all "informal" relationships (i.e. family, friends, and other villagers), the experiment significantly increased the willingness of households to interact financially. For example, 73% of households in control villages report that family members would help them, and treatment increases that percentage by 4.7 percentage points (first row, first column). These point estimates are consistently between 4-8 percentage points for *receiving* help from family, friends, or other villagers, and consistently between 9-11 percentage points for *providing* help. While there is also an effect on beliefs about relationships with NGOs, the effect for moneylenders is small and insignificant, suggesting that the experiment affected informal risk-sharing arrangements but less so formal relationships.

Table 4: Treatment effect on financial assistance from and to others

	Would help you	Would help you and you'd ask	Would ask you for help	Would ask you for help and you'd help
Family	0.047*	0.043*	0.111***	0.106***
	(0.026)	(0.026)	(0.033)	(0.031)
Control mean	[0.730]	[0.707]	[0.516]	[0.475]
Friends	0.081***	0.073**	0.096***	0.090***
	(0.031)	(0.030)	(0.029)	(0.027)
Control mean	[0.258]	[0.239]	[0.207]	[0.182]
Other villagers	0.069**	0.070**	0.106***	0.105***
	(0.028)	(0.027)	(0.031)	(0.026)
Control mean	[0.628]	[0.588]	[0.365]	[0.306]
NGOs	0.067**	0.071**		
	(0.030)	(0.029)		
Control mean	[0.540]	[0.494]		
Moneylenders	0.031	0.029		
	(0.021)	(0.020)		
Control mean	[0.208]	[0.180]		

Note: Data from round 4, and drops households that received treatment in round 4. Each cell is a separate regression of the effect of treatment on whether the source denoted in the row would behave as described in the column. Each regression also controls for upazila. Standard errors, clustered by village, are in parentheses, and the mean of the control group is in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

One concern with the interpretation that the experiment strengthened informal relationships within a village is that it may have increased the willingness to risk-share among particular households that were induced by the experiment to send a migrant, but not others. To examine this more closely, Tables 5 and 6 repeat the analysis separately for households that sent a migrant in 2009 and those that did not send a migrant.[7] As

[7]Of course, this sample split is endogenous to the decision to migrate, but at least provides suggestive

expected, the results for in the migrant sample are even stronger, but it is particularly interesting that the results for the non-migrant sample are still relatively large and significant, at least for providing assistance to others.[8]

Table 5: Treatment effect on financial assistance from and to others, migrant sample

	Would help you	Would help you and you'd ask	Would ask you for help	Would ask you for help and you'd help
Family	0.061	0.056	0.150***	0.139***
	(0.037)	(0.038)	(0.044)	(0.042)
Control mean	[0.729]	[0.714]	[0.497]	[0.462]
Friends	0.124***	0.107**	0.127***	0.106**
	(0.046)	(0.047)	(0.041)	(0.041)
Control mean	[0.322]	[0.312]	[0.266]	[0.246]
Other villagers	0.096**	0.081**	0.138***	0.121***
	(0.039)	(0.041)	(0.042)	(0.039)
Control mean	[0.568]	[0.518]	[0.327]	[0.266]
NGOs	0.105**	0.112***		
	(0.041)	(0.040)		
Control mean	[0.538]	[0.497]		
Moneylenders	0.017	0.021		
	(0.030)	(0.029)		
Control mean	[0.191]	[0.171]		

Note: Data from round 4, and drops households that received treatment in round 4 and households that did not send a migrant in round 2. Each cell is a separate regression of the effect of treatment on whether the source denoted in the row would behave as described in the column. Each regression also controls for upazila. Standard errors, clustered by village, are in parentheses, and the mean of the control group is in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

There are three takeaways from these results. First, there is a strong norm that households would provide and receive financial assistance among each other, as shown in the control means of Table 4. Second, the point estimates show that the migration experiment significantly increased the willingness of households to participate in these arrangements, particularly the informal arrangements. Finally, this increase is not limited to house-

evidence of risk-sharing benefits spilling over to households in the village that did not receive the direct migration incentives provided by the experiment.

[8] The coefficient on NGOs for the control villages is most likely linked to the fact that a NGO implemented the migration subsidy experiment.

Table 6: Treatment effect on financial assistance from and to others, non-migrant sample

	Would help you	Would help you and you'd ask	Would ask you for help	Would ask you for help and you'd help
Family	0.040	0.036	0.083**	0.080**
	(0.029)	(0.029)	(0.037)	(0.035)
Control mean	[0.715]	[0.689]	[0.511]	[0.466]
Friends	0.045	0.042	0.067**	0.072**
	(0.032)	(0.030)	(0.032)	(0.029)
Control mean	[0.260]	[0.234]	[0.223]	[0.195]
Other villagers	0.051	0.060*	0.082**	0.095***
	(0.034)	(0.034)	(0.036)	(0.028)
Control mean	[0.619]	[0.573]	[0.331]	[0.271]
NGOs	0.039	0.042		
	(0.035)	(0.033)		
Control mean	[0.582]	[0.531]		
Moneylenders	0.032	0.026		
	(0.023)	(0.022)		
Control mean	[0.181]	[0.158]		

Note: Data from round 4, and drops households that received treatment in round 4 and households that sent a migrant in round 2. Each cell is a separate regression of the effect of treatment on whether the source denoted in the row would behave as described in the column. Each regression also controls for upazila. Standard errors, clustered by village, are in parentheses, and the mean of the control group is in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

holds who were induced to migrate: non-migrant households in treatment villages also reported an increase in the ability to use these informal arrangements.

One limitation to this analysis is that these measures of risk-sharing are hypothetical in nature. In the next sub-section, we turn to outcome-based measures of risk-sharing to examine whether the experiment in fact changed the amount of risk-sharing taking place within villages.

3.2 Effects on the correlation between income and consumption

We now use the data and the experimental variation to investigate the extent to which income relates to consumption, based on the specification in Townsend (1994). We test two key ideas. First, under within-community full insurance, consumption should only depend on aggregate community-level income fluctuations. Second, the ability to migrate more easily may affect the transmission of both aggregate and idiosyncratic shocks (the latter, if full insurance is absent).

To implement these tests we regress log of per-capita household consumption on log per-capita household income with the following regression:

$$\log C_{ivt} = \gamma_{vt} + \alpha \log Y_{ivt} + \epsilon_{ivt} \tag{1}$$

where $\log C_{ivt}$ and $\log Y_{ivt}$ are household i's log per capita consumption and income, respectively, in village v at time t.[9] γ_{vt} are village-year fixed effects that capture the effects of aggregate shocks on consumption.[10] The main parameter of interest is α, which captures the correlation between income and consumption, conditional on aggregate fluctuations.

Table 7: Consumption smoothing among control villages

	(1) Log total consumption	(2) Log food consumption
Log income	0.197***	0.174***
	(0.015)	(0.014)
Observations	2169	2169
R-squared	0.229	0.232

Note: The sample includes households in control villages in the 2011 and 2013 rounds of the survey. The dependent variable is log annual per capita total consumption in column (1) and log annual per capita food consumption in column (2). The independent variable is log annual per capita total income. Both regressions include village-round fixed effects. Standard errors, clustered by village, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7 reports the results of equation (1) for total consumption (column 1) and food

[9] Consumption and income are converted into per capita terms by dividing by the number of household members who have been present in the house for at least seven days.

[10] We also run regressions with household fixed effects (δ_i) and find similar results. Our preferred specification does not incorporate household fixed effects because we only have two datapoints for each household.

consumption (column 2) in the sample of control villages in 2011 and 2013. In both cases, income (conditional on village-year fixed effects) has a significant impact on consumption, confirming the absence of complete insurance: a 10% increase in income corresponds to a 2.0% increase in total consumption and a 1.7% increase in food consumption.

The next set of regressions leverage the experimental variation in the data to test whether an increase in migration affects the level of village insurance.[11] We augment equation (1) to allow the transmission parameter α to vary by whether the village is in the treatment sample[12]

$$\log C_{ivt} = \gamma_{vt} + \alpha_0 \log Y_{ivt} + \alpha_1 \left(\log Y_{ivt} * T_v \right) + \epsilon_{ivt} \tag{2}$$

where T_v is an indicator variable taking a value of 1 if the village is a treatment village. The parameter of interest in this regression is α_1, which captures the effect of the migration treatment on the correlation between income and consumption.[13]

Table 8 reports the transmission parameter (α_1), again for total consumption (columns 1-3) and food consumption (columns 4-6). The first row shows that for the overall sample, the migration treatment significantly improved risk sharing opportunities. Specifically, treatment reduced the effect of income on both total consumption and food consumption by 4 percentage points. Compared to a baseline exposure of consumption to 17-20 percent of income shocks (Table 7), the migration treatment cuts this exposure by around 20%,

[11] Bryan et al. (2014) shows that the experiment significantly increased migration rates in 2008 by 22 percent points, in 2009 by 9 percentage points, and in 2011 by 7 percentage points.

[12] There were other experiments in 2011 and 2013, which we control for through the village-round fixed effects. Specifications that also test the 2011 and 2013 treatment effects yield similar results for the 2008 treatment effect.

[13] We also run a difference-in-differences specification that incorporates pre-treatment data from 2008. Results are very similar and are available on request. However, since the model in Section 4 is estimated only on post-treatment data (2011 and 2013), we highlight the post-treatment results in this section for consistency.

Table 8: Effect of migration incentives on consumption smoothing

	Log total consumption			Log food consumption		
	(1)	(2)	(3)	(4)	(5)	(6)
Overall treatment effect	-0.042**			-0.038**		
	(0.020)			(0.019)		
Group restrictions						
Unassigned group		-0.053**			-0.047**	
		(0.024)			(0.023)	
Self-formed group		-0.021			-0.014	
		(0.028)			(0.029)	
Assigned group		-0.050*			-0.047*	
		(0.030)			(0.027)	
Destination restrictions						
Unassigned destination			-0.054**			-0.050**
			(0.022)			(0.023)
Assigned destination			-0.030			-0.025
			(0.025)			(0.023)
Observations	4419	4419	4419	4421	4421	4421
R-squared	0.205	0.205	0.206	0.206	0.206	0.206

Note: The sample includes households from the 2011 and 2013 rounds of the survey. The dependent variable is log annual per capita total consumption in columns (1)-(3) and log annual per capita food consumption in columns (4)-(6). The main independent variable is log annual per capita income, interacted with the respective treatment variable. Other control variables include village-round fixed effects. Standard errors, clustered by village, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

implying a substantial increase in village insurance.[14]

The remaining rows in Table 8 use experimental variation stemming from the attractiveness of the migration offer. In the first set, migration offers were either (1) conditional on migrating in an assigned group, (2) conditional on migrating in a self-formed group, or (3) unconditional. These distinctions create variation in offer attractiveness, since the unconditional offer dominates the self-formed group offer, which dominates the assigned group offer. The results in rows 2-4 confirm this overall: individuals with the most attractive treatment experience the largest consumption smoothing effect, while those with a less attractive treatment experience smaller effects. In the second set of restrictions, some migration offers were conditional on migrating to an assigned destination. The results in rows 5 and 6 again show that individuals with the more attractive offer (the unassigned destination) experienced better risk-sharing than those with the less attractive offer (assigned destination).

One concern with the results in Table 8 is that our ability to measure income for treatment households may be worse than for control households, either because migration income is inherently more difficult for the econometrician to capture, or because migration income is easier to hide both from other households and from the econometrician. If such measurement error were classical, either of these measurement issues would bias downwards the treatment effect estimates. To investigate this, we repeat the analysis among households who did *not* send a migrant in Table 9. While this is clearly an endogenously selected sample, our ability to measure their income should not vary by treatment, and

[14]These estimates are particularly large given that they are intent-to-treat estimates. IV estimates that instrument household migration with treatment may be problematic because treatment is at the village level, not the household, so treatment of a neighbor household affects one's own consumption through risk-sharing, not only through own migration.

Table 9: Effect of migration incentives on consumption smoothing, non-migrant sample

	Log total consumption			Log food consumption		
	(1)	(2)	(3)	(4)	(5)	(6)
Overall treatment effect	-0.048*			-0.046**		
	(0.026)			(0.023)		
Group restrictions						
Unassigned group		-0.073**			-0.070**	
		(0.030)			(0.029)	
Self-formed group		-0.002			0.006	
		(0.038)			(0.037)	
Assigned group		-0.061*			-0.065**	
		(0.037)			(0.032)	
Destination restrictions						
Unassigned destination			-0.060**			-0.049*
			(0.030)			(0.029)
Assigned destination			-0.036			-0.043
			(0.032)			(0.029)
Observations	2615	2615	2615	2626	2626	2626
R-squared	0.234	0.236	0.235	0.232	0.234	0.233

Note: The sample includes households from the 2011 and 2013 rounds of the survey who did not send a migrant. The dependent variable is log annual per capita total consumption in columns (1)-(3) and log annual per capita food consumption in columns (4)-(6). The main independent variable is log annual per capita income, interacted with the respective treatment variable. Other control variables include village-round fixed effects. Standard errors, clustered by village, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

hence this exercise should help to address the measurement concerns. The results show that risk-sharing also improves for non-migrant households, suggesting that measurement concerns are not driving the treatment effects we see in Table 8. Similarly to the results in Table 6 the non-migrant results also suggest that the experiment changed the equilibrium between all households in the risk-sharing network; it was not just those who were induced to migrate whose ability to share risk was affected.

3.3 Effects on savings

A change in the correlation between income and consumption does not necessarily imply a change in risk-sharing. An alternative smoothing mechanism is savings. In Table 10, we test the effect of the experiment on savings over the 2009-2013 period. Columns (1) and (2) show that there is no significant effect of treatment on the percentage of households that have any savings nor the total amount of savings, respectively. The remaining columns separate the sample into migrants and non-migrants and also show no effect.

These null savings results, coupled with strong income-consumption correlation results and informal financial assistance results, suggests that the experiment had a significant impact on the willingness and ability of villagers to share risk. To understand the mechanism behind these findings, we next turn to a model of endogenous migration and risk-sharing.

Table 10: Treatment effect on savings

	Everyone		Migrant sample		Non-migrant sample	
	Any	Amount	Any	Amount	Any	Amount
Treatment	0.0034	1.00	0.0082	-12.5	-0.0084	18.9
	(0.034)	(24.9)	(0.049)	(37.1)	(0.041)	(33.9)
Control mean	0.57	214.5	0.58	333.6	0.57	273.6
N	1865	1864	950	949	913	913

Note: Data from round 2. Columns (3) and (4) only include households that sent a migrant in round 2, and columns (5) and (6) only include households that did *not* send a migrant in round 2. The dependent variables are whether the household has any savings (first column of each grouping) and how much savings the household has in Taka including zeros (second column of each grouping). Each regression also controls for upazila. Standard errors, clustered by village, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4 Joint model of risk sharing and migration

To understand the mechanisms by which the experiment increased risk-sharing, we develop and estimate a joint model of income and risk sharing in which migration is temporary and endogenous. Households are risk averse and derive utility over consumption and migration (costs). Village and migration income are both uncertain, and migration income is only revealed if migration is undertaken. Risk sharing is endogenous, and is constrained by limited commitment.

4.1 Village and migration income processes

We model a household's income in the village as an AR(1) process. Specifically, village log income Y_{it}^{v} of household i in year t is a function of three components: (1) a constant term a^{v}, (2) a risky component u_{it}^{v}, and (3) measurement error x_{it}^{v}.

$$\log Y_{it}^{v} = a^{v} + u_{it}^{v} + x_{it}$$

Measurement error is mean zero and serially uncorrelated, and the risky component follows an AR(1) process with peristence ρ^v and yearly innovations η_{it}^v that are mean zero and serially uncorrelated.

$$u_{it}^v = \rho^v u_{i,t-1}^v + \eta_{it}^v$$

Migration income, for now, is simply a transitory shock: $\log Y_{it}^m = e_{it}^m$.

4.2 Model timing

In each period we distinguish the time before the migration decision (ex ante) and the time following the migration decision (ex-post). In the first sub-period the household receives an income draw in the village, and makes a decision about whether or not to migrate. In the second sub-period, the migration outcome is realized if a household member migrates, informal transfers occur, and consumption is realized. Migration is temporary, so at the end of the second sub-period all migrants have returned to the village.

4.3 Limited commitment

We solve the problem as one where a planner mandates migration decisions and enacts transfers across the entire village community, pooling resources while at the same time respecting the participation constraints, implied by the possibility that households may decide to move into autarky permanently and the total resource constraint in the village. In addition to transfers the planner must also makes promises about future utilities in order to make sure the participation constraints are satisfied. At present our model is stationary and there are no aggregate village shocks over the year.

When deciding on current and future allocations for a particular household the relevant state variables are i) the realized state of village income at the start of the period, denoted by y_j which can take one of J possible values ($j \in 1, ..., J$), with probability π_j and ii) the level of state-dependent expected (ex-ante) utility w_{jk} promised to the agent with realization y_j for each of the possible K migration outcomes ($w_{jk}, k \in 0, ..., K$). Given the state variables the planner solves for:

1. The contemporaneous utility level h_{jk} (which then implies a transfer, τ, and a level of consumption)
2. The migration rule $\mathbb{I}$
3. Tomorrow's continuation values, $w'_{j'k'}$ for future village income state j' and migration states k'.

In the model risk sharing is constrained by limited commitment: because households cannot commit to making transfers in the future, all contemporaneous transfers must give the participant at least as much utility from continuing to participate in the risk sharing agreement as they would have if they exited the agreement and remained in autarky. This means that we need to track the value of autarky at the two sub-points within the time period: first, the value of autarky at the start of the period (before the migration decision), where the participant only knows their village income y_j and has an expectation over their migration outcome, and then the ex-post value of autarky, once the migration decision has been made and migration income uncertainty resolved.

The ex-ante value of autarky, which only depends on the income in the village, y_j, is given by the following functional equation:

[15]

$$\text{Ex ante autarky:} \qquad \hat{\Omega}(y_j) = \max_{\mathbb{I}} \left\{ (1-\beta) \sum_k \pi_k \, u\left[(1-\mathbb{I})y_j + \mathbb{I}y_k, \mathbb{I}\right] + \beta \sum_{j'} \tau_{j'} \hat{\Omega}(y_{j'}) \right\}$$

and ex-post autarky, which depends on whether the participant migrated ($\mathbb{I}$) and the migration income realization y_k if they migrated, is given by:

$$\text{Ex post autarky:} \qquad \Omega(y_j, y_k, \mathbb{I}) = (1-\beta) \, u\left[(1-\mathbb{I})y_j + \mathbb{I}y_k, \mathbb{I}\right] + \beta \sum_{j'} \tau_{j'} \hat{\Omega}(y_{j'})$$

where we assume that migrating has a utility cost d: $u(c, \mathbb{I}) = u(c) - d\mathbb{I}$.

We follow Krueger and Perri (2010) and find the allocations by minimizing the cost of allocating current utility and future contingent promised utilities to a household coming into the period with realization y^j and an associated vector of promised utilities depending on the realization of migration income. The tradeoff between the future and the present has to be consistent with the overall resource constraint in the village. This can be ensured by finding an intertemporal price R that ensures equilibrium in the village.

Specifically, we can write $C(h_{jk}, \mathbb{I}) = u^{-1}(h + d\mathbb{I})$ as the cost to the social planner of providing h_{jk} level of contemporaneous utility. For each point of the support of village income y_j the social planner solves:

$$V(s_j) = \min_q \left\{ V^{\text{mig}}(s), V^{\text{no mig}}(s) \right\} \tag{3}$$

[15] Alternatively, can write

$$\hat{\Omega}(y_j) = \sum_k \pi_k \Omega(y_{jk}, \mathbb{I}^*)$$

where $\mathbb{I}^*$ is the migration that is optimal under autarky.

where the state variables are $s_j = (y_j, w_{j0}, w_{j1}, ..., w_{jK})$, the decision variables are $q = (\mathbb{I}, h_0, h_k, w_{j'0'}^{'\text{mig}_k}, w_{j'k'}^{'\text{mig}_k}, w_{j'0'}^{'\text{no mig}}, w_{j'k'}^{'\text{no mig}})$ for all $k \in 1, ...K$, $j' \in 1, ...J$, and $k' \in 1, ..., K$,[16] and where the value conditional on migrating is given by

$$V^{\text{mig}} = \min \sum_k \pi_k \Big[\left(1 - \frac{1}{R}\right) C(h_{jk}) + \frac{1}{R} \sum_{j'} \tau_{j'} V(y_{j'}, w_{j'0'}^{'\text{mig}_k}, w_{j'1}^{'\text{mig}_k}, ..., w_{j'K'}^{'\text{mig}_k}) \Big]$$

while the value conditional on not migrating is

$$V^{\text{no mig}} = \min \Big[\left(1 - \frac{1}{R}\right) C(h_{j0}) + \frac{1}{R} \sum_{j'} \tau_{j'} V(y_{j'}, w_{j'0'}^{'\text{no mig}}, w_{j'1}^{'\text{no mig}}, ..., w_{j'K'}^{'\text{no mig}}) \Big]$$

subject to:

(1) ex-post participation constraints:

$$\begin{aligned}
\Omega(y_{j'k'}, \mathbb{I}) &\leq w_{j'k'}^{'\text{mig}_k} && \forall k, j', k' \\
\Omega(y_{j'}, \mathbb{O}) &\leq w_{j'0'}^{'\text{mig}_k} && \forall k, j' \\
\Omega(y_{j'k'}, \mathbb{I}) &\leq w_{j'k'}^{'\text{no mig}} && \forall j', k' \\
\Omega(y_{j'}, \mathbb{O}) &\leq w_{j'0'}^{'\text{no mig}} && \forall j'
\end{aligned}$$

[16] For clarity, the subscripts without a tick (′) refer to the current period and the subscripts with a tick refer to the next period. The $j0$ and $j'0'$ subscripts denote non-migration in the current and next period, respectively. The superscript tick indicates that the variable relates to the next period, and "mig$_k$" and "no mig" superscripts refers to the migration outcome this period.

(2) ex-ante participation constraints:

$$\hat{\Omega}(y_{j'}) \leq \mathbb{I}' \sum_{k'} \pi_{k'} w_{j'k'}^{'\text{no mig}} + (1 - \mathbb{I}') w_{j'0'}^{'\text{no mig}} \qquad \forall j'$$

$$\hat{\Omega}(y_{j'}) \leq \mathbb{I}' \sum_{k'} \pi_{k'} w_{j'k'}^{'\text{mig}_k} + (1 - \mathbb{I}') w_{j'0'}^{'\text{mig}_k} \qquad \forall k, j'$$

and (3) promise-keeping constraints defining how the promised utilities are to be allocated in terms of contemporaneous utility and future promises:

$$w_{j0} = (1 - \beta) h_{j0} - \beta \sum_{j'} \tau_{j'} (\mathbb{I}' \sum_{k'} \pi_{k'} w_{j'k'}^{\text{no mig}} + (1 - \mathbb{I}') w_{j'0'}^{\text{no mig}})$$

$$w_{jk} = (1 - \beta) h_{jk} - \beta \sum_{j'} \tau_{j'} (\mathbb{I}' \sum_{k'} \pi_{k'} w_{j'k'}^{\text{mig}_k} + (1 - \mathbb{I}') w_{j'0'}^{\text{mig}_k}) \qquad \forall k$$

The intertemporal price then is found by requiring that the aggregate resource constraint is satisfied. The associated first order conditions and the solution algorithm, are in Appendix A.

5 Estimation

We begin by estimating a model of endogenous risk sharing without migration. The full set of parameters are shown in Table 11, and we estimate parameters in two main steps. In the first step, we estimate the parameters of the income process outside the limited commitment model, and in the second step we estimate the remaining risk aversion and consumption parameters. The parameters estimated within the model of risk-sharing are estimated using only the data from control villages; we then compare the data from the treatment villages to model-generated ‘treatment’ data to validate our specification of the model.

Table 11: Model parameters

Parameter	Description	Identification	Value
Village Income Parameters			
a_c^v, a_t^v	Log income constant	E(log Y), E(log C)	0.59, 1.45
$\text{var}(\eta_c^v)$, $\text{var}(\eta_t^v)$	Variance of persistent shock	Moment estimation	0.07, 0.07
ρ_c^v, ρ_t^v	Persistence	Moment estimation	0.80, 0.51
$\text{var}(x^v)$	Variance of income meas. error	Moment estimation	0.21
Other Parameters			
$\text{var}(z^v)$	Variance of cons. meas. error	Var(log C_c)	0.06
γ	Risk aversion	Risk-sharing α_c	1.08
β	Discount factor	Calibrated	0.9

Note: Superscript ‘v’ denotes village, and subscripts ‘c’ and ‘t’ denote control and treatment parameters, respectively.

5.1 Estimation of the income process

The village income process is defined by the constant a^v, the variance of the persistent shock $\text{var}(\eta^v)$, the persistence parameter ρ^v, and the variance of measurement error $\text{var}(x^v)$. We allow the experiment to change the mean, the variance of the persistent shock and the persistence parameter, and hence we estimate seven income parameters.

To begin, we remove variation in $\log Y^v$ and $\log C$ due to village-round fixed effects, while still preserving the mean. This allows for consistency with the reduced form results in Section 3. The constant of the log income process is then identified by mean log income or consumption: $E(\log Y^v) = E(\log C) = \frac{a^v}{1-\rho^v}$.[17]

For the remaining income parameters, we build on Meghir and Pistaferri (2004) by specifying the following equations from the covariance structure of the income process:

[17] We use mean log consumption due to scaling issues. See Appendix B for more details.

$$var(\Delta \log Y_t^v) = \frac{2(1-\rho^v)}{1-(\rho^v)^2} var(\eta^v) + 2var(x^v) \tag{4}$$

$$cov(\Delta \log Y_t^v, \Delta \log Y_{t-1}^v) = -\frac{(1-\rho^v)^2}{1-(\rho^v)^2} var(\eta) - var(x^v) \tag{5}$$

$$cov(\Delta \log Y_t^v \log Y_t^v) = \frac{(1-\rho^v)}{1-(\rho^v)^2} var(\eta^v) + var(x^v) \tag{6}$$

$$cov(\Delta \log Y_t^v \log Y_{t-1}^v) = \frac{(\rho^v-1)}{1-(\rho^v)^2} var(\eta^v) - var(x^v) \tag{7}$$

$$cov(\Delta \log Y_t^v \log Y_{t-2}^v) = \frac{\rho^v(\rho^v-1)}{1-(\rho^v)^2} var(\eta^v) \tag{8}$$

$$cov(\Delta \log Y_t^v \log Y_{t+1}^v) = \frac{\rho^v(1-\rho^v)}{1-(\rho^v)^2} var(\eta^v) \tag{9}$$

in which $\Delta \log Y_t^v$ denotes the difference in $\log Y_t^v$ over one year: $\Delta \log Y_t^v = \log Y_t^v - \log Y_{t-1}^v$. Identification comes easily from this set of moments: summing the first moment with twice the second moment, and dividing by the last moment identifies ρ^v, then either of the last moments identifies $var(\eta^v)$, and finally any of the first four moments identifies $var(x^v)$. A similar argument identifies the first two parameters separately for treatment and control villages.

To estimate these parameters, we calculate the left-hand side of these equations from data, and use generalized method of moments with a diagonal weighting matrix (Altonji and Segal, 1996; Blundell et al., 2008).[18] We compute standard errors using the block bootstrap, clustering at the village level (Hall and Jorowitz, 1996; Horowitz, 2001) to account for arbitrary spatial correlation between households in a village.

The results in the right-most column of Table 11 show a high degree of persistence

[18]The moments used in estimation are a modified version of the equations above to account for longer time differences between rounds of data. Details of these moments are available on request.

over time, as well as a high degree of measurement error (var(x^v)=0.21). The variance of the persistent shock did not change with treatment (var(η_c^v)=var(η_t^v)=0.07), while the persistence parameter decreased significantly with treatment ($\rho_c^v = 0.80$ to $\rho_c^v = 0.51$).

We now turn to the estimation of the limited commitment model to understand potential mechanisms by which this significant change in the income process translates into risk-sharing.

5.2 Estimation of parameters within the endogenous risk-sharing model

Within the model of risk-sharing, we estimate two remaining parameters: (1) risk aversion γ and (2) the variance of consumption measurement error var(z^v). For this purpose we use two moments: (1) the risk-sharing coefficient α from a regression of $\log(C_t)$ on $\log(Y_t)$, similar to those from Section 3 and (2) the variance of consumption var(C).[19] The risk-sharing coefficient α identifies the risk aversion parameter γ because as risk aversion increases, household preference for a more certain consumption stream increases, and therefore they should be more willing to share risk (and hence have a lower risk-sharing coefficient). The variance of consumption var(C) then identifies the variance of consumption measurement error var(z^v) because, after accounting for risk-sharing, the remaining variation in consumption comes from measurement error (see Appendix XX) for the derivation.

To estimate γ and var(z^v), we construct data moments of α and var(C) using *control households only* in rounds 4 and 5, and use the simulated method of moments (McFadden,

[19] The risk-sharing coefficients from this section and Section 3 are slightly different because the coefficients in Section 3 use rounds 4 and 5 (post-experiment) as well as round 1 (baseline, pre-experiment), while the coefficients in this section only use the post-experiment rounds.

1989) to pin down parameter estimates that match model-simulated moments to the data moments as best as possible. With these structural parameter estimates, we then use the model to predict how the experiment would affect risk sharing and the variance of consumption in the treatment data.[20]

The estimates of the structural parameters are again in the right-most column of Table 11. The variance of consumption measurement error is considerably smaller than that of income measurement error, and households are relatively risk averse (a CRRA parameter of 1.08). The model fit and out-of sample treatment predictions are shown in Table 14. The first two columns, which correspond to the control group, are matched in estimation; the model fits the data well. The treatment data moments are in column 3, and the the fourth column uses the structural parameter estimates in addition to the treatment income parameter estimates to predict the risk-sharing coefficient and variance of log consumption for the treatment group. The model predicts more risk-sharing in the treatment group than the data (0.06 in the model versus 0.16 in the data). This may also explain why the model's variance of log consumption in the treatment group is lower than the treatment data, since more risk-sharing leads to a lower variance of consumption.

To visualize the effect of the change in income parameters on risk-sharing in the model, Figure 2 shows the relationship between the income persistence (ρ^v) on the x-axis and the variance of the persistent shock ($\text{var}(\eta^v)$) on the y-axis on the risk-sharing coefficient α. Cooler colors (green) signify better risk-sharing, while warmer colors (red) signify worse risk-sharing. The arrow denotes the change in the income parameters from

[20] Alternatively, we could have used the treatment data as additional moments in estimation. By estimating the structural parameters on the control data only, we trade off the efficiency gains from the additional moments in favor of the ability to validate our model specification by predicting the treatment effect on risk-sharing.

Table 12: Model estimaton and out-of-sample prediction

	Control (estimation)		Treatment (prediction)	
	Data	Model	Data	Model
Targeted moments				
Risk-sharing beta	0.19	0.19	0.16	0.06
Variance of consumption	0.13	0.13	0.13	0.09
Estimated parameters				
Estimated coeff. relative risk aversion		1.12		
Estimated measurement error variance (cons)		0.07		

Notes: Table shows the risk-sharing coefficient and the variance of log consumption in the data (columns 1 and 3) and the model (columns 2 and 4). Columns 1 and 2 correspond to the control group and columns 3 and 4 correspond to the treatment group. The structural parameters are estimated on data from control villages only by matching columns 1 and 3. Column 4 predicts the moments using these structural parameter estimates.

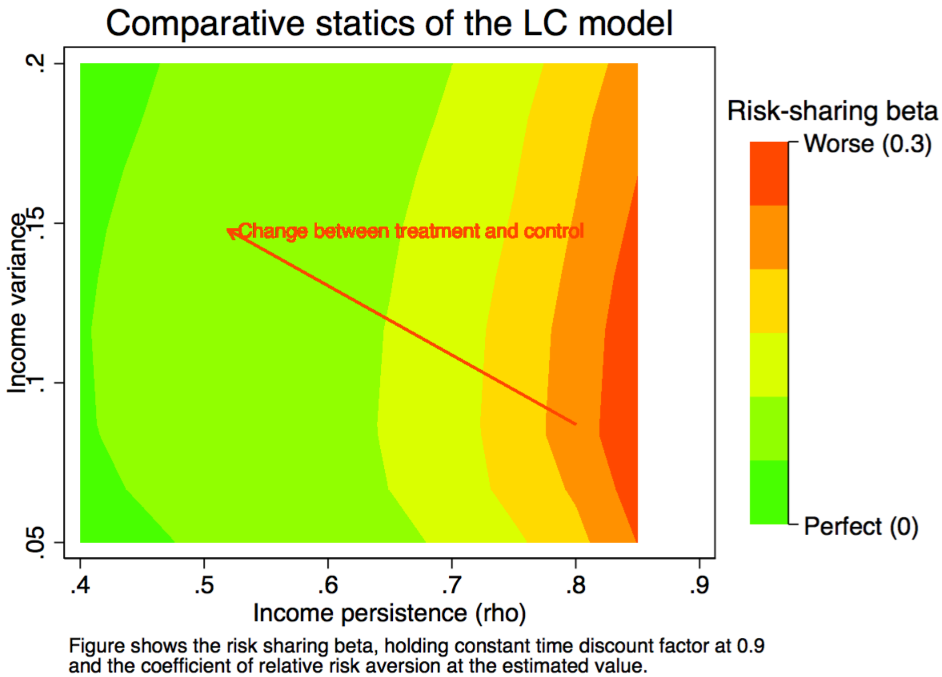
control to treatment, and shows that the model predicts that risk-sharing improves as the persistence parameter decreases, as shown in Table 14.

The estimates in Tables 11 and 14 do not take into account the role of migration in influencing both the income distribution or endogenous risk-sharing. Indeed, the fact that the model without migration over-predicts the treatment effect on risk-sharing suggests a role for migration as a mediator of endogenous risk-sharing. We next turn to estimation of the model of migration and endogenous risk-sharing.

5.3 Incorporating migration

We incorporate migration in the estimation by assuming that the experiment did not change the underlying income processes; rather, it changed the cost of migrating and therefore how likely someone was to migrate to the city. We estimate the income process off the control villages only.

Figure 2: Income parameters and risk-sharing with limited commitment



The underlying migration income model is a simple AR(1) process with measurement error.[21] However, we only observe migration income for households that send a migrant, so the observed migration income distribution may be truncated due to who decides to migrate. To control for this selection, we propose a reduced form model of the migration decision, using treatment as an instrument for migration that does not otherwise affect migration income.

Migration income and unexplained migration income growth are defined as:

$$\log Y_{it}^m = a_{it}^m + u_{it}^m + r_{it}^m$$

$$\Delta u_{it}^m = u_{it}^m - u_{i,t-1}^m = (\rho^m - 1)u_{i,t-1}^m + \eta_{it}^m + \Delta r_{it}^m$$

[21] For now, it is actually IID (zero persistence) but the identification argument goes through either way.

in which a^m_{it} is mean log migration income, u^m_{it} is the stochastic component which has persistence ρ^m and shock η^m_{it}, and r^m_{it} is measurement error. Village income is defined similarly, with subscripts v. The problem is that we do not see potential migration income for those who do not migrate, so this is a classic selection problem. We model the decision to migrate as:

$$M_{it} = 1 \quad \text{if} \quad Z'_{it}\delta + N'_{it}\gamma + \varepsilon_{it} > 0$$

in which N'_{it} are exogenous variables that are not included in either village or migration income equations and Z'_{it} are variables that appear in both the migration decision and income growth equations. N'_{it} are indicators of treatment-by-round, and Z'_{it} are round fixed effects and potential village income.[22] ε_{it} is an unexplained component to the decision to migrate, such as heterogeneous utility costs of migrating.

We assume that the distribution of the unexplained components of village income, migration income, and the migration equation, η^v_{it}, η^m_{it}, and ε_{it}, are uncorrelated. Specifically,

$$\begin{pmatrix} \eta^v_{it} \\ \eta^m_{it} \\ \varepsilon_{it} \end{pmatrix} \sim N \left[\begin{pmatrix} 0 \\ 0 \\ 0 \end{pmatrix}, \begin{pmatrix} \sigma^2_{\eta^v} & & \\ 0 & \sigma^2_{\eta^m} & \\ 0 & 0 & 1 \end{pmatrix} \right]$$

With this distribution, we simulate many individuals over many time periods, and match several simulated moments with the data moments to identify the key parameters of in-

[22]We observe village income for households that do not send a migrant, and assume that households who send a migrant still receive 90% of potential village income, hence we can back out 'potential' village income for both migrant and non-migrant households.

terest: the ρ parameters and σ_η^2 parameters (as well as covariances), though they are not of interest for the risk-sharing model. The particular moments we match are those we use to estimate the village income process as well as mean migration income growth.[23]

The specific steps we follow are:

1. Using data on treatment status and migration decisions for rounds 2, 4, and 5, we run a probit of migration M_{it} on round fixed effects and treatment-by-round and obtain estimates $\hat{\delta}$ and $\hat{\gamma}$.

2. We calculate the moments of the (selected) migration income distribution in the data.

3. We calculate the moments of the (selected) migration income distribution in the simulated model, using the following steps:

   (a) Simulate the income process and ε_{it} for 10,000 households over 1,005 periods, and drop all but periods 1,002, 1,004, and 1,005 (to correspond to rounds 2, 4, and 5 in the data).

   (b) Assign half of the sample as control, half as treatment, and assign $Z_{it}'\hat{\delta} + N_{it}'\hat{\gamma}$.

   (c) Apply the decision rule that household i sends a migrant in period t if

$$\varepsilon_{it} > -(Z_{it}'\hat{\delta} + N_{it}'\hat{\gamma})$$

   (d) Drop simulated observations in which the households does not send a migrant.

[23] We conjecture that the identification arguments will follow from (?) and (?); confirming this conjecture is in progress.

Now we have a (selected) simulated sample of households.

4. Solve for the unknown parameters by matching the simulated moments to the data moments and iterating over guesses of these parameters until the criterion function is met.

The results of this procedure are in Table 13.

Table 13: Estimated income parameters

	(1) Village income	(2) Migration income
Mean of log income	2.13	1.96
Income persistence	0.50	
Variance of income	0.11	0.30
Measurement error	0.31	0.50

Notes: Source: estimated by GMM. Estimated opportunity cost if migrate is 10% of village income. Parameters estimated from data in control villages only.

We then follow the same procedure to fit the model to the control data, taking the migration income process as given. We now estimate one additional parameter, migration cost, in addition to the risk sharing coefficient and the measurement error in consumption. We target the migration rate to identify the migration cost parameter. The fit of the model is given in Table 14. We are able to match the joint risk-sharing and migration behavior.

We then simulate the experiment using the estimated model. We do this by scaling the estimated migration cost to fit the impact of treatment on migration: while the experiment awarded a fixed dollar amount of $8.50 this does not necessarily relate directly to the estimated migration cost which relates to a utility loss and is not a corresponding money

Table 14: Fit of model to data: control

	Data	Model
Targeted moments		
Risk-sharing beta	0.20	0.21
Variance of consumption	0.12	0.13
Mean migration rate	0.38	0.38
Estimated parameters		
Coeff. relative risk aversion		1.57
Measurement error variance (cons)		0.11
Migration cost		0.06
Set exogenously		
Discount factor		0.90

Notes: Estimated on data from control villages only.

metric. [24]

The results of this exercise is shown in Figure 3. The model generates an improvement in risk-sharing as reflected in a reduction in the coefficient of income in the Townsend regression, although the the magnitude of the reduction is smaller than the treatment effect. The model also predicts an increase in migration as expected. As shown in the right hand panel, the value of the $8.50 subsidy is equivalent to a reduction of 20% of the migration cost.

6 Counterfactuals

We use data from new experiments run in these villages in 2014 (Akram et al., 2016) to validate the model's predictions out-of-sample. The new experiments varied the proportion of the village network that received migration subsidy offers simultaneously.

[24] A related point is made in Attanasio et al. (2012) for their evaluation of PROGRESA.

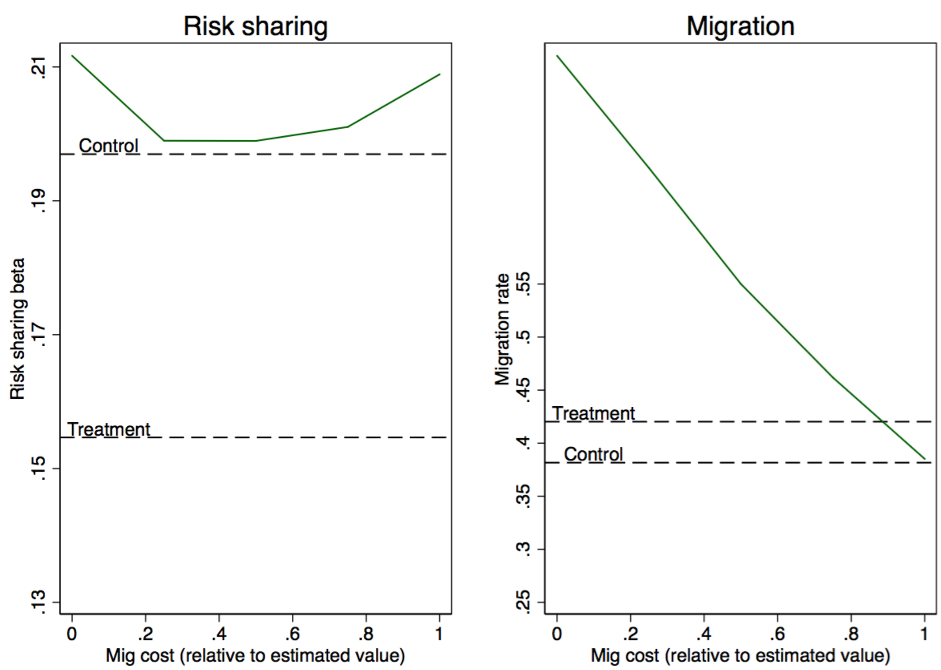
[In progress]

7 Conclusion

Life in developing countries is risky. Households have several risk mitigation options to choose from. However, in order to understand the benefits of migration it is important to understand how migration interacts with other risk-mitigation strategies, such as informal insurance.

Using a large scale randomized experiment that incentivized households to migrate we show the effects of increasing access to migration on informal risk sharing. We do this using two complementary analyses. First, we show that using an omnibus measure of risk sharing, the experiment improved risk sharing. Given the reduced form results, we then estimate a model of risk sharing with migration, which allows for the endogeneity of migration decisions and can quantify the amount of risk sharing that takes place as well as provide a way of carrying out counterfactual simulations to show the effects of migration costs on risk sharing networks. We find model predicts an increase in risk-sharing as a result of the experiment.

Figure 3: Simulating the experiment within the model



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A Structural model

A.1 Algorithm

As Krueger and Perri (2010) show, a limited commitment model with a continuum of agents can be solved computationally through policy function iteration by exploiting the first order conditions of the problem. However, our model introduces a discrete migration choice, which renders the problem in equation (3) non-differentiable. Our solution to this computational issue is replace a discrete choice over migration with a probability of migration, which is defined as a function of the values of migrating and not migrating, smoothed by a parameter $\lambda > 0$ (Horowitz, 1992):

$$p = \frac{\exp(\frac{V^{\text{mig}}}{\lambda})}{\exp(\frac{V^{\text{mig}}}{\lambda}) + \exp(\frac{V^{\text{no mig}}}{\lambda})}$$

This formulation delivers a continuous function and in the limit ($\lambda \to 0$) produces the discrete choice model. The model in equation (?) is now written as

$$V(s) = \min_{\tilde{q}} \quad pV^{\text{mig}}(s) + (1-p)V^{\text{no mig}}(s)$$

where choices $\tilde{q}$ include all of those in q except the migration choice $\mathbb{I}$.

We solve this model through a mixture of policy function iteration and value function iteration. All decisions $\tilde{q}$, conditional on p, are solved through policy function from the first order conditions, as described below. To solve for p, we also need the value functions, so we additionally iterate on the value function.

A.2 First order conditions for cost minimization problem

The FOC of the cost-minimization problem yield:

$$\frac{\partial}{\partial h_0}: \quad (1-p)\left(1-\frac{1}{R}\right) C_h(h_0,\mathbb{O}) = \alpha_0(1-\beta) \tag{1}$$

$$\frac{\partial}{\partial h_k}: \quad p\left(1-\frac{1}{R}\right) C_h(h_0,\mathbb{I}) = \alpha_k(1-\beta) \tag{2}$$

$$\frac{\partial}{\partial w_{j'0'}^{\text{no mig}}}: \quad (1-p)\frac{1}{R}\tau_{j'} V_{w_{j'0'}^{\text{no mig}}} = \mu_{j'0'}^{\text{no mig}} + (1-p'_{\text{mig}_k j'})(\gamma_{j'}^{\text{no mig}} + \beta\tau_{j'}\alpha_0) \tag{3}$$

$$\frac{\partial}{\partial w_{j'k'}^{\text{no mig}}}: \quad (1-p)\frac{1}{R}\tau_{j'} V_{w_{j'k'}^{\text{no mig}}} = \mu_{j'k'}^{\text{no mig}} + p'_{\text{no mig} j'}\pi_{k'}(\gamma_{j'}^{\text{no mig}} + \beta\tau_{j'}\alpha_0) \tag{4}$$

$$\frac{\partial}{\partial w_{j'0'}^{\text{mig}_k}}: \quad p\frac{1}{R}\tau_{j'} V_{w_{j'0'}^{\text{mig}_k}} = \mu_{j'0'}^{\text{mig}_k} + (1-p'_{\text{mig}_k j'})(\gamma_{j'}^{\text{mig}_k} + \beta\tau_{j'}\alpha_k) \tag{5}$$

$$\frac{\partial}{\partial w_{j'k'}^{\text{mig}_k}}: \quad p\frac{1}{R}\tau_{j'} V_{w_{j'k'}^{\text{mig}_k}} = \mu_{j'k'}^{\text{mig}_k} + p'_{\text{mig}_k j'}\pi_{k'}(\gamma_{j'}^{\text{mig}_k} + \beta\tau_{j'}\alpha_k) \tag{6}$$

$$\text{Envelope, no mig}: \quad V_{w_{j0}} = \alpha_0 \tag{7}$$

$$\text{Envelope, mig}_k: \quad V_{w_{jk}} = \alpha_k \tag{8}$$

where the probability of migrating in the next period, p', depends on the migration state in the current period and the village income state tomorrow.

B Rescaling income

Identifying the mean of log income is non-trivial because we are missing a portion of household income in the data (i.e. $E(Y_{it}^{data}) < E(C_{it}^{data})$. To account for this issue, we assume the income we observe ("data") is equal to a fraction k of the true income ("true"), so $E(Y_{it}^{data}) = kE(Y_{it}^{true})$. We also assume that we observe total consumption, so $E(Y_{it}^{data}) = kE(C_{it}^{data})$.

Rewriting income with the scaling factor k gives:

$$\begin{aligned}
Y_{it}^{data} &= kY_{it}^{true}\exp(x_{it}) \\
\log Y_{it}^{data} &= \log(k) + \log Y_{it}^{true} + x_{it} \\
\log Y_{it}^{true} &= a + u_{it}; u_{it} = \rho u_{i,t-1} + \eta_{it} \\
\log Y_{it}^{true} &\sim N\left(\frac{a}{1-\rho}, \frac{var(\eta)}{1-\rho^2}\right) \\
\log Y_{it}^{data} &\sim N\left(\log(k) + \frac{a}{1-\rho}, \frac{var(\eta)}{1-\rho^2} + var(x)\right)
\end{aligned}$$

Similarly, from consumption we know:

$$
\begin{aligned}
c_{it}^{data} &= c_{it}^{true} \exp(\epsilon_{it}^{c}) \\
\log c_{it}^{data} &= \log c_{it}^{true} + \epsilon_{it}^{c} \\
\log c_{it}^{data} &\sim N\left(E(\log c_{it}^{true}), var(\log c_{it}^{true}) + var(\epsilon^{c})\right)
\end{aligned}
$$

so $E(\log c_{it}^{data}) = E(\log Y_{it}^{true}) = \frac{a}{1-\rho}$, and $k = \log Y_{it}^{data} - \log Y_{it}^{true}$.

C Additional Tables and Figures