Migration and Informal Insurance *

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Abstract

Do new migration opportunities for rural households change the nature and extent of informal risk sharing? We experimentally document that randomly offering poor rural households subsidies to migrate leads to a 40% improvement in risk sharing in their villages. We explain this finding using a model of endogenous migration and risk sharing. When migration is risky, the network can facilitate migration by insuring that risk, which in turn crowds-in risk sharing when new migration opportunities arise. We estimate the model and find that welfare gains from migration subsidies are 42% larger, compared with the welfare gains without spillovers, once we account for the changes in risk sharing. Our analysis illustrates that (a) ignoring the spillover effects on the network gives an incomplete picture of the welfare effects of migration, and (b) informal risk sharing may be an essential determinant of the takeup of new income-generating technologies.

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1 Introduction

Poverty is highly concentrated in rural areas. Four out of every five poor households in Asia are rural (Asian Development Bank, 2007), and many of these households rely on risky, weather-dependent agricultural activities. Rural livelihoods are therefore both volatile across years, and fluctuate across seasons with the crop cycle. To address this uncertainty, agrarian households smooth consumption and manage risk using two primary mechanisms: they share risk with other members of their community (Munshi and Rosenzweig, 2016; Ferrara, 2003), and they migrate to diversify income sources (Banerjee and Duflo, 2007). However, each of these mechanisms is imperfect. Informal risk sharing provides only partial insurance (Townsend, 1994; Ligon et al., 2002; Kinnan, 2019), and migration may be costly or itself risky (Bazzi, 2017; Beam et al., 2016). The two approaches may also interact with one another. The presence of a safety net at home might either facilitate or deter risky migration. New migration opportunities might either undermine informal insurance schemes by providing exit options or, conversely, produce spillover benefits for other members of the network through transfers and sharing of the extra migration income. Therefore to understand the overall welfare effects of encouraging migration, it is necessary to consider the effects of migration on risk sharing.

In this paper, we study the interaction between migration and social safety nets by examining how new migration opportunities affect the nature and extent of informal risk sharing within those villages. We have three main objectives. The first is to empirically estimate the causal effect of migration subsidies on risk sharing. We take advantage of the randomized controlled trial (RCT) described in Bryan et al. (2014) in which households in rural Bangladesh were randomly offered subsidies to migrate temporarily during the agricultural lean season. While existing evaluations of the program consider only the effects on direct beneficiaries (Bryan et al., 2014; Lagakos et al., 2018), we analyze the spillover effects on the entire risk-sharing network. The second objective is to interpret and explain our empirical findings using a model of endogenous risk sharing and endogenous migration, allowing for the complex set of interactions between the two. We build upon the model in Morten (2019) and show that migration subsidies can interact with the underlying risk environment to generate either positive or negative spillovers; this is an important insight because it demonstrates that the welfare effects of policy are heavily context dependent. We thus characterize key features of the environment and of preferences that can alter how these policies interact. Our third objective is to estimate this model using the experimental variation, use these estimates to quantify the welfare effects of migration subsidies that we implemented, and consider counterfactual policies of permanent migration subsidies and unconditional cash transfers.

The Bangladesh experiment provided very poor agrarian households money for the round-trip bus fare (worth USD 8.50) conditional on one member migrating temporarily. Bryan et al. (2014) present the effects of the experiment on direct beneficiaries. The subsidy offer led to a 22 percentage point increase in migration in the first year, household consumption increased by 30% for those induced to migrate, and migration rates remained 11 percentage points higher during the next lean season, one year after the one-time subsidy was removed.[1] Our analysis goes beyond these direct benefits and con-

[1] Lagakos et al. (2018) examines potential unobserved disutility associated with migrating away from home in the same experiment, and again focuses only on direct benefits, but not spillovers to others in the network.

siders the indirect spillover effects on households in the village that did not send migrants themselves. We find clear experimental evidence that risk sharing improved in treatment villages. For both migrants and non-migrants, the correlation between consumption and own income declined by 40%, actual transfers between households increased, and households in treatment villages became more likely to report that they receive help from family and friends in the village. We also show that these results are not driven by an increase in self-insurance (savings) nor by an increase in measurement error. These changes in risk sharing imply that analyzing only the direct effects of the migration subsidies gives an incomplete picture of the overall welfare effects.

Next, in order to understand why subsidies for migration led to an improvement in risk sharing in this context, we develop a model in which both migration and risk sharing are endogenous and jointly determined. The model allows us to study the underlying economic mechanisms that link migration and risk sharing, which is important because our results are in stark contrast to Morten (2019), who found that in rural India, an increase in migration led to a crowding-out, rather than a crowding-in, of informal insurance. We reconcile these conflicting empirical results by showing that it is possible to get either crowd-in or crowd-out of risk sharing from the same theoretical model under different local conditions and preferences. Our model augments Morten (2019), which considered a model with limited commitment constraints on risk sharing (Kocherlakota, 1996; Ligon et al., 2002; Krueger and Perri, 2010) and an endogenous migration decision based on the net return to migrating (Harris and Todaro, 1970). We add the possibility that migrants develop contacts in the city, allowing them to better obtain jobs. This "asset" can lead to persistence in migration episodes. We model the migration subsidy in a flexible, agnostic way: alongside the USD 8.50 financial subsidy, we allow the experiment to change the utility value of migrating resulting from the encouragement offered by the experimental design and from the fact that increased migration allowed people to migrate with their friends.[2]

As mentioned above, the model can explain why a migration subsidy can induce either an improvement or a decline in risk sharing in different contexts. In many settings, migration is a risky lottery: a household gives up some income in the village for a chance at income in the destination. A migration subsidy increases the return to migrating, which may have two effects. On the one hand, increasing migration may increase the total resources available to the village, thus increasing the *social* return to the village of pooling income through risk sharing. On the other hand, the migration subsidy increases the *private* return to migrating, thus affecting the incentive to participating in risk sharing. For example, if it is very risky to migrate, the private return to migrating may be much lower than the social return because without insurance, migration is simply too risky to undertake. In contrast, if it is relatively safe to migrate, then the migrant may not need the safety net provided by the network, and a migration subsidy may lead to crowding-out of informal risk sharing. We show, by simulating the model for different values of migration risk and subsidy levels, that we can indeed easily generate both positive and negative spillovers of a migration subsidy on risk sharing.

While the RCT allows us to estimate the effect of the subsidies on risk sharing directly, the theoretical issues discussed above demonstrate that a theoretical framework is essen-

[2]A later round of experiments in the same villages found that people are more likely to migrate when others in the village are offered subsidies, even if they themselves were not subsidy recipients (Akram et al., 2018).

tial to understand the underlying reasons behind our empirical result of improved risk sharing and how it may change in other contexts. We thus use the results from the RCT to estimate a model that allows us to identify the underlying parameters. By combining our model with experimental variation, we achieve cleaner identification by not having to rely on observational data, as Morten (2019) did. The model itself provides a powerful toolkit for analyzing alternative policies, and the resulting combination adds to a nascent but growing literature that combines RCTs and structural models (see, for example, Attanasio et al. (2012); Todd and Wolpin (2006); Kaboski and Townsend (2011)). We estimate parameters that characterize income processes, migration costs, migration asset paths, and preferences to match experimental outcomes over three periods. Our model can replicate the dynamics and treatment effects of migration and risk sharing.

Using these estimates, we quantify the overall welfare effect of the migration subsidies and conduct counterfactual experiments to evaluate different policy levers. We estimate that the experiment led to an increase in welfare equivalent to a 7.2% permanent increase in consumption the year the experimental subsidies were disbursed, not counting the subsidy itself. While some of this gain is due to increased resources from migration, the welfare gain is 42% higher after accounting for the improvement in risk sharing.

Our analysis has two important implications for understanding policy impacts and the development process. First, it is crucial to account for spillover effects – which may be negative or positive – to understand the full welfare effect of any policy that addresses the income stream of households. This finding resonates with the growing literature estimating spillover effects from financial inclusion initiatives, which can also have either positive or negative interactions with risk-sharing (see, e.g., Angelucci and de Giorgi

(2009) and Dupas et al. (2017)). Second, the fact that migration and risk sharing are jointly determined implies that informal insurance may be an important factor determining if, and when, households adopt new income-generating methods. In some cases, because of obligations to transfer some of the returns, informal insurance may reduce the take-up of new methods to generate income. In others, by providing a safety net in case of failure, informal insurance may increase take-up.

The paper continues by describing the data and experimental results on risk sharing in Sections 2 and 3. We then move to the model of endogenous risk sharing and migration in Section 4, and show, by simulating the model, the key comparative statics driving the mechanisms in the model. We describe our estimation procedure in Section 5 and then consider model counterfactuals in Section 6. We conclude by discussing the broader implications of our findings for understanding the interaction between informal insurance and technology adoption.

2 Data

The experiment randomly offered some households subsidies to temporarily emigrate from villages in a poor region of northern Bangladesh. This region is prone to a period of preharvest seasonal deprivation during September to December known as *Monga*, which was the original motivation for the experimental intervention.[3] Bryan et al. (2014) provides additional details on the experiment and reports on the program evaluation, focus-

[3] Income and consumption levels drop by roughly 50% and 25%, respectively during this period (Khandker, 2012). Up to 60% of our sample respondents report missing meals during this period. This same phenomenon, colloquially known as the "hunger season," is prevalent in many poor agrarian societies around the world.

ing on the direct beneficiaries. In this section, we describe the experiment and the data that is critical for understanding our analysis of the risk-sharing effects of this experiment.

The migration subsidy treatment was randomized at the village level. In each treatment village, 19 households were randomly selected to receive treatment.[4] The migration subsidy was an offer of 600 Taka (USD 8.50) *conditional* on one person from the household migrating, with an additional 200 Taka given if the migrant reported to our enumerators in the destination. This amount is sufficient to cover the cost of a return bus ticket and a few days food in the destination. The subsidy was offered in the form of a grant in 37 villages or a zero-interest loan in 31 additional villages. We follow Bryan et al. (2014) and combine these two into a 68-village "incentive" arm, and compare that against a 32-village "control arm" (composed of 16 villages where we provided general information about migration opportunities and 16 others where we did nothing).[5]

The subsidies were distributed in August–September 2008. We will make use of three periods of data: (a) a pre-intervention survey conducted in July 2008, (b) effects during the intervention year, collected in 2008–2009, and (c) longer-run post-intervention data collected in 2011, approximately two-and-a-half years after the experiment. We rely heavily on 2011 data for our analysis of risk sharing because it is the only round to contain annual data on income, consumption, and migration.[6]

[4] Households were eligible for the experiment if the household 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. In total, 56% of households satisfied both criteria. Households were randomly selected from this group.

[5] A subset of households in the incentive arm were assigned additional conditionalities such as migrating with assigned partners or migrating to a specific destination. These will only play a small role in our analysis in that they will help us rule out "hidden income" as an impediment to risk sharing in this context.

[6] Appendix Table 1 summarizes the data collection timeline, including the modules collected in each survey round. In 2011, the sample was expanded from the original 100 villages to include 33 new villages, adding 247 additional households to the data. In 2011, additional experiments were also run in the sample. Our primary analysis focuses on analyzing the longer-run effects of the original (2008) experiments. We discuss robustness tests addressing the additional (2011) experiments in Section 3.

Table 1 shows summary statistics for the sample of households in 2011 that we use for estimation, which includes the 1900 households from the original experiment plus 247 new households from 33 randomly selected new villages in the same two districts. Statistics are shown separately for all households (column 1), households in control villages (column 2), and households in incentive villages (column 3).[7] Our main measures of interest are income, consumption, and migration rates, but we also show summary statistics of other measures of consumption smoothing. For income and consumption, we exclude outliers and winsorize the top and bottom percentiles.

Income We use three measures of income in our analysis: home income, city income, and total income. Home income comprises income earned by the household in the rural village and consists of wage income, non-farm business income, agricultural income (crops and non-crops), and miscellaneous income such as lottery winnings and interest income. We do not include transfers from other members of the community in income. City income is defined as earnings during migration episodes, net of travel costs to and from the migration destination.[8] Total income is the sum of home income and city income (which is equivalent to home income for non-migrant households). Each of these measures span the previous 12 months and are thus annual measures of income. Following Bryan et al. (2014), we convert these measures to per capita amounts by dividing by the household size, defined as all individuals reported to be living in the house for at least seven days at the time of the interview (which may or may not include migrants,

[7] For the original balance tables of the sample in 2008, see Bryan et al. (2014).
[8] The survey elicits travel costs as "all costs related to travel such as bus/train, rickshaw, etc."

Table 1: Summary statistics (post-intervention)

mean/sd	All villages	Control villages	Treatment villages
Total income	9627	9360	9886
	(4829)	(4702)	(4938)
Home income	8947	8770	9119
	(4866)	(4721)	(4999)
City income (among migrant households)	1649	1519	1758
	(1113)	(1036)	(1163)
Total consumption	19177	18883	19460
	(5880)	(6038)	(5713)
Any savings in last 12 months	0.47	0.43	0.51
	(0.50)	(0.49)	(0.50)
Amount, among those who saved	369	363	373
	(568)	(619)	(523)
Any transfers received from community	0.57	0.57	0.56
	(0.50)	(0.49)	(0.50)
Amount, if any transfers received	5641	4808	6470
	(11912)	(7707)	(14925)
Any transfers given to community	0.18	0.15	0.20
	(0.38)	(0.36)	(0.40)
Amount, if any transfers given	2775	2001	3368
	(6671)	(3858)	(8157)
Household size	4.05	4.04	4.06
	(1.43)	(1.38)	(1.48)
% Migrant households	0.41	0.39	0.44
	(0.49)	(0.49)	(0.50)
Number of households	1928	946	982

Note: Income, consumption, savings, and transfers are in Taka (approximately 75 Taka per USD in 2011). Income and consumption are annual and per capita.

depending on when they returned home).[9] City income is around 20% of home income, and households in treatment villages have slightly higher home income and city income than households in control villages.

Consumption Our measure of consumption closely follows Bryan et al. (2014) and consists of 215 food items and 63 non-food items. Some of these items have a weekly recall and others have a bi-weekly or monthly recall, so we convert these items to annual and per capita amounts in order to be consistent with our measure of income. Like income, consumption for households in treatment villages is higher than consumption for households in control villages.[10]

Other summary statistics Households in our sample typically contain four members, and around 40% of households sent a migrant over the course of the year. Some 47% of households saved some amount during the year, but the amounts were very small (under 400 Taka). In contrast, many households gave or received transfers from other households in the community: 57% received transfers, averaging 5,600 Taka, and 18% gave transfers, averaging 2,800 Taka.

[9] In the next section we show that our main treatment effects on consumption smoothing are robust to alternative household size definitions, including the current number of household members at the time of the interview and the number of household members who are living in the house for at least 14 days at the time of the interview.

[10] Total consumption is higher than total income, as is often found in rural household survey data collected in agrarian areas. The ratio of income to consumption, however, does not vary significantly between treatment and control households.

3 Effect of experiment on risk sharing

As Table 1 shows, households often engage in direct transfers between each other, presumably as a way to protect themselves against bad income realizations. In this section, we empirically investigate whether offering migration subsidies affects the functioning of risk-sharing networks. We conduct direct tests of the effect of the experiment on several measures of transfers and risk sharing.

3.1 Effect on financial transfers

We start by showing that actual transfers, as well as the household's self-reported willingness to ask for help, were affected by the migration subsidies. Table 2 regresses these measures on treatment to test whether the experiment changed these beliefs in the financial arrangements between villagers ("Willingness to help") and the transfers that occurred ("Actual transfers").

Each row in the first column is a separate regression of the effect of treatment on each outcome between community members (i.e., family, friends, and other villagers). The second column contains the mean of the variable among households in the control group. The top panel suggests that the experiment significantly increased the willingness of households to interact financially. For example, 57% of households in control villages report that community members would ask them for help, and treatment increases that by 11 percentage points. Not only are such intentions affected in villages where migration subsidies were offered, but actual amounts of transfers also increased as a result of treatment. While there is no change detected in the probability of receiving a transfer, treatment increased the value of transfers received (among those who did receive) by 1,821 Taka off a base of 4,808 Taka, or 38%. The results for transfers given are even stronger: a 3.6 percentage point increase in the propensity to give, and a 1,310 Taka (65%) increase in the amount given, conditional on giving. Since we collect data on a set of people within the village who recently received external migration subsidies, it is perhaps sensible that the results on "transfers given" are larger than those on "transfers received."

Table 2: Treatment effect on transfers within the community

	Treatment effect	Control mean
Willingness to help		
Community member would help you	0.030	0.85
	(0.020)	
... and you would ask for help	0.025	0.83
	(0.020)	
Community member would ask you for help	0.109***	0.57
	(0.033)	
... and you would help them	0.109***	0.53
	(0.032)	
Actual transfers		
Receive any transfer from community member	-0.024	0.57
	(0.022)	
Amount, if any transfer received (Tk)	1821***	4808
	(678)	
Give any transfer to community member	0.036**	0.15
	(0.018)	
Amount, if any transfer given (Tk)	1310**	2001
	(558)	

Note: The sample includes households from the 2011 survey. Each cell is a separate regression of the effect of treatment on whether the source denoted in the row would behave as described. Each regression also controls for upazila (county). Standard errors, clustered by village, are in parentheses, and the mean of the control group is in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Perhaps the act of migration leads to migrants bringing back gifts for friends in the village, which could be why we observe the increase in "transfers given," but this does not signal a broader improvement in risk sharing. In Appendix Table 2 we repeat the analysis of transfers separately for households that sent a migrant in the past year and those that did not.[11] The results show similar effects for both the migrant and non-migrant samples. Migration not only increased the willingness to share risk among particular households that were induced by the experiment to send a migrant but strengthened informal relationships within a village more broadly.

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 summary statistics of Table 1. Second, the point estimates show that the migration experiment significantly increased the willingness of households to participate in these arrangements as well as actual transfers between households. Third, this increase is not limited to households that were induced to migrate: non-migrant households in treatment villages also reported an increase in the ability to use these informal arrangements.

3.2 Effects on the correlation between income and consumption

Since it is difficult for survey data to enumerate the full range of relevant gifts, transfers, and loans, we now investigate the effect of the experiment on the extent to which income relates to consumption, implementing tests of risk sharing that date back to Townsend (1994). We test two key ideas. First, we explore the correlation between income and consumption to identify the extent of informal insurance within the village. Second, we investigate whether the experiment changed this correlation.

To implement these tests, we regress log of per capita household consumption on log

[11] Although this sample split is endogenous to the decision to migrate, we argue that it provides suggestive evidence of risk-sharing benefits spilling over to households in the village that did not receive the direct migration incentives provided by the experiment.

of per capita household income and estimate the following regression:

$$\log C_{iv} = \gamma_v + \alpha \log Y_{iv} + \epsilon_{iv} \tag{1}$$

where $\log C_{iv}$ and $\log Y_{iv}$ are household i's log per-capita consumption and income, respectively, in village v. Village fixed effects γ_v capture the effects of aggregate shocks on consumption. The main parameter of interest is α, which captures the correlation between income and consumption, conditional on aggregate fluctuations.

Table 3: Consumption smoothing among control villages

	Log consumption		
	(1)	(2)	(3)
Log total income	0.162***		
	(0.022)		
Log home income		0.122***	
		(0.019)	
Log city income			0.107***
			(0.036)
Sample	Full	Full	Migrants
Observations	909	946	351
R^2	0.216	0.194	0.301

Note: The sample includes households in control villages in the 2011 survey. The dependent variable is log of annual per-capita total consumption and the independent variable is log of annual per-capita total income. Each model also includes village fixed effects. Standard errors, clustered by village, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3 reports the results of Equation (1) using total income (column 1), home income (column 2), and city income among migrant households (column 3) in the sample of control villages. In all cases, income has a significant impact on consumption, confirming the absence of full insurance: a 10% increase in total household income corresponds to a 1.6% increase in total household consumption, controlling for aggregate village income via village fixed effects.

The next set of regressions leverage the experimental variation in the data to test whether a one-time exogenous decrease in migration costs via the experiment led to a change in village insurance in a subsequent year. We augment Equation (1) to allow the transmission parameter α to vary by whether the village is in the treatment sample:

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

where T_v is an indicator variable taking a value of 1 if the village is a treatment village.[12] 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.

Table 4 reports the transmission parameter (α_1) for log total income on log total consumption. The first row shows that for the overall sample, the migration treatment significantly improved risk-sharing opportunities. Specifically, treatment reduced the effect of household income on household consumption by over seven percentage points.[13] Compared to a baseline exposure of consumption to income shock of 16% (shown in Table 3), the migration treatment cuts this exposure by around 40%, implying a substantial increase in village insurance. These estimates are particularly large given that they are intent-to-

[12] The treatment indicator captures villages randomized to receive migration subsidies in 2008. As described in Footnote 6, additional experiments were implemented in 2011. We control for the additional round of treatments through the village fixed effects and additional interaction terms between log income and 2011 treatment arms. The 2011 treatments were implemented outside the *Monga* period (around April) and did not have any effects on risk sharing. We also run the risk-sharing regressions using only households that did not receive any additional 2011 experiments (i.e., were control villages). We find similar results on risk sharing. These results are in Appendix Table 3.

[13] We test the sensitivity of this result to our definition of household size in Appendix Table 4 and find similar estimates.

Table 4: Effect of migration incentives on consumption smoothing

	Log total consumption		
	(1)	(2)	(3)
Log income x treatment	-0.073***		
	(0.028)		
Treatment group restrictions			
Log income x unassigned group		-0.084**	
		(0.036)	
Log income x self-formed group		-0.022	
		(0.034)	
Log income x assigned group		-0.106***	
		(0.037)	
Treatment destination restrictions			
Log income x unassigned destination			-0.067**
			(0.031)
Log income x assigned destination			-0.082**
			(0.033)
Observations	1857	1857	1857
R-squared	0.185	0.187	0.186

Note: The sample includes households from the 2011 survey. The dependent variable is log annual per-capita total consumption. The main independent variable is log annual per-capita income, interacted with the respective treatment variable. All models control for village fixed effects and log income interacted with 2011 treatment arms. Standard errors, clustered by village, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

treat estimates.[14] Appendix Table 5 shows similar effects using a difference-in-difference specification that includes pre-intervention data collected in July 2008.[15]

Beyond limited commitment, the literature identifies hidden income as another friction that can undermine households' ability to share risk effectively (Townsend, 1982; Rogerson, 1985; Kinnan, 2019). If migration makes it easier to hide income because some of the income is earned away from other villagers' watchful eyes, risk sharing can break down. To investigate the relevance of hidden income in our setting, we take advantage of sub-treatment variations in the experiment, where some of those receiving migration subsidy offers were additionally required to migrate in groups. Those group members were either assigned by the experimenters in one sub-treatment or self-formed by the migrants in another. Columns (2) and (3) in Table 4 show that the treatment effect on the correlation between own-income and own-consumption continues to be negative, even when migrants are required to travel in groups or to particular destinations. There is no statistically significant change in the treatment effect when one of these requirements is imposed. This result suggests that hidden income may not be a key constraint limiting risk sharing in this setting. Our model presented in the next Section, therefore, focuses on limited commitment (as opposed to hidden income) as the primary friction undermining risk sharing.

One concern with the results in Table 4 is that our ability to measure income for treatment households may be less accurate than for control households, either because mi-

[14]We report ITT rather than LATE (IV) estimates because treating one household may affect a neighbor's consumption through changes in demand for transfers – even absent sharing any extra migration income – so the exclusion restriction for the IV would be violated.

[15]The difference-in-difference specification with household fixed effects produces a similar point estimate, although with only two observations per household the statistical significance disappears.

gration income is inherently more difficult for the econometrician to capture, or again 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 may create an attenuation bias in treatment effect estimates. To investigate this, we repeat the analysis from Table 4 among households that did *not* send a migrant. We present results in Appendix Table 3. While this is an endogenously selected sample, our ability to measure their income should not vary by treatment, and 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 observe in Table 4. The experiment changed the risk-sharing equilibrium between all households in the network, not only the subset who were induced to migrate.

A second concern is that the coefficient α could mechanically decrease if the variance of income increased as a result of the experiment. While the result mentioned above showing that risk sharing improved among non-migrants should alleviate this concern, a more structural measure of risk sharing is the difference in the consumption equivalents between autarky and participating in the network. We consider this alternative measure later in the paper and show that it is consistent with our measure of α.

3.3 Effects on savings

A change in the correlation between income and consumption does not necessarily imply a change in risk sharing across households if the migration subsidy offers also increased the household's ability to save. These households could be using savings (as opposed to migration and sharing risk with other households) as an alternative consumption-smoothing mechanism (although this would not explain why we see an improvement in risk sharing even among non-migrant households). In Table 5, we test the effect of the experiment on the amount saved over the past 12 months. Columns (1) and (2) show that there is no significant effect of treatment on the amount saved, both among the full sample of households (column 1) and the sample of those that reported saving a non-zero amount (column 2).

Table 5: Treatment effect on savings

	Amount saved in last 12 months (Taka)	
	(1) Unconditional	(2) Conditional on any
Treatment	33.4	5.4
	(21.0)	(37.4)
Mean amount saved	172.2	369.0
Observations	2359	1101
R^2	0.01	0.02

Note: The sample includes households from the 2011 survey. The dependent variable is the amount saved (in Taka) in the previous year, including zeros (column 1) and excluding zeros (column 2). Both models control for upazila (county) and 2011 treatment arms. Standard errors, clustered by village, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Not only are the coefficients statistically indistinguishable from zero, but the coefficient estimates on savings are also very small. This is consistent with the fact that these are extremely poor people, and the marginal propensity to consume any extra migration income during this lean (hungry) season is very high. These null savings results, coupled with substantial reductions in the income-consumption correlation and significant increases in financial transfers, all point to the experiment causing a substantial improvement in the willingness and ability to share risk in treatment villages. To understand why this happened, we next turn to a model of endogenous migration and risk sharing.

4 Joint model of risk sharing and migration

We consider a joint model of risk sharing and migration based on Morten (2019). Households make migration decisions taking into account the returns to migrating, including risk-sharing transfers. Risk sharing is constrained by limited commitment frictions (Kocherlakota, 1996; Ligon et al., 2002; Krueger and Perri, 2010). We extend the framework in Morten (2019) to allow for a migration asset, which we think of as a job connection in the destination. This asset can be accumulated (or lost) over time, based on the experiences of the migrant in the destination. This extension will allow the model to approximate the fact that migrants tend to return to the same employer – for example, Bryan et al. (2014) find that 60% of incentivized migrants return to work for the same employer – which in turn allows the model to explain why the one-time experiment led to persistent effects on migration. In this setting, therefore, migration serves two purposes. First, within a period, it potentially increases the income available to the household. Second, because it allows individuals to update their migration assets, it provides a dynamic payoff for the future.

We model the risk-sharing game as between two households, denoted by $i = \{1, 2\}$, with identical preferences. We assume that households cannot save, which is consistent with the empirical finding in Section 3 that savings are very low and did not respond to the experiment. The game has two sub-periods: a "before-migration" period, when the village income state is realized and migration decisions are made, and an "after-migration" period, when migration outcomes are realized and transfers and consumption occur. We denote "after-migration" variables with a "hat" accent. The timing of the model is as follows. At the start of period t both households are in the village. Each household receives a village income, $e^i(s_t)$, where s_t denotes the state in the village and is finite.

Each household i, based on its past migration, either has or does not have an active job connection in the city at the start of the period. This job connection is denoted by $a_t^i = \{0, 1\}$. A household that sends a migrant without a job contact may find one on arrival with probability $\pi^{\text{get contact}}$. The within-period updating rule for the job contact asset is therefore given by (where $\mathbb{I}$ indicates migrating):

$$Pr(\widehat{a}_t^i = 1) = \begin{cases} 1 & \text{if } a_t^i = 1 \\ \pi^{\text{get contact}} & \text{if } a_t^i = 0 \text{ and } \mathbb{I}_t = 1 \\ 0 & \text{if } a_t^i = 0 \text{ and } \mathbb{I}_t = 0 \end{cases} \tag{3}$$

Each household then decides whether to migrate based on the expected return to migrating. Migration income is uncertain and is not observed until after the migration decision is made. Net migration income depends on whether or not the migrant has a job contact; the state of the world in the destination, q_t, which is finite; and any net financial migration cost (which may be negative if migration is subsidized) paid to migrate, d_t^{fin}. Household income depends therefore on the realization of the state of the world in the village, the migration decision, the realization of the state of the world in the destination, the realization of the migration asset, and the net financial migration cost:

$\hat{y}^i(s_t, q_t, \mathbb{I}_t^i, \widehat{a_t}^i, d_t^{\text{fin}})$.[16] In addition to financial concerns, migration also involves a utility cost d_t^{utility}.

Each household either sends a migrant or does not send a migrant; we summarize the migration outcome by $j_t = \{\mathbb{I}_t^1, \mathbb{I}_t^2\}$, which takes four possible values. We denote the before-migration job contact assets of each household by the vector $A_t = \{a_t^1, a_t^2\}$, and the after-migration job contact assets by the vector $\widehat{A}_t = \{\widehat{a}_t^1, \widehat{a}_t^2\}$. Depending on the interaction capabilities between households, after all migration decisions are made and all income is realized, household one makes a transfer (which may be negative) to household two, $\tau_t(s_t, j_t, q_t, \widehat{A}_t)$, and then consumption occurs. At the end of the period, all migrants return home. With some probability, which depends on whether the household migrated that period, the migration contact is lost, and the household starts the following period with a migration asset given by:

$$Pr(a_{t+1}^i = 1) = \begin{cases} 1 - \pi^{\text{lose contact,mig}} & \text{if } \hat{a}_t^i = 1 \text{ and } \mathbb{I}_t^i = 1 \\ 1 - \pi^{\text{lose contact,no mig}} & \text{if } \hat{a}_t^i = 1 \text{ and } \mathbb{I}_t^i = 0 \\ 0 & \text{if } \hat{a}_t^i = 0 \end{cases} \tag{4}$$

The game then repeats itself in the following period. The history up to the end of period $t-1$ (of the village state, the migration state, the migration decision, and the migration assets) is expressed by the vector $h^{t-1} = (s_0, q_0, j_0, \widehat{A}_0, s_1, q_1, j_1, \widehat{A}_1 ..., s_{t-1}, q_{t-1}, j_{t-1}, \widehat{A}_{t-1})$.

To determine the risk-sharing capabilities between households, it is useful to first describe the optimization problem if each household is independent (i.e., not part of a risk-sharing arrangement). Households solve maximization problems at two points in time

[16] This general formulation allows for the case in which migrant households also receive some income from the village (for example, if other household members still work).

that result in the "before-migration" value, $\Omega_t^i(s_t, a_t^i)$, and the "after-migration" value, $\widehat{\Omega_t^i}(s_t, q_t, \mathbb{I}_t^i, \hat{a}_t^i)$. The "before-migration" value is the expected utility at the time the household is deciding whether or not to migrate:

$$\Omega^i(s_t, a_t^i) = \max_{\mathbb{I}_t=\{0,1\}} \sum_q \sum_{\hat{a}} \pi_q \pi_{\hat{a}|a,\mathbb{I}} \left[u(\hat{y}^i(s_t, q_t, \mathbb{I}_t^i, \hat{a}_t^i, d_t^{\text{fin}})) - \mathbb{I}_t d_t^{\text{utility}} + \beta \sum_{s'} \sum_{a'} \pi_{s'|s} \pi_{a'|\hat{a},\mathbb{I}} \Omega(s_{t+1}, a_{t+1}^i) \right] \quad (5)$$

The "after-migration" value is the expected utility once the migration decision has been made and the household learns if it has a job contact, and then learns the state of the world in the destination:

$$\widehat{\Omega}^i(s_t, q_t, \mathbb{I}_t^i, \hat{a}_t^i) = u(\hat{y}(s_t, q_t, \mathbb{I}_t^i, \hat{a}_t^i, d_t^{\text{fin}})) - \mathbb{I}_t^i d_t^{\text{utility}} + \beta \sum_{s'} \sum_{a'} \pi_{s'|s} \pi_{a'|\hat{a},\mathbb{I}} \Omega(s_{t+1}, a_{t+1}^i) \quad (6)$$

Equations 5 and 6 are important objects for determining the value of migration subsidies in an environment without spillovers as well as for determining the credible threat points in the full endogenous risk-sharing model.

We can now describe the full model, in which both risk sharing and migration are endogenously determined. The optimization problem involves migration choices of both households and the net transfer from household one to household two, τ, to maximize total welfare. This problem is constrained by two sets of incentive compatibility constraints (one for each household at the "before-migration" stage, and one for each household at the "after-migration" stage), as well as a promise-keeping constraint that household one receives the utility promised to them. We follow the solution concept proposed in Ligon et al. (2002) by solving for the conditional Pareto frontier that maximizes the utility of household two given a promised level of utility to household one. Because households make choices at two points in time during a period, we define two Pareto frontiers: first, the frontier that maximizes the "before-migration" utility of household two, V_{sA}, given a state-dependent level of "before-migration" promised utility, U_{sA}, to household one, where s is the state of the world in the village, and A is before-migration assets. Second, the "after-migration" frontier maximizes the "after-migration" utility of household two, $\widehat{V}_{sjq\hat{A}}$, conditional on s, the migration decision j, the migration outcome q, the after-period migration asset $\hat{A}$, and the promised utility to household one $\widehat{U}_{sjq\hat{A}}$.

We solve the model in two steps. Starting in the second sub-period, we solve for optimal transfers and continuation utility:

$$\widehat{V}_{sjq\hat{A}}(\widehat{U}_{sjq\hat{A}}) = \max_{\left\{\tau_{sjq\hat{A}}, \{U_{s',A'}\}_{s'=1..,S,A'=1,..,4}\right\}} u\left(\hat{y}^2_{sjq\hat{A}} + \tau_{sAjq\hat{A}}\right) - \mathbb{I}^2_j d^{\text{utility}} + \beta \sum_{s'} \sum_{A'} \pi_{s'|s} \pi_{A'|\hat{A},j} \left[V_{s'A'}(U_{s'A'})\right] \quad (7)$$

subject to a promise-keeping constraint:

$$(\widehat{\lambda}_{sjq\hat{A}}): \quad u(\hat{y}^1_{sqj\hat{A}} - \tau_{sqj\hat{A}}) - \mathbb{I}^1_j d^{\text{utility}} + \beta \sum_{s'} \sum_{A'} \pi_{s'|s} \pi_{A'|A,j} \left[U_{s'A'}\right] \geq \widehat{U}_{sqj\hat{A}}$$

and incentive compatibility constraints for the before-migration problem in the following period for both households:

$$(\beta \pi_{s'|s} \pi_{A'|\hat{A},j} \phi^1_{sqjs'A'}): \quad U_{s'A'} \geq \Omega^1_{s'A'} \quad \forall s', A'$$

$$(\beta \pi_{s'|s} \pi_{A'|\hat{A},j} \phi^2_{sqjs'A'}): \quad V_{s'A'}(U_{s'A'}) \geq \Omega^2_{s'A'} \quad \forall s', A'$$

The first order conditions (after rescaling the multiplier for household one by its initial Pareto weight, $\hat{\lambda}$) yield:

$$\frac{u'(c^2_{sqj\hat{A}})}{u'(c^1_{sqj\hat{A}})} = \hat{\lambda}_{sqj\hat{A}}$$

$$V'_{s'A'}(U_{s'A'}) = -\hat{\lambda}_{sqj\hat{A}} \frac{(1+\phi^1_{sqj\hat{A}s'A'})}{(1+\phi^2_{sqj\hat{A}s'A'})} \quad \forall s', A'$$

$$\hat{V}'_{sqj\hat{A}}(\hat{U}_{sqj\hat{A}}) = -\hat{\lambda}_{sqj\hat{A}}$$

Then, given this optimized Pareto frontier, the planner solves for the optimal utility value, conditional on migration j, $\widehat{U}_{sqj\hat{A}}$, and then solves for the optimal migration rule j:

$$V_{sA}(U_{sA}) = \max_j \left\{ \max_{\{\widehat{U}_{sqj\hat{A}}\}} \left[\sum_q \sum_{\hat{A}} \pi_q \pi_{\hat{A}|A,j} \widehat{V}_{sqj\hat{A}}(\widehat{U}_{sqj\hat{A}}) \right] \right\}$$

subject to satisfying a promise-keeping constraint:

$$(\lambda_j): \quad \sum_q \sum_{\hat{A}} \pi_q \pi_{\hat{A}|A,j} \widehat{U}_{sqj\hat{A}} \geq U_{sA} \quad \forall j$$

and incentive compatibility constraints for the after-migration problem in the second sub-period for both households:

$$(\pi_q \pi_{\hat{A}|A,j} \alpha^1_{sqj\hat{A}}): \quad \widehat{U}_{sqj\hat{A}} \geq \widehat{\Omega}^1_{sqj\hat{A}} \quad \forall q, j, \hat{A}$$

$$(\pi_q \pi_{\hat{A}|A,j} \alpha^2_{sqj\hat{A}}): \quad \widehat{V}(\widehat{U}_{sqj\hat{A}}) \geq \widehat{\Omega}^2_{sqj\hat{A}} \quad \forall q, j, \hat{A}$$

The first order conditions (after rescaling the multiplier for household one by its initial

Pareto weight, λ) yield:

$$\hat{V}'_{sqj\hat{A}}(\hat{U}_{sqj\hat{A}}) = -\lambda_j \frac{(1+\alpha^1_{sqj\hat{A}})}{(1+\alpha^2_{sqj\hat{A}})} \quad \forall q, j, \hat{A}$$

$$V'_{sA}(U_{sA}) = -\lambda_j \quad \forall j$$

Combining the first order conditions from the before-migration problem and the after-migration problem therefore yields a simple updating rule for the before-migration Pareto frontiers. Given a migration decision j^*, an update $\hat{A}^*$ to the migration asset, and migration state q^*, the slope of the Pareto frontier is given by:

$$V'_{s'A'}(U_{s'A'}) = V'_{sA}(U_{sA}) \left(\frac{1+\alpha^1_{sq^*j^*\hat{A}*}}{1+\alpha^2_{sq^*j^*\hat{A}*}} \right) \left(\frac{1+\phi^1_{sq^*j^*\hat{A}*s'A'}}{1+\phi^2_{sq^*j^*\hat{A}*s'A'}} \right) \quad \forall s', A'$$

This means that the Pareto weight follows a simple updating rule, as in Ligon et al. (2002) and Morten (2019). Given an initial before-migration Pareto weight $\lambda^i_t(s_t, A_t, h^{t-1})$, the after-migration Pareto weight is given by:

$$\widehat{\lambda}^i_t(s_t, q_t, j_t, \widehat{A}_t, h^{t-1}) = \begin{cases} \underline{\widehat{\lambda}}^i_{sqj\widehat{A}}, & \text{if } \lambda_t(s_t, A_t, h^{t-1})^i \leq \underline{\widehat{\lambda}}^i_{sqj\widehat{A}} \\ \lambda^i_t(s_t, A_t, h^{t-1}) & \text{if } \lambda_t(s_t, A_t, h^{t-1})^i \in [\underline{\widehat{\lambda}}^i_{sqj\widehat{A}}, \overline{\widehat{\lambda}}^i_{sqj\widehat{A}}] \\ \overline{\widehat{\lambda}}^i_{sqj\widehat{A}} & \text{if } \lambda_t(s_t, A_t, h^{t-1})^i \geq \overline{\widehat{\lambda}}^i_{sqj\widehat{A}} \end{cases}$$

And the before-migration Pareto weight the following period is given by:

$$\lambda_{t+1}^{i}(s_{t+1}, A_{t+1}, h^{t}) = \begin{cases} \underline{\lambda}_{s'A'}^{i}, & \text{if } \widehat{\lambda}_{t}^{i}(s_t, q_t, j_t, \widehat{A}_t, h^{t-1}) \leq \underline{\lambda}_{s'A'}^{i} \\ \widehat{\lambda}_{t}^{i}(s_t, q_t, j_t, \widehat{A}_t, h^{t-1}), & \text{if } \widehat{\lambda}_{t}^{i}(s_t, q_t, j_t, \widehat{A}_t, h^{t-1}) \in [\underline{\lambda}_{s'A'}^{i}, \overline{\lambda}_{s'A'}^{i}] \\ \overline{\lambda}_{s'A'}^{i} & \text{if } \widehat{\lambda}_{t}^{i}(s_t, q_t, j_t, \widehat{A}_t, h^{t-1}) \geq \overline{\lambda}_{s'A'}^{i} \end{cases}$$

While the model presented above is for two households, the model easily extends from two people to N people. We show the extension to the N-person case in Appendix A.1. As the appendix shows, the N-person case yields one additional first order condition that the rate of growth of marginal utility for unconstrained households is equal. This additional equation, together with the budget constraint that total income equals total consumption, allows us to estimate the level of consumption. To do this, we need to estimate a scaling factor, ζ, where ζ is such that $\hat{\lambda}_t^i = \max(\zeta_t \hat{\lambda}_{t-1}^i, \underline{\hat{\lambda}})$ so that total consumption is equal to total income.

We solve for the equilibrium policy functions, taking into account the ex-ante and ex-post incentive constraints for all households and the budget account for the economy (which includes income earned from the endogenous migration decision), and solving the full transition path of the economy after the implementation of the temporary subsidy (which is introduced in the next subsection).[17] We describe the computational algorithm in Appendix A.2. Solving the model is computationally intensive; we implement the estimation on a high-performance computer cluster.

[17] To ensure that first order conditions hold, we smooth our objective function so that all migration states occur with positive probability.

4.1 Introducing the experiment into the model

We now introduce the experiment into the model as a one-time unanticipated subsidy to the financial cost of migrating, d_t^{fin}. We also allow the experiment to change the utility cost of migrating, $\Delta d_t^{\text{utility}}$, for example, by either providing an endorsement effect or, through an increase in migration, by generating a utility benefit of migrating with friends.[18] In estimation, we fix the financial cost but are agnostic about whether the experiment led to an additional utility benefit and thus estimate this parameter based on the observed treatment effects.

4.1.1 Comparative statics

Deriving comparative statics in a limited commitment framework is challenging. In this section, we discuss the intuition about the key forces driving migration and risk-sharing outcomes – including the role of migration subsidies – and verify this intuition through model simulations. We show that migration subsidies can either crowd in risk sharing or crowd out risk sharing, depending on the underlying parameters and the nature of the subsidies.

The underlying intuition is that risky migration is effectively a lottery: by paying a migration cost, the migrant gets a new income draw from a support that may include both very low incomes as well as very high ones. Migration leads to a tradeoff for risk sharing. On the one hand, migration introduces an additional source of income that – in expectation – increases the total resources available to the network and thereby increases

[18] Consistent with a utility effect, later experiments conducted in the same region found that un-incentivized households were more likely to migrate when more of their co-villagers were offered subsidies to migrate (Akram et al., 2018).

the gains from sharing risk. On the other hand, migration may change the private ability to smooth risk and thus change the incentive to share risk.

The smoothing value of migration for the individual is reflected in the certainty equivalence of the migration lottery, which, given a level of risk aversion, reflects both the riskiness and the overall resources it can offer. Risk sharing will tend to improve when a migration subsidy increases expected income relative to its certainty equivalent value (for example, if migration is particularly risky). Risk sharing will tend to worsen when a migration subsidy increases the certainty equivalent value relative to its expected income (for example, if it is easy for migrants to find a high-paying job). We provide support for this intuition by simulating the model and plotting migration responses, welfare (measured in consumption-equivalent units), and risk sharing coefficients. In the simulations below, we assume a uniform distribution of income in the village, characterized by mean $\mu = 3$ and equal support in $[1.8, 4.2]$, and a normal distribution of income in the destination, also with mean $\mu = 3$ but different variances.[19]

Figure 1 plots the simulated outcomes of the model against the standard deviation of migration income (reflecting the risk of migrating) when the subsidy is purely a utility subsidy. Panel (a) (Pre-subsidy) shows that migration under autarky (the red line) decreases as migration becomes riskier, which is consistent with the decline in the certainty equivalent value of migration (Panel (b) (Pre-subsidy)). Unsurprisingly, the treatment effect panel (Panel (c) (Treatment effect)) shows that migration increases with the subsidy. Interestingly, if risk sharing is present, migration rates increase more when migration is

[19]We use a uniform distribution in the village for these simulations (in contrast to the lognormal distribution used in estimation) to abstract from the distribution of marginal migrants in the village; we show simulations with alternative income distributions in Appendix A.4.

risky, but the opposite effect occurs if risk sharing is not present. This result is also mirrored in the treatment effect of the consumption-equivalent value (Panel (b) (Treatment effect)): under endogenous risk sharing, the treatment effect on welfare barely decreases with migration income risk. The key to understanding this counterintuitive result is to examine risk sharing (Panel (c)). Risk sharing is better when migration is riskier (i.e., the risk sharing beta is smaller) both before the subsidy and after the subsidy. The subsidy crowds-out risk sharing (i.e., has a positive treatment effect on risk sharing) only when migration income is not very risky; when migration income is risky then the treatment effect is negative, indicating an improvement in risk sharing.[20]

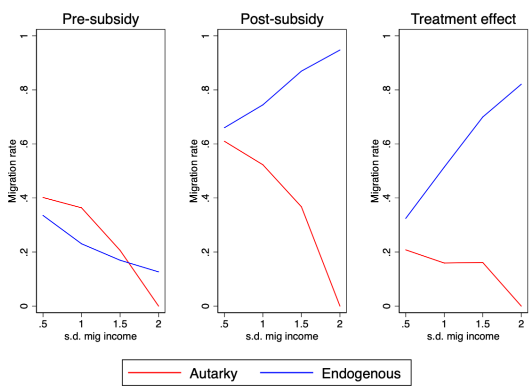
These results are intuitive after considering the effects of the migration subsidy on the certainty equivalent value of migrating. The utility subsidy is a pure utility gain, meaning the subsidy does not alter the marginal utility of consumption. The utility subsidy, therefore, causes a larger gain to the certainty equivalent value of migrating when income is less risky. When the certainty equivalent value is larger, which corresponds to the case where migration income is less risky, then the incentive channel is stronger, and so risk sharing worsens.

Figure 2 considers a financial subsidy which, in contrast to a utility subsidy, directly reduces the marginal utility of consumption. This leads to the opposite intuition: a financial subsidy is particularly valuable when migration is risky because it provides income and thus insurance against a negative migration realization. Panel (a) shows that, as with the utility subsidy, migration is decreasing in migration risk, but now the treatment effect

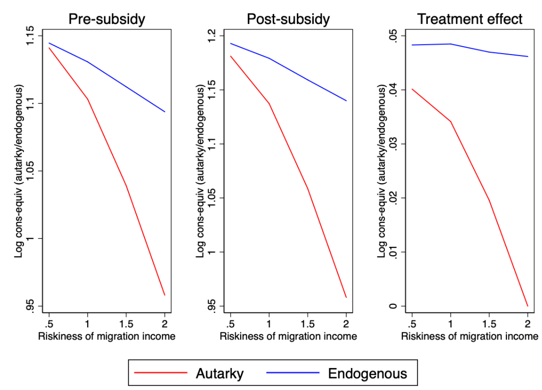
[20]When migration income is very risky then the network supports perfect risk sharing (equivalent to a risk-sharing coefficient of zero). The nonlinearity in Panel (c) (Treatment effect) is because the risk-sharing coefficient cannot fall below zero.

Figure 1: Utility subsidy

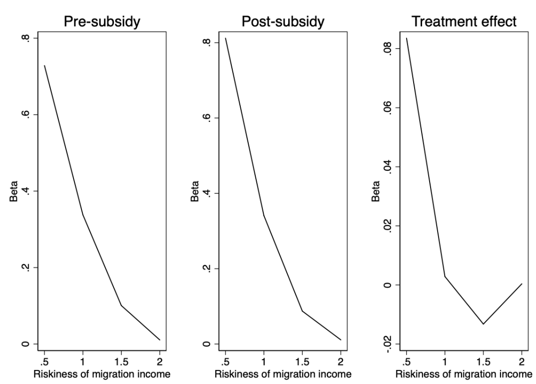
(a) Migration



(b) Cons-equivalent value



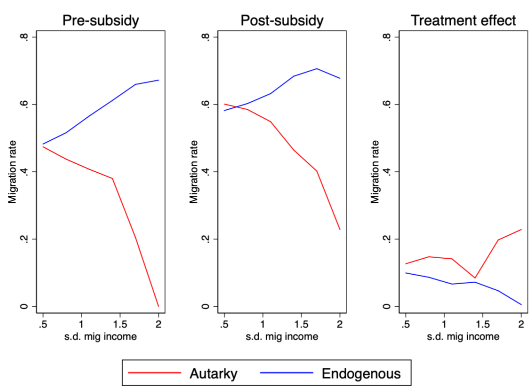
(c) Risk-sharing coefficient



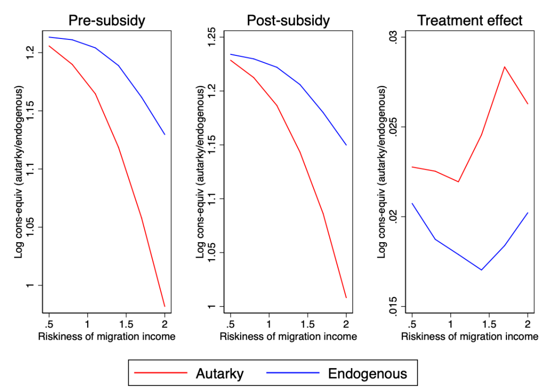
on migration decreases with migration risk more quickly under endogenous risk sharing than under autarky. Panel (b) shows that the treatment effect of welfare under autarky is increasing in migration income risk, while the treatment effect of welfare under risk sharing is decreasing in migration income risk. Subsequently, Panel (c) shows that for low levels of income risk, the treatment effect on the risk-sharing coefficient is positive, reflecting an improvement in risk sharing. However, for high levels of income risk the treatment effect is negative, reflecting a worsening of risk sharing, and consistent with the welfare patterns.

Figure 2: Financial subsidy

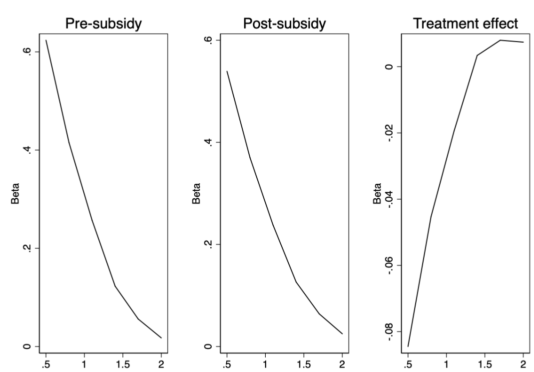
(a) Migration



(b) Cons-equivalent value



(c) Risk-sharing coefficient



The simulations above considered a uniform income distribution in the village. We consider other income distributions in Appendix A.4 and find similar patterns. These simulations shed light on how a limited commitment model can result in either crowding-in or crowding-out of informal insurance, depending on the underlying economic environment in both the origin and destination. We now turn to estimating the economic environment for our empirical setting.

5 Estimation

We structurally estimate the model described in Section 4 using the method of simulated moments and data on the experimental effects of migration subsidies in Bangladesh. There are 13 model parameters, including: (a) the home income process (mean and standard deviation), (b) the city income process for migrants with a job contact (mean and standard deviation), (c) the migration contact process (probability of gaining or losing a contact, conditional on migrating or not), (d) migration costs (utility cost, opportunity cost, and utility subsidy),[21] and (e) risk aversion and the discount factor. We next discuss identification of these parameters, including moment selection and how we exploit the experimental variation for identification, before turning to the estimates.

5.1 Identification

We use the experimental intervention to help identify the model. The experiment both provides a genuine source of exogenous variation and allows us to account for the utility

[21] We net out financial costs of migration from income, and so only estimate the utility cost of migrating.

aspects of the experiment that would otherwise not be identifiable.[22] To estimate the parameters of our model, we construct moments from the data. Here we provide a heuristic discussion of identification based on how the chosen moments inform the parameters. We focus primarily on data collected after the experiment was conducted, combined with data on migration during the experiment to capture migration dynamics. It is important to note that each moment is related to many parameters and we do not imply that there is a one-to-one relationship between each parameter and a moment.[23]

The first set of parameters we consider are those that determine the income distributions, each assumed to be lognormal. These include the home income distribution and the city income distribution for migrants with a contact.[24] This is a complex identification problem because the observed distributions of home income for migrants and non-migrants are censored by the migration decision. In addition, the observed home distribution for migrants is net of the opportunity cost of migration. We identify the home income distribution based on non-migrants, using the decision rule from the model to correct for endogenous selection. We use the observed distribution of home income for migrants to estimate the opportunity cost of migration as the residual – given the distribution of home income and the censoring mechanism – that equalizes the simulated

[22]See Attanasio et al. (2012) for a discussion of the advantages of using experimental variation to identify structural models.

[23]In addition to the main parameters, we estimate scaling factors because the experiment affects the invariant distribution of agents across the state space. This effect may happen in both year $t = 0$, when the migration cost is subsidized, as well as in following periods as the distribution of agents returns to the pre-experiment steady state. To ensure that the budget constraint is satisfied for each period, we solve for a set of transition scaling factors for each time period after the experiment. That is, we solve for one scaling parameter in the pre-experiment steady state ζ_{-1}, one for the year of the experiment ζ_0, and one for each year t after the experiment $(\zeta_1, \zeta_2, ..., \zeta_T)$, until the economy has converged back to the equilibrium steady state (measured by $\zeta_T = \zeta_{-1}$). In practice, the economy converges within seven periods.

[24]In practice, to focus on the role of uncertainty, we set the income distribution in the city for migrants without a contact to be equivalent to receiving zero income (that is, the mean of the lognormal distribution is negative one, and the variance is zero.)

home income for migrants with the value we observe. Finally, the information on the proportion of migrants helps us identify the utility cost of migration in the control group. This utility cost is reduced by the estimated utility subsidy in the treatment group.

The dynamics in our model are in part driven by a migration asset, which captures job contacts formed as a result of past migration episodes and leads to state dependence in migration. The parameters that characterize this asset include three probabilities: obtaining a contact ($\pi^{\text{get contact}}$), losing a previously formed contact if one migrates ($\pi^{\text{lose contact, mig}}$), and losing a previously formed contact if one does not migrate ($\pi^{\text{lose contact, nomig}}$). To identify these parameters, we construct moments that capture migration transitions conditional on earlier migration histories. The experimental variation plays an important role here because it induces a number of people to migrate who did not have any previous migration history. This exogenous variation helps identify the probability of obtaining a contact by observing the re-migration rate following the first migration episode. More specifically, we construct eight moments from the data to identify these parameters: the mean migration rate in control villages; the treatment effect of the experiment on migration during the experiment and after the experiment; the share of people who do not migrate either during or after the experiment (both the control village mean and the treatment effect); and the share of people who migrate both during and after the experiment (again, both the control village mean and the treatment effect).[25]

We directly net out the observed financial costs of migration from our measure of city income. Given this, migration rates also play a key role in identifying the opportunity

[25]The third group, the share of people who migrate in either the experiment or after the experiment, is redundant, so we do not include it in the moment list.

cost (i.e., the share of home income given up) of migrating (α^y) and the utility cost of migrating (d^{utility}). The treatment effect of migration rates help identify the utility subsidy in the experiment ($\Delta d^{\text{utility}}$). The lower treatment effect over time suggests that the utility subsidy decays over time, so we estimate the fraction of the utility subsidy that is still present in the next period. Finally, migration rates may help identify the risk aversion parameter (γ) because migration is risky and therefore a high level of risk aversion would imply a lower rate of migration for a given income gain.

The final set of parameters are those that determine risk sharing. Risk sharing is affected by risk aversion, the discount factor (through patience, since part of the return to risk sharing occurs in the future) and the income distributions (which characterize the riskiness in the economy). We use the risk-sharing coefficient in control villages and the treatment effect on risk sharing to help identify these parameters.[26] Some of these parameters are also informed by other moments. For example, risk aversion is informed by the migration rate.

With this set of moments, we use simulated method of moments (McFadden, 1989; Pakes and Pollard, 1989) to pin down parameter estimates that match model-simulated moments to the data moments as best as possible. We use an identity weighting matrix, placing additional weight on the risk-sharing moments and the migration treatment effects to prioritize model fit on those dimensions, which is the main focus of our paper.

Table 6: Parameter estimates

Preferences	
CRRA parameter	2.10
	(0.043)
Opportunity cost of migration	0.10
	(0.17)
Utility cost of migrating	0.031
	(0.020)
Utility subsidy	0.040
	(0.022)
Decay rate of utility subsidy	0.41
	(0.070)
Income processes	
Mean home income	0.71
	(0.70)
Std. home income	0.39
	(0.14)
Mean city income with contact	0.21
	(0.29)
Std. city income with contact	1.07
	(0.41)
Dynamics	
Prob. get contact	0.96
	(0.65)
Prob. lose contact if migrate	0.47
	(0.094)
Prob. lose contact if don't migrate	0.63
	(0.35)
Model criterion	10.265

Notes: The table shows parameter estimates and standard errors. The parameter estimates arise from estimating the model by simulated method of moments. The analytical standard errors are computed by numerical differentiation.

5.2 Estimation results

Table 6 reports our parameter estimates.[27] The parameters determining the contact rate of migration are all in line with intuition: the chance of finding a contact for migrants who leave without one is 96%, and the contact decays at a slower rate if the worker migrates to keep it active (47% vs 63%). We estimate that the opportunity cost of migrating is equal to 10% of village income.[28] This is in addition to the direct cost of migration, which we observe. The size of the utility subsidy is estimated to be 0.04 utils. Note that although the intervention only occurred in one period, its impact on both migration and risk sharing is felt over future periods. We implement this by estimating the decay rate of the utility subsidy of 0.41 to match the dynamics in the data. Finally, we estimate a CRRA parameter of 2.1, showing moderate levels of risk aversion and not dissimilar to what is found on consumption studies in other countries (Blundell et al., 1994; Attanasio and Weber, 1995). In our main estimates, we set the discount factor to 0.7 (having determined this to be the best fitting value through our grid search process).

The discussion in Section 4.1.1 emphasized the riskiness of migration as a key determinant of the risk-sharing effect and welfare effect, as measured by consumption equivalents. In Appendix Figure 8 we show the sensitivity of the treatment effects (both measured in the post-experiment period) for migration and relative welfare gains of risk sharing over autarky to the riskiness of migration income (x-axis) and the size of the utility

[26]In practice, for computational purposes we use a grid search for the discount factor and for each value we find the optimal coefficient of risk aversion as well as all other parameters.

[27]Details of our procedure to calculate standard errors are in Appendix A.3

[28]We estimate the opportunity cost to match the observed home income of migrants. The estimated opportunity cost is lower than the average migration trip length of 75 days (approximately 20% of the year), but these two do not necessarily need to align, especially if people migrate in periods of the year when income is lower.

subsidy (y-axis). Consistent with the earlier discussion, we find that the relative welfare gains of the experiment are larger (implying an improvement in risk sharing) when income is riskier, as well as when the utility subsidy is bigger. A similar pattern emerges for migration under risk sharing. These patterns suggest that the economic environment in our setting is characterized by high-risk migration.

In Table 7 we show how these parameter estimates fit the treatment effects that we observe through the experiment.[29] The model matches the decline in risk sharing as a result of the experiment reasonably well, predicting a decline of four percentage points in the Townsend-β coefficient for the post-intervention period (compared with a seven percentage point decline in the data). The model also captures the treatment effect on migration during the RCT (20% in the model compared with 22% in the data during the experiment, and 14% compared with 9% after the experiment). The persistence of the migration effect is also captured well: we estimate an increase of 15% in the share of people migrating both during and after the experiment, exactly matching the 15% rate in the data. We slightly underestimate the persistence of non-migration, however, which is perhaps not that surprising given the parsimonious way we have approached the modeling of state dependence: we estimate a decrease of 7% in the share of people migrating neither during nor after the experiment, compared with 17% in the data. Overall, the model is capable of fitting the main patterns in the data, including the change in risk sharing and the dynamics of migration, even with such a parsimonious specification. The fit would likely improve if we allowed for more heterogeneity in the model, which we decided to

[29] Column (2) in Appendix Table 6 presents the model fit on all targeted moments. The model matches the income moments very closely.

avoid to preserve parsimony.

Table 7: Model fit: treatment effects

	(1) Data	(2) Model
Migration rate during RCT	0.22	0.20
Migration rate after RCT	0.094	0.14
Migrate neither during/after RCT	-0.17	-0.067
Migrate during and after RCT	0.15	0.15
Risk sharing after RCT	-0.073	-0.040

Notes: The table shows the treatment effects of the experiment estimated in the data (column (1)) and the treatment effects simulated by the model (column (2)).

5.2.1 Robustness checks of estimates

We run several robustness checks of our estimates. In Appendix Table 6 we reestimate the model with and without the migration asset and with and without allowing for the utility effect of the experiment. We find that the simpler model, without the migration asset, generates similar risk-sharing treatment effects but other moments do not fit the data as well. The utility shock is also important for matching the risk sharing effect. In Appendix Table 7 we show the values of the function for different points of the discount factor grid. The fit of the model is best for a discount factor of 0.7.

5.3 Simulating the experiment inside the model

We now simulate the experiment to study the dynamic effects of the subsidy as well as to decompose the change in risk sharing into separate components attributable to the financial subsidy and to the change in utility costs of migration induced by the experiment (utility subsidy).

Figure 3 plots the time path of migration and risk sharing.[30] Panel (a) shows the combined effect from the two subsidies (financial and utility) under endogenous risk sharing and under autarky (no risk sharing). The left figure shows the migration response. Panels (b) and (c) show the effect of the financial and utility components of the experiment, respectively. While both components contribute to the immediate risk-sharing effect, the utility subsidy plays a larger role in the risk-sharing effect over time. All of the migration effect, on the other hand, stems from the utility subsidy; the financial subsidy has a small and negative net effect on migration due to the income effect from the extra resources (a point we will return to in the counterfactuals in Section 6.)

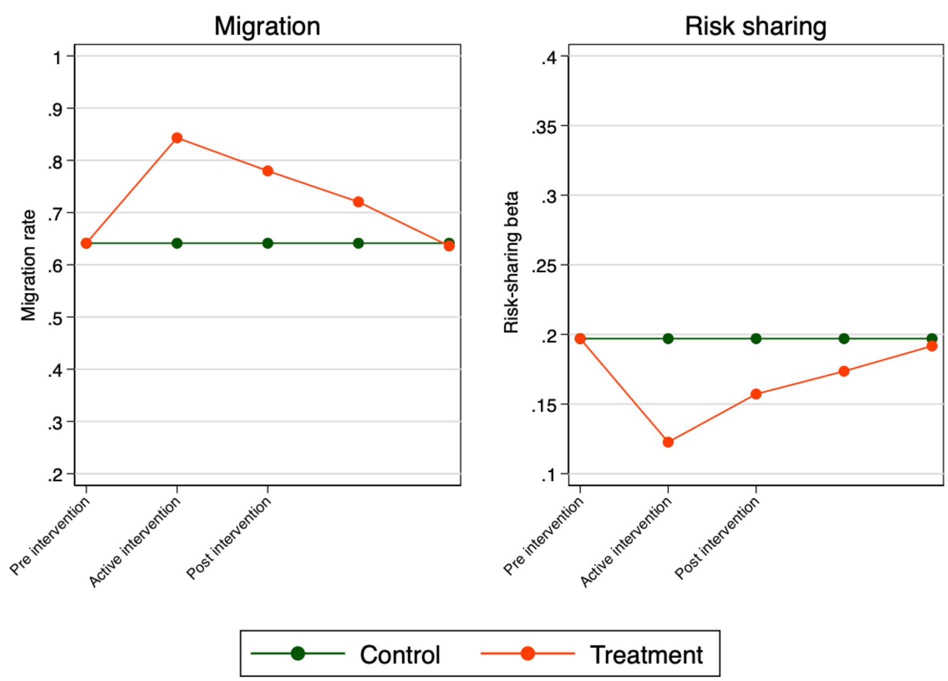
Table 8 converts these results into welfare gains of the experiment in units of permanent consumption equivalents, both during the experiment and the period after the incentives were disbursed. The experiment led to an increase in welfare equivalent to a permanent 7.2% increase in consumption, net of the financial subsidy itself, the year the experimental subsidies were disbursed. In comparison, the estimated gain of the experiment without accounting for the risk-sharing response would be a consumption-equivalent gain of 5.1%. In other words, welfare gains are 42% higher when accounting for spillover effects of the experiment through risk sharing. The second panel examines the welfare effects in the period after the incentives were disbursed (which aligns with the period we match in estimation). Because of the persistent improvement in risk sharing, the welfare gains of the experiment remain higher under risk sharing (equivalent

[30] Appendix Figure 9 shows the same figure for alternative estimates where we do not include the migration asset or where we do not allow the model to affect utility.

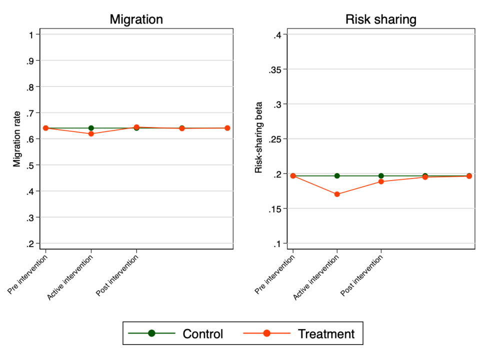
to a 4.7% increase in permanent consumption) than under autarky (a 3.2% increase in consumption).

Figure 3: Effect of the experiment on migration and risk sharing

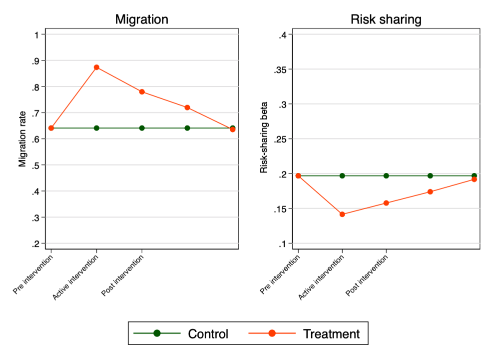
(a) Combined effect



(b) Financial component



(c) Utility component



In sum, our model of endogenous migration and risk sharing can replicate key features of the economic environment and experimental setting in rural Bangladesh. Using the model, we show that both the financial and utility components of the migration sub-

Table 8: Effect of the experiment on risk sharing and welfare

Endogenous risk sharing			Autarky	
(1)	(2)	(3)	(4)	(5)
Risk sharing	Cons-equiv	Cons-equiv (net of subsidy)	Cons-equiv	Cons-equiv (net of subsidy)
Effect when subsidies disbursed				
-0.0743	0.135	0.0723	0.131	0.0508
Effect after subsidies disbursed				
-0.0398	0.0469	0.0469	0.0319	0.0319

Notes: Risk sharing is percentage point changes. Consumption-equivalent, consumption-equivalent net of subsidy, and welfare are percent changes.

sidies have important effects on migration, risk sharing, and welfare, and that ignoring the welfare effects stemming from risk-sharing improvements leads to an underestimate of the overall benefits of the subsidies. We next turn to alternative counterfactual policies as a way to further understand the economic forces that drive the codetermination of migration and risk sharing.

6 Counterfactuals

In this section, we use our model of endogenous risk sharing and migration to conduct two alternative policy experiments: first, the effect of a permanent subsidy rather than temporary subsidy; and second, an unconditional cash transfer rather than a subsidy conditional on migrating.

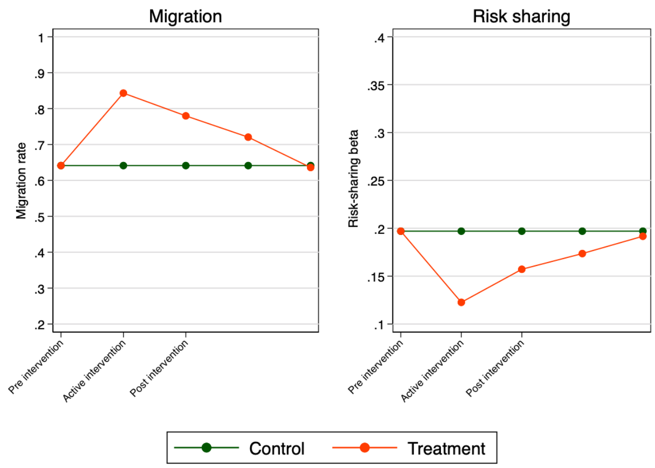
Figure 4 shows the migration and risk sharing effects of a temporary subsidy (i.e., our main results) in Panel (a) and the effects of a permanent subsidy in Panel (b). We expect that the permanent subsidy may differ from the temporary subsidy for two reasons: first, the fact that the subsidy is permanent implies a permanent improvement in the outside option which could make risk sharing less important, and second, the fact that migration is persistent may imply that the average migrant improves their job contacts over time, reducing the risk of migration. We find that longer-term subsidies (rather than the one-off subsidy in the experiment) can reverse the risk sharing effects and instead lead to crowding out because the subsidy acts as a more permanent safety net, improving the outside option substantially relative to participating in the risk sharing network.[31] In our context, making the subsidy permanent would lead to a permanent 16 percentage point decline in risk sharing rather than the short-term improvement in risk sharing that accompanied the temporary subsidy. This finding has an important implication for the validity of extrapolating from RCTs to alternative policies: in this case the experimental evidence, which is by nature short-lived, has the opposite implications for risk sharing than the permanent policy.

We next consider an unconditional cash transfer (UCT) rather than a conditional cash transfer (CCT, corresponding to the financial subsidy only) in Figure 5. We estimate that the UCT would reduce migration further and improve risk sharing. Because everyone in the village receives the UCT, whereas only those who migrate receive a CCT, the UCT is a much larger income transfer. As such, the driving force in the risk-sharing effect is an income effect, compared with the driving force for the CCT coming through the income risk of migrating.

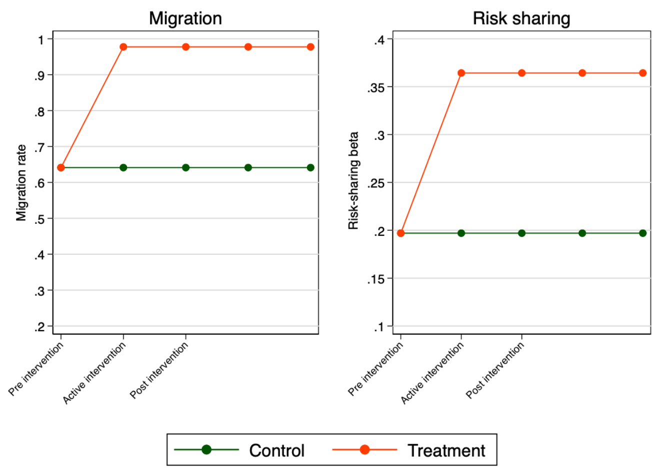
[31] Appendix Figures 10 and 11 consider the decomposition into the financial and utility components.

Figure 4: Temporary vs permanent subsidy

(a) Temporary subsidy



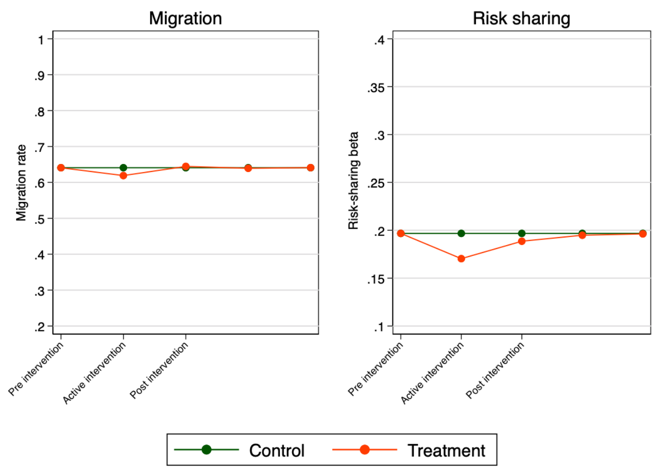
(b) Permanent subsidy



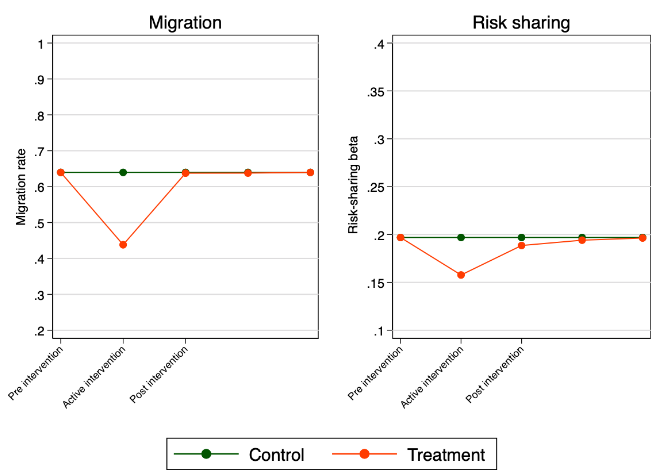
Note: Shock is both financial and utility shock.

Figure 5: One-time conditional vs unconditional transfer

(a) Conditional cash transfer



(b) Unconditional cash transfer



Note: The left panel shows the effect of an 800 Taka subsidy conditional on migrating. The right panel shows the effect of an unconditional 800 Taka subsidy.

7 Conclusion

Our paper makes several contributions to our understanding of the trade-offs involved with experimentally introducing new income opportunities into a village. The experiment we use is described in Bryan et al. (2014): households in rural Bangladesh were randomly offered subsidies to migrate temporarily during the agricultural lean season. We focus on the interaction between migration and social safety nets and examine how new migration opportunities affect the nature and extent of informal risk sharing within villages. It is not obvious the direction these effects will take, as they depend on the nature of the intervention and the local context. Understanding the effects of facilitating migration also has broad relevance in many developing countries with high levels of agricultural risk.

Our first contribution, based directly on the experimental results, is to show that the subsidy *improved* risk sharing by 40%. In order to understand this result, as well as the broader policy implications, we build a model that reveals the conditions under which risk sharing improves. Our model draws on Morten (2019), which includes limited commitment constraints on risk sharing (Kocherlakota, 1996; Ligon et al., 2002) and an endogenous migration decision based on the net return of migrating (Harris and Todaro, 1970), and adds a dynamic component making migration state-dependent.

With the model in hand, we next show that the model can produce both improvements and declines in risk sharing. The key forces are the riskiness of the migration option and the expected value of city income. A highly risky migration option, which on average yields substantial returns, encourages risk sharing in exchange for the benefit of this potentially large return. On the other hand a relatively safe migration option, made easier by the intervention, discourages risk sharing because it also substantially improves the outside autarky option. Other factors also play a role, such as the relative size of financial and utility migration costs; our analysis points to many of these factors and how they interact.

We then fit our model to the experimental data to provide a framework to interpret the experimental findings and to evaluate the welfare effects of the program. The model fits the experimental treatment effects on migration and risk sharing reasonably well, providing a set of parameters that is consistent with improvements in the amount of insurance available. We use the model to conduct alternative policy experiments in which the subsidy is decomposed and made permanent, and show that a permanent utility subsidy leads to a permanent improvement in risk sharing while a permanent financial subsidy leads to a permanent *decline* in risk sharing. This result serves to illustrate how experimental evidence, which may involve only temporary interventions, may have very different impacts than longer-term policies.

These findings suggest a broader policy implication: the effectiveness of safety net programs and conditional transfers very much depends on how community networks operate and on the risk/return profile of the activities encouraged. In our experimental setting, the subsidy led to a positive spillover on risk sharing, generating larger welfare gains than in the absence of risk sharing. Nevertheless, we also show that in slightly different contexts, the opposite result may occur. As a result, it is important to take into account these spillover effects when designing social protection programs.

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A Theoretical and computational appendix

A.1 Model extension to N people

The model presented in the paper was between 2 households. It is possible to extend the model to N households. Now, the Pareto frontier traces out the utility to household N, given a promised utility to households $1, ..., N-1$. The first order conditions for this problem are presented here.

Starting with the after-migration problem:

$$\begin{aligned}
\widehat{V}_{sqj\hat{A}}(\widehat{U}^1_{sqj\hat{A}}, \widehat{U}^2_{sqj\hat{A}}, ..., \widehat{U}^{N-1}_{sqj\hat{A}}) = & \max_{\left\{\tau^1, \tau^2, ..., \tau^{N-1}, \left\{U^1_{s'A'}, U^2_{s'A'}, ..., U^{N-1}_{s'A'}\right\}_{\forall s', A'}\right\}} u(c^N + \tau^1 + \tau^2 + ... + \tau^{N-1}) - \mathbb{I}^2_j d + \beta \sum_{s'} \sum_{A'} \pi_{s'|s} \pi_{A'|\hat{A},j} V_{s'A'}(U^1_{s'A'}, U^2_{s'A'}, ..., U^{N-1}_{s'A'}) \\
& + \sum_{i=1}^{N-1} \widehat{\lambda}^i \left(u(c^i - \tau^i) - \mathbb{I}^i_j d + \beta \sum_{s'} \sum_{A'} \pi_{s'|s} \pi_{A'|\hat{A},j} U^i_{s'A'} - \widehat{U}^i_{sqj\hat{A}} \right) \\
& + \beta \sum_{i=1}^{N-1} \sum_{s'} \sum_{A'} \pi_{s'|s} \pi_{A'|\hat{A},j} \widehat{\lambda}^i \phi^i_{s'A'} \left(U^i_{s'A'} - \Omega^i_{s'A'} \right) \\
& + \beta \sum_{s'} \sum_{A'} \pi_{s'|s} \pi_{A'|\hat{A},j} \widehat{\lambda}^N \phi^N_{s'A'} \left(V_{s'A'}(U^1_{s'A'}, U^2_{s'A'}, ..., U^{N-1}_{s'A'}) - \Omega^N_{s'A'} \right)
\end{aligned}$$

The FOC are:

$$\begin{aligned}
\frac{\partial}{\partial \tau^i} : \quad & u'(c^N) - \widehat{\lambda}^i u'(c^i) = 0 \quad , \forall i \neq N \\
\frac{\partial}{\partial U^i_{s'A'}} : \quad & \beta \pi_{s'|s} \pi_{A'|\hat{A},j} \frac{\partial V_{s'}(U^1_{s'A'}, U^2_{s'A'}, ..., U^{N-1}_{s'A'})}{\partial U^i_{s'A'}} + \beta \widehat{\lambda}^i \pi_{s'|s} \pi_{A'|\hat{A},j} + \beta \pi_{s'|s} \pi_{A'|\hat{A},j} \phi^i_{s'A'} \\
& + \beta \pi_{s'|s} \pi_{A'|\hat{A},j} \phi^N_{s'A'} \frac{\partial V_{s'A'}(U^1_{s'A'}, U^2_{s'A'}, ..., U^{N-1}_{s'A'})}{\partial U^i_{s'A'}} = 0 \quad , \forall i = 1, .., N-1; s' \\
\text{Envelope condition for } \widehat{U}^i_{sqj\hat{A}} : \quad & \frac{\partial \widehat{V}_{sqj\hat{A}}}{\partial \widehat{U}^1_{sqj\hat{A}}} (\widehat{U}^1_{sqj\hat{A}}, \widehat{U}^2_{sqj\hat{A}}, ..., \widehat{U}^{N-1}_{sqj\hat{A}}) = -\widehat{\lambda}^i \quad , \forall i = 1, ..., N-1
\end{aligned}$$

Rearranging, this yields:

$$\begin{aligned}
\frac{u'(c^N)}{u'(c^i)} &= \widehat{\lambda}^i \quad , \forall i \neq N \\
\frac{\partial \widehat{V}_{sqj\hat{A}}}{\partial \widehat{U}^1_{sqj\hat{A}}} (\widehat{U}^1_{sqj\hat{A}}, \widehat{U}^2_{sqj\hat{A}}, ..., \widehat{U}^{N-1}_{sqj\hat{A}}) &= -\widehat{\lambda}^i \\
\frac{\partial V_{s'A'}(U^1_{s'A'}, U^2_{s'A'}, ..., U^{N-1}_{s'A'})}{\partial U^i_{s'A'}} &= -\widehat{\lambda}^i \frac{(1 + \phi^i_{s'A'})}{(1 + \phi^N_{s'A'})} \quad , \forall s', A', i \neq N
\end{aligned}$$

The before-migration problem is the following:

$$
\begin{aligned}
V_{sA}(U^1_{sA}, U^2_{sA}, ..., U^{N-1}_{sA}) = \max_j \max_{\left\{\widehat{U}^1_{sqj\hat{A}}, \widehat{U}^2_{sqj\hat{A}}, ..., \widehat{U}^{N-1}_{sqj\hat{A}}\right\}_{\forall q, \hat{A}}} & \sum_q \sum_{\hat{A}} \pi_q \pi_{\hat{A}|A,j} V_{sqj\hat{A}}(\widehat{U}^1_{sqj\hat{A}}, \widehat{U}^2_{sqj\hat{A}}, ..., \widehat{U}^{N-1}_{sqj\hat{A}}) \\
& + \sum_{i=1}^{N-1} \lambda^i (\sum_q \sum_{\hat{A}} \pi_q \pi_{\hat{A}|A,j} \widehat{U}^i_{sqj\hat{A}} - U^i_{sA}) \\
& + \sum_q \sum_{\hat{A}} \pi_q \pi_{\hat{A}|A,j} \sum_{i=1}^{N-1} \lambda^i \alpha^i_{qj\hat{A}} (\widehat{U}^i_{sqj\hat{A}} - \widehat{\Omega}^i_{sAjq\hat{A}}) \\
& + \sum_q \sum_{\hat{A}} \pi_q \pi_{\hat{A}|A,j} \alpha^N_{qj\hat{A}} (\widehat{V}_{sqj\hat{A}}(\widehat{U}^1_{sqj\hat{A}}, \widehat{U}^2_{sqj\hat{A}}, ..., \widehat{U}^{N-1}_{sqj\hat{A}}) - \widehat{\Omega}^N_{sAjq\hat{A}})
\end{aligned}
$$

The FOC are:

$$
\begin{aligned}
\frac{\partial}{\partial \widehat{U}^i_{sqj\hat{A}}}: \quad & \pi_q \pi_{\hat{A}|A,j} \frac{\partial \widehat{V}_{sqj\hat{A}}(\widehat{U}^1_{sqj\hat{A}}, \widehat{U}^2_{sqj\hat{A}}, ..., \widehat{U}^{N-1}_{sqj\hat{A}})}{\partial \widehat{U}^i_{sqj\hat{A}}} + \lambda^i \pi_q \pi_{\hat{A}|A,j} + \pi_q \pi_{\hat{A}|A,j} \lambda^i \alpha^i_{qj\hat{A}} \\
& + \pi_q \pi_{\hat{A}|A,j} \alpha^N_{qj\hat{A}} \frac{\partial V_{sqj\hat{A}}(\widehat{U}^1_{sqj\hat{A}}, \widehat{U}^2_{sqj\hat{A}}, ..., \widehat{U}^{N-1}_{sqj\hat{A}})}{\partial \widehat{U}^i_{sqj\hat{A}}} \\
\text{Envelope:} \quad & \frac{\partial V_{sA}(U^1_{sA}, U^2_{sA}, ..., U^{N-1}_{sA})}{\partial U^i_{sA}} = -\lambda^i
\end{aligned}
$$

Rearranging yields:

$$
\begin{aligned}
\frac{\partial V_{sqj\hat{A}}(\widehat{U}^1_{sqj\hat{A}}, \widehat{U}^2_{sqj\hat{A}}, ..., \widehat{U}^{N-1}_{sqj\hat{A}})}{\partial \widehat{U}^i_{sqj\hat{A}}} &= -\lambda^i \frac{(1+\alpha^i_{qj\hat{A}})}{(1+\alpha^N_{qj\hat{A}})} \quad \forall q, j \\
\frac{\partial V_{sA}(U^1_{sA}, U^2_{sA}, ..., U^{N-1}_{sA})}{\partial U^i_{sA}} &= -\lambda^i
\end{aligned}
$$

Putting together the two problems, the before-migration and after-migration Pareto weights follows the following updating rule. Given an initial before-migration Pareto weight $\lambda^i_t(s_t, A_t, h^{t-1})$, the after-migration Pareto weight is given by:

$$
\widehat{\lambda}^i_t(s_t, q_t, j_t, \widehat{A}_t, h^{t-1}) = \begin{cases} \widehat{\underline{\lambda}}^i_{sqj\widehat{A}}, & \text{if } \lambda_t(s_t, A_t, h^{t-1})^i \le \widehat{\underline{\lambda}}^i_{sqj\widehat{A}} \\ \lambda^i_t(s_t, A_t, h^{t-1}), & \text{if } \lambda_t(s_t, A_t, h^{t-1})^i \in [\widehat{\underline{\lambda}}^i_{sqj\widehat{A}}, \widehat{\overline{\lambda}}^i_{sqj\widehat{A}}] \\ \widehat{\overline{\lambda}}^i_{sqj\widehat{A}} & \text{if } \lambda_t(s_t, A_t, h^{t-1})^i \ge \widehat{\overline{\lambda}}^i_{sqj\widehat{A}} \end{cases}
$$

And the before-migration Pareto weight the following period is given by:

$$
\lambda^i_{t+1}(s'_{t+1}, A_{t+1}, h^t) = \begin{cases} \underline{\lambda}^i_{s'A'}, & \text{if } \widehat{\lambda}^i_t(s_t, q_t, j_t, \widehat{A}_t, h^{t-1}) \le \underline{\lambda}^i_{s'A'} \\ \widehat{\lambda}^i_t(s_t, q_t, j_t, \widehat{A}_t, h^{t-1}), & \text{if } \widehat{\lambda}^i_t(s_t, q_t, j_t, \widehat{A}_t, h^{t-1}) \in [\underline{\lambda}^i_{s'A'}, \overline{\lambda}^i_{s'A'}] \\ \overline{\lambda}^i_{s'A'} & \text{if } \widehat{\lambda}^i_t(s_t, q_t, j_t, \widehat{A}_t, h^{t-1}) \ge \overline{\lambda}^i_{s'A'} \end{cases}
$$

As a direct result of this updating rule, the rate of growth of marginal utility for any two households i and g is given by:

$$\frac{u'(c_t^g(s_t, q_t, j_t, \hat{A}_t, h^{t-1}))/u'(c_{t-1}^g(h^{t-1})}{u'(c_t^i(s_t, q_t, j_t, \hat{A}_t, h^{t-1}))/u'(c_{t-1}^i(h^{t-1}))} = \frac{(1+\alpha^i_{sqj\hat{A},t})}{(1+\alpha^g_{sqj\hat{A},t})} \frac{(1+\phi^i_{sA,t})}{(1+\phi^g_{sA,t})}$$

If neither household is constrained ($\alpha^i_{sqj\hat{A},t} = \alpha^g_{sqj\hat{A},t} = \phi^i_{sA,t} = \phi^g_{sA,t}$ =0) then the rate of growth of marginal utility is constant (but unknown) for the two households. This equation therefore gives one more equation that, together with the budget constraint that total income equals total consumption, allows us to estimate the level of utility. To do this we need to estimate the scaling factor, ζ_t, where t indexes the time period relative to the one-off experiment, such that total consumption is equal to total income. That is, we find ζ_t such that $\hat{\lambda}^i_t = \max(\zeta_t \hat{\lambda}^i_{t-1}, \underline{\hat{\lambda}})$ and the invariant distribution of income is equal to the invariant distribution of consumption. We solve for one scaling parameter in the pre-experiment steady state ζ_{-1}, one for the year of the experiment ζ_0, and one for each year t after the experiment $(\zeta_1, \zeta_2, ..., \zeta_T)$, until the economy has converged back to the equilibrium steady state (measured by $\zeta_T = \zeta_{-1}$).

A.2 Computational algorithm

We solve the problem using a similar algorithm to that in Morten (2019). We solve the algorithm assuming that there are two households: an individual household and the "rest of the village" household. We solve by value function iteration for two grids: the before-migration grid and the after-migration grid.

The before-migration grid has the following state variables: $\{\lambda, s, A, t\}$ where λ is the before-migration Pareto weight; s is the state of the world in the village; A is before-migration asserts, and t measures time relative to the temporary shock: $t = -1, 0, 1, ..., T$ where T is large enough so that the economy has returned to the steady-state equilibrium (in practice, this occurs by $T = 7$).

The after-migration grid has the following state variables: $\{\hat{\lambda}, s, q, j, \hat{A}, t\}$ where $\hat{\lambda}$ is the after-migration Pareto weight, s is the state of the world in the village, q is the state of world in the destination, j indexes the migration decision, $\hat{A}$ is after-migration asserts, and t measures time relative to the temporary shock.

The algorithm loops through three steps to solve the problem. First, holding the scaling factor constant and migration constant, it solves for the optimal lower and upper bounds for the before-migration and after-migration Pareto weights. Second, holding the scaling factor constant, it solves for a fixed point of the migration decision. Third, it solves for a fixed point of the scaling factor such that the economy-wide budget constraint is satisfied.

This requires the following steps:

1. Start with an initial guess for the scaling vector Z. Set $Z_0 = \{\zeta_0, \zeta_1, ..., \zeta_T\} = 1$.

2. Start with a guess of the migration rule, j_0, for each point of the before-migration grid. A good initial guess is autarky migration.

3. For Z, j_0, find the optimal LB and UB for the before-migration and after-migration Pareto intervals:

   (a) Construct an initial transition rule, $\Pi_{before-mig,after-mig}$ which maps each point on the before-migration grid to a point on the after-migration grid. This transition rule will depend on probability of migrating,j_0, the probability of making a contact if migrate, $p_{\hat{A}|A,j}$ and the probability of the migration state, π_j. It will also depend on the minimum rule for the Pareto weight. Start by assuming $\underline{\widehat{\lambda}} = \lambda_{min}, \overline{\overline{\lambda}} = \lambda_{max}$. .

   (b) Construct an initial transition rule for the after-migration to next period , $\Pi_{after-mig,before-mig}$, which maps each point on the after-migration grid to the before-migration grid. This transition rule will depend on the updating rule for the migration contact, $\pi A'|\hat{A}, j$ and the probability of the village state, $\pi_{r|s}\pi_{A'|\hat{A},j}$. It will also depend on the minimum rule for the Pareto weight. Start by assuming $\underline{\widehat{\lambda}} = \lambda_{min}, \overline{\overline{\lambda}} = \lambda_{max}$.

   (c) Using j_0, solve for the expected value of total resources in the economy. Use this to construct the implied income for the rest-of-village household.

(d) Start solving the algorithm:

i. Guess an initial value for the before-migration value function. A good guess for this is the maximum of the autarky and perfect risk sharing value.

ii. Hold migration constant at the initial value .

iii. Solve for the value of the after-migration value function, using the Pareto weight and total resources to set consumption, and then the continuation value with the before-migration value function.

iv. Find the $\underline{\lambda}$ and $\overline{\lambda}$ such that household a's budget constraint is binding and the r-o-v budget constraint is binding. For values where the other household is at a binding participation constraint, impose the interpolated value of the value function for the non-binding household.

v. Now, turn to the before-migration grid. Construct the expected value of migrating and the expected value of not migrating. Impose the before-migration participation constraint the same way as the step above.

vi. Then, find the new migration rate. We use a smoothing rule where the probability of migrating is given by $\frac{V_{mig}-V_{nomig}}{\psi}$, where ψ is a smoothing parameter.

vii. Then, construct the new before-migration value function as the probability of migrating multiplied by the migration after-migration value function, and (1-probability of migrating) multiplied by the no-migration after-migration value function.

viii. Iterate until the before-migration value function has converged .

ix. Compare the migration rate with the initial migration rate. Update initial migration rate and repeat steps 3(d)ii-3(d)viii.

(e) This yields an updated guess for the migration policy function, j_1, as well as the lower and upper bounds for the before-migration and after-migration Pareto intervals. Update the transition matrices in Steps 2 and 3 using the new Pareto weights and an updated guess for migration .

(f) Compute $\epsilon_{mig} = ||j_1 - j_0||$ for each point on the before-migration grid. Update the guess for j_0. This then leads to an update in the total resources available to the economy.

(g) Complete Steps 3a - 3e until $\epsilon_{mig} \leq \underline{\epsilon}$ for some small ϵ. When this occurs, migration decisions (and therefore total resources) are stable.

4. Given the final transition matrices, find the invariant distribution of households across the after-migration grid. Compute total consumption and total income for each value of t. Find $Z_1 = \{\zeta^*_{-1}, \zeta^*_0, ..., \zeta^*_T\}$ such that excess demand is equal to zero for each t.

5. Compute $\epsilon_Z = ||Z_1 - Z_0||$ for each time period t. Update the value of Z_0. This updates the transition matrices and so therefore affects the value function and the invariant distribution of agents, so affects the total resources available to the economy.

6. Update 3a-5 until $\epsilon_Z \leq \underline{\epsilon}$ for some small ϵ.

A.3 Standard errors

To compute standard errors we numerically approximate the variance-covariance matrix. Following the notation in Hansen (2016), the SMM estimator minimizes the weighted deviations of the simulated moments from the data moments, $g(\beta)$. The moment vector has dimension $(l \times 1)$ and the parameter vector, β, has dimension $(k \times 1)$.

The SMM estimator minimizes:

$$\beta_{SMM} = \texttt{argmin}_{\beta}\bar{g}(\beta)'W\bar{g}(\beta)$$

where W is a weight matrix of dimension $l \times l$.

The variance-covariance matrix is given by:

$$V_{\beta} = \left(Q'WQ\right)^{-1}\left(Q'W\Omega WQ\right)\left(Q'WQ\right)^{-1}$$

We estimate the Jacobian matrix, Q, by numerical differentiation, taking step sizes of 1% of the estimated value in either direction.

$$\widehat{Q} = \mathbb{E}\frac{\partial}{\partial\beta'}g_i(\beta)$$

And we approximation Ω by the sample analog:

$$\widehat{\Omega} = \mathbb{E}g_i(\beta)g_i(\beta)'$$

A.4 Efficient vs autarkic migration

This appendix derives some results regarding the certainty equivalence value of migrating and the expected income from migrating. We then use these results to develop intuition for when a migration subsidy will lead to a crowding-out of risk sharing and when it will lead to a crowding-in.

Consider a migration income process, $y_m \sim F(\mu_m, \sigma_m)$. Define three certainty equivalent values of migrating – no subsidy, financial subsidy, and utility subsidy, respectively – by:

$$\begin{aligned} ce &= Eu(y_m) \\ ce^{\text{fin}} &= Eu(y_m + d^{\text{fin}}) \\ ce^{\text{utility}} &= Eu(y_m) + d^{\text{utility}} \end{aligned}$$

Define the change in utility as the new utility minus the no-subsidy utility:

$$\begin{aligned} \Delta ce^{\text{fin}} &= ce^{\text{fin}} - ce \\ \Delta ce^{\text{utility}} &= ce^{\text{utility}} - ce \end{aligned}$$

Define similar objects for the expected income from migrating – no subsidy, financial subsidy, and utility subsidy respectively – with the expected income under utility subsidy defined implicitly in turns of the income that gives the same utility,

$$\begin{aligned} Ey &= E_y y_m \\ Ey^{\text{fin}} &= E_y y_m + d^{\text{fin}} \\ u(Ey^{\text{utility}}) &= u(Eu(y_m)) + d^{\text{utility}} \end{aligned}$$

And define the change in expected income in the same way:

$$\begin{aligned} \Delta Ey^{\text{fin}} &= Ey^{\text{fin}} - Ey \\ \Delta Ey^{\text{utility}} &= Ey^{\text{utility}} - Ey \end{aligned}$$

We plot examples of ce and Ey in Appendix Figure 7.

Consider a mean-preserving change in the distribution arising from increasing the standard deviation of the income distribution. If utility is concave, the following two statements are true:

1. $\Delta \text{ce}^{fin} > \Delta \text{Ey}^{fin}$: The change in the certainty value of migration is always larger than the change in the mean resources after a financial subsidy. This is because the financial subsidy increases income in all states of the world, acting as insurance against the worst income realizations.

2. $\Delta ce^{utility} < \Delta Ey^{utility}$: The change in the certainty value of migration is always smaller than the (equivalent) change in the mean resources after a utility subsidy. Because $Ey \geq ce$, marginal utility is lower at Ey, and so a larger equivalent increase in consumption is required to generate the same utility gain.

3. $\frac{\partial \Delta ce^{fin}}{\partial \sigma} > 0$: The certainty equivalent gain from a financial subsidy is increasing in the standard deviation of the shock. This is due to the insurance function of the financial subsidy, which becomes more valuable the riskier migration is.

4. $\frac{\partial \Delta ce^{utility}}{\partial \sigma} < 0$: The certainty equivalent gain from a utility subsidy is decreasing in the standard deviation of the shock. This is because a riskier income process lowers ce and so a smaller consumption increase is required to generate the same utility gain.

When households are independent, migration decisions are made based on the certainty equivalent value of migration. If households instead were fully pooling income risk, migration decisions would be made based on expected income. The actual change in migration is a function of the effect of the shock and the mass of marginal potential migrants in the village, which depends on the underlying income distribution in the village. The migration response for a shock $i = \{\text{fin}, \text{utility}\}$ is given by:

$$\text{Change in migration if household independent: } \int_{\text{ce}}^{\text{ce}^{\text{i}}} f(x)dx$$
$$\text{Change in migration if fully pool income: } \int_{\text{Ey}}^{\text{Ey}^{\text{i}}} f(x)dx$$

A financial subsidy will have a larger effect on the certainty equivalent value of migrating relative to the expected income (and more so if migrating is riskier). If village income is uniformly distributed, then the migration response if households are independent will be larger than the migration response if households are fully pooling income. In this case, we may expect the incentive effect to be tighter and hence risk sharing to reduce. On the other hand, a utility subsidy will have a smaller effect on the certainty equivalent value of migrating, relative to the expected income (and more so if migrating is less risky). This may lead to a looser incentive effect and a improvement in risk sharing.

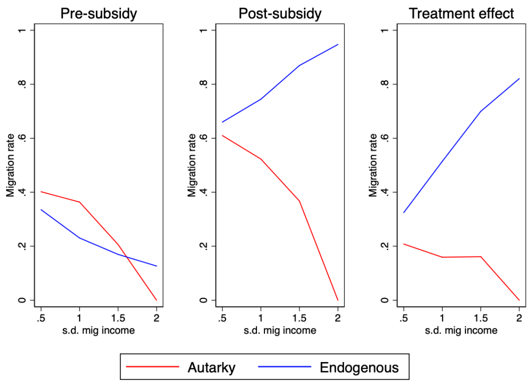
We simulate the risk sharing model for three different village income distributions: an uniform distribution, characterized by mean $\mu = 3$ and equal support in $[1.8, 4.2]$, a normal distribution, characterized by mean $\mu = 3$ and variance $\sigma = 0.8$, and a lognormal distribution characterized by mean $\mu = 3$ and variance $\sigma = 0.8$.

Appendix Figures 1 and 2 show the case of a utility subsidy and financial subsidy, respectively, when income is uniformly distributed in the village.

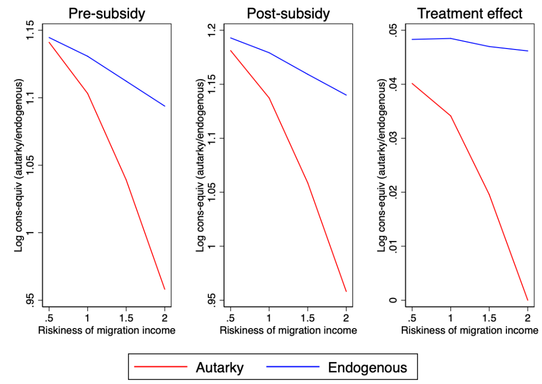
Appendix Figures 3 and 4 show the case of a utility subsidy and financial subsidy, respectively, when income is normally distributed in the village.

Appendix Figure 1: Utility subsidy (uniform income distribution in village)

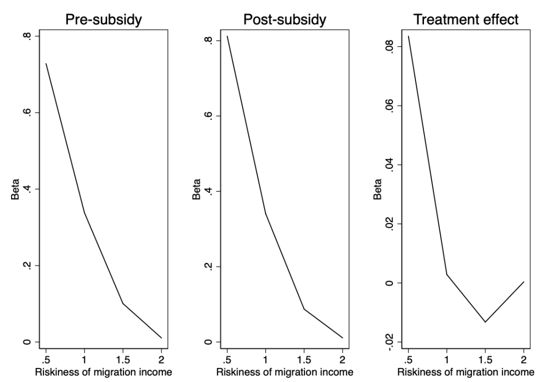
(a) Migration



(b) Cons-equivalent value

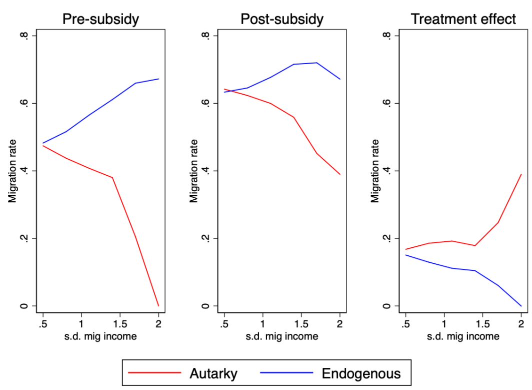


(c) Risk-sharing coefficient

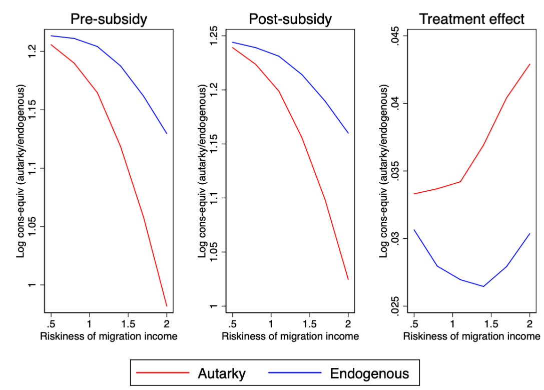


Appendix Figure 2: Financial subsidy (uniform income distribution in village)

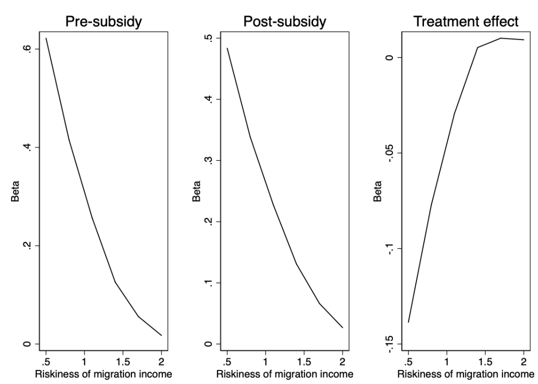
(a) Migration



(b) Cons-equivalent value

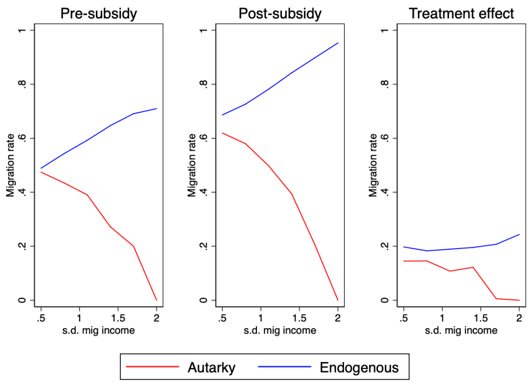


(c) Risk-sharing coefficient

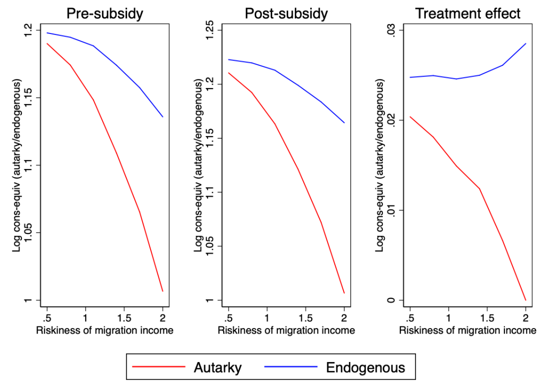


Appendix Figure 3: Utility subsidy (normal income distribution in village)

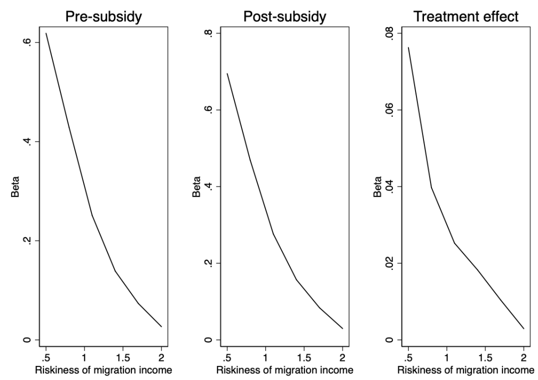
(a) Migration



(b) Cons-equivalent value

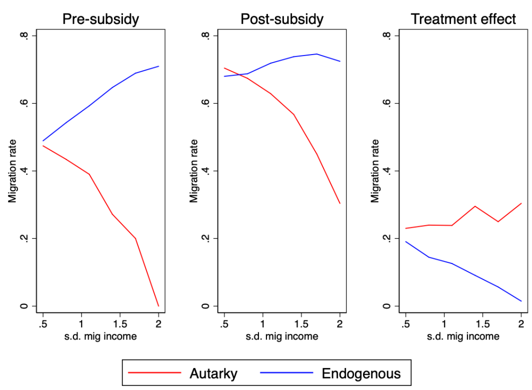


(c) Risk-sharing coefficient

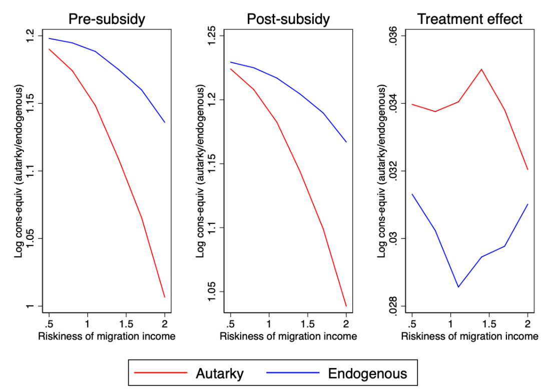


Appendix Figure 4: Financial subsidy (normal income distribution in village)

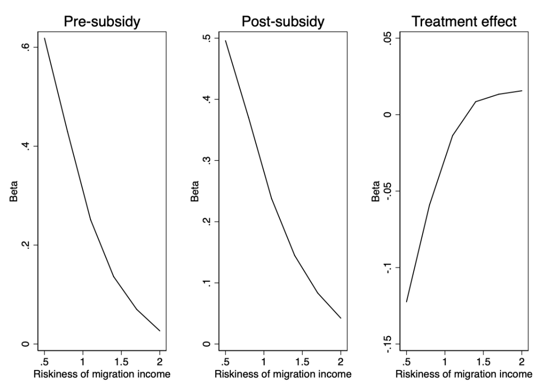
(a) Migration



(b) Cons-equivalent value

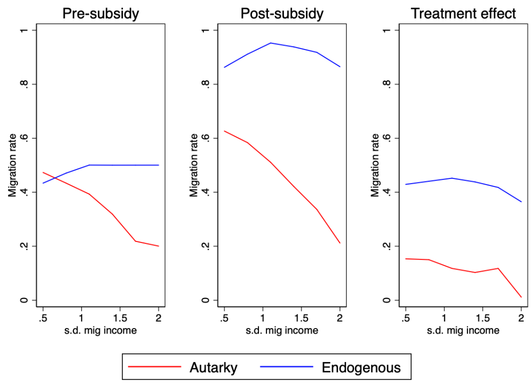


(c) Risk-sharing coefficient

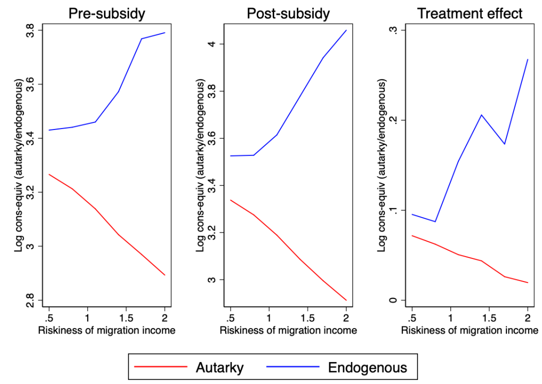


Appendix Figure 5: Utility subsidy (lognormal income distribution in village and city)

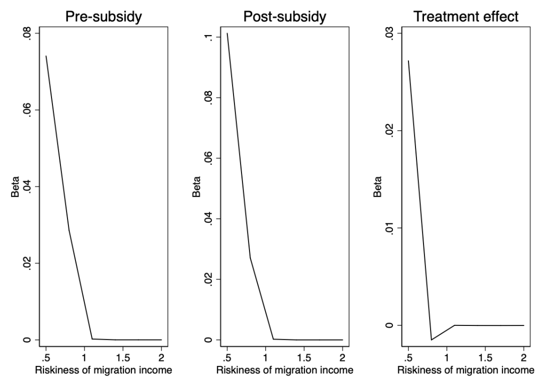
(a) Migration



(b) Cons-equivalent value

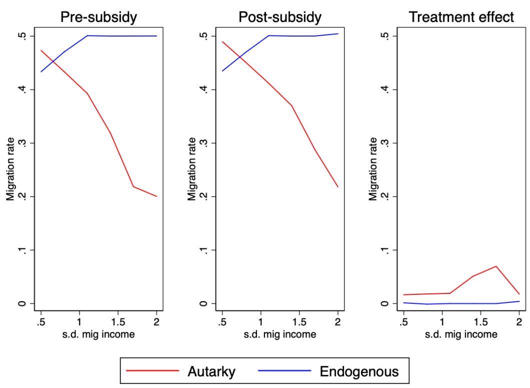


(c) Risk-sharing coefficient

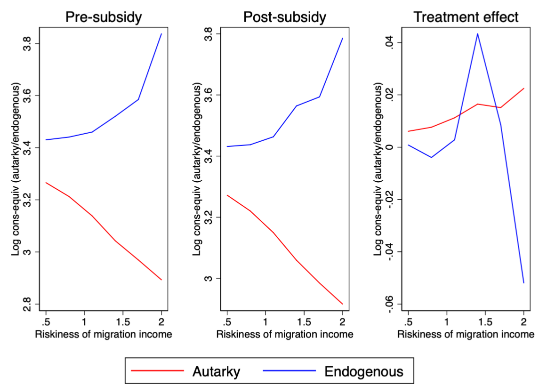


Appendix Figure 6: Financial subsidy (lognormal income distribution in village and city)

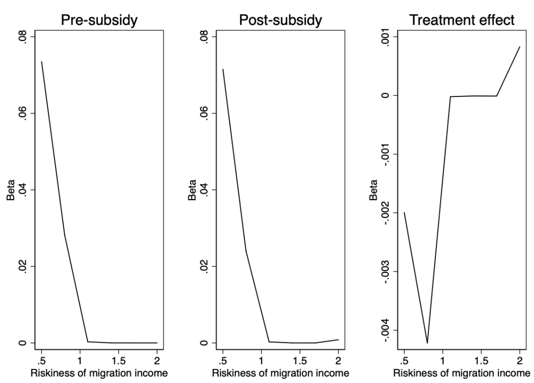
(a) Migration



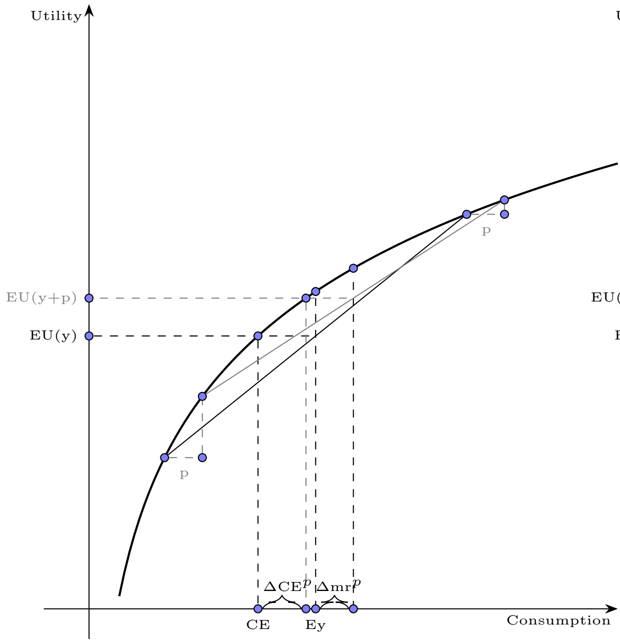
(b) Cons-equivalent value



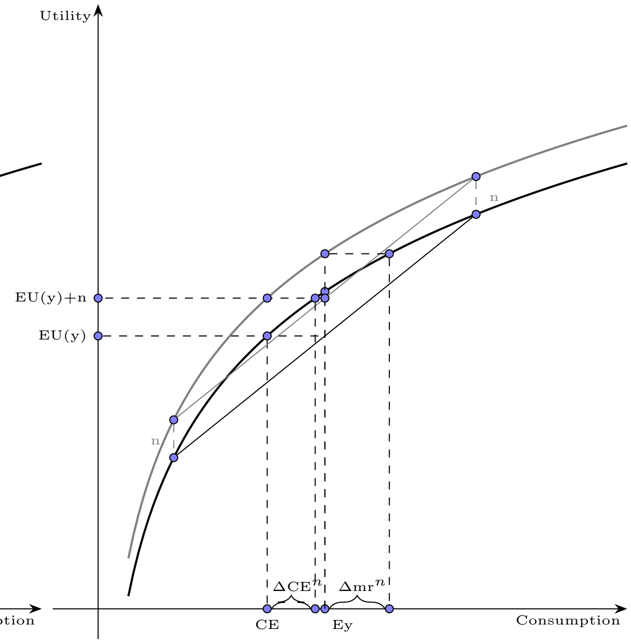
(c) Risk-sharing coefficient



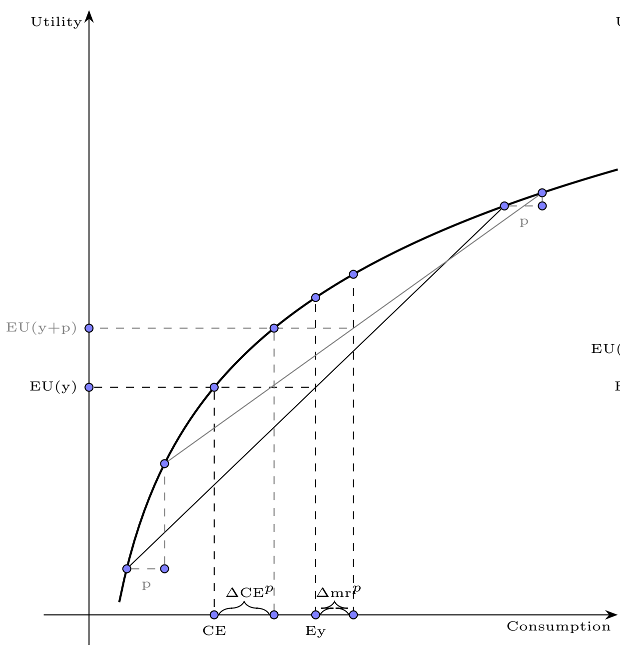
Appendix Figure 7: Certain and average values of financial and utility subsidies



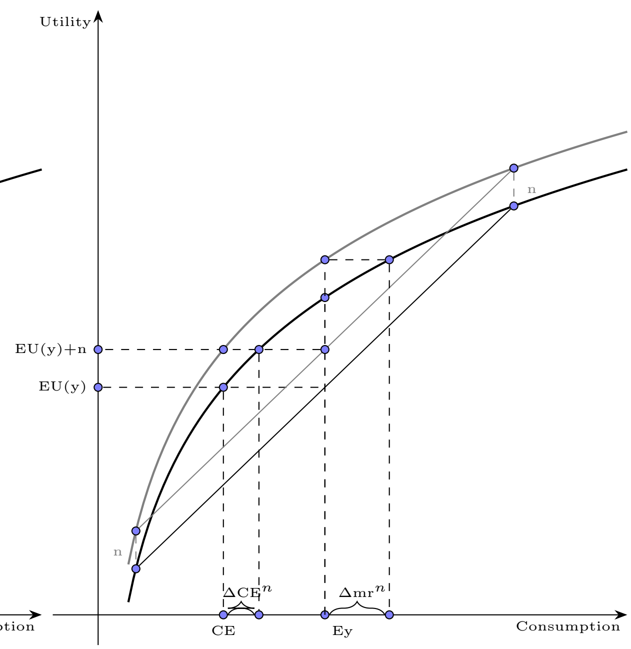
(a) Financial subsidy, small variance



(b) Utility subsidy, small variance



(c) Financial subsidy, large variance



(d) Utility subsidy, large variance

Note: All panels have mean income of 3.0. Panels (a) and (b) have min/max income of 1/5, while panels (c) and (d) have min/max income of 0.5/5.5. The financial subsidy is 0.5 and the utility subsidy is 0.15 utils.

B Appendix Tables

Appendix Table 1: Experimental design and data collection timeline

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

Note: Income in Rounds 1 and 4 span the previous 12 months. Income in round 2 spans the previous 4 months. Income data was not collected in round 3. Migration information was not collected in round 1.

Appendix Table 2: Treatment effect on transfers within the community, by migration status

	All households		Migrant households		Non-migrant households	
	Treatment effect	Control mean	Treatment effect	Control mean	Treatment effect	Control mean
Willingness to help						
Community member would help you	0.030	0.85	0.045	0.85	0.032	0.85
	(0.020)		(0.030)		(0.022)	
... and you would ask for help	0.025	0.83	0.034	0.83	0.031	0.83
	(0.020)		(0.031)		(0.023)	
Community member would ask you for help	0.109***	0.57	0.140***	0.59	0.091**	0.56
	(0.033)		(0.043)		(0.037)	
... and you would help them	0.109***	0.53	0.129***	0.55	0.094***	0.51
	(0.032)		(0.044)		(0.035)	
Actual transfers						
Receive any transfer from community member	-0.024	0.57	-0.003	0.55	-0.015	0.59
	(0.022)		(0.036)		(0.028)	
Amount, if any transfer received (Tk)	1821***	4808	648	4169	3175	5185
	(678)		(593)		(1108)	
Give any transfer to community member	0.036**	0.15	0.049	0.17	0.028	0.14
	(0.018)		(0.031)		(0.020)	
Amount, if any transfer given (Tk)	1310	2001	816	1984	1937*	2014
	(558)		(677)		(1024)	

Note: The sample includes households from the 2011 survey. Columns 1 and 2 contain all households, columns 3 and 4 contain migrant households, and columns 5 and 6 contain non-migrant households. Each cell is a separate regression of the effect of treatment on whether the source denoted in the row would behave as described. Each regression also controls for upazila (county). Standard errors, clustered by village, are in parentheses, and the mean of the control group is in the second column of each set. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 3: Effect of migration incentives on consumption smoothing, alternative samples

	Log total consumption								
	Main sample			Controls in later experiment			Non-migrant households		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log income x treatment	-0.073***			-0.134***			-0.081**		
	(0.028)			(0.047)			(0.035)		
Treatment group restrictions									
Log income x unassigned group		-0.084**			-0.136**			-0.098**	
		(0.036)			(0.053)			(0.041)	
Log income x self-formed group		-0.022			-0.011			-0.004	
		(0.034)			(0.061)			(0.046)	
Log income x assigned group		-0.106***			-0.254***			-0.129***	
		(0.037)			(0.057)			(0.048)	
Treatment destination restrictions									
Log income x unassigned destination			-0.067**			-0.126**			-0.073
			(0.031)			(0.060)			(0.046)
Log income x assigned destination			-0.082**			-0.143**			-0.089**
			(0.033)			(0.052)			(0.040)
Observations	1857	1857	1857	497	497	497	1142	1142	1142
R-squared	0.185	0.187	0.186	0.170	0.190	0.180	0.224	0.229	0.225

Note: The sample in columns (1)-(3) is our main sample of households from the 2011 survey, the sample in columns (4)-(6) includes households from the 2011 survey that were in control villages in the 2011 treatments, and the sample in columns (7)-(9) include non-migrant households from the 2011 survey. The dependent variable is log of annual per-capita total consumption. The main independent variable is log of annual per-capita income, interacted with the respective treatment variable. All models control for village fixed effects, and columns (1)-(3) and (7)-(9) also control for log income interacted with 2011 treatment arms. Standard errors, clustered by village, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 4: Effect of migration incentives on consumption smoothing, different household size assumptions

	Log total consumption		
	Size: past 7 days (main specification)	Size: current	Size: past 14 days
	(1)	(2)	(3)
Log income x treatment, size: past 7 days	-0.073***		
	(0.028)		
Log income x treatment, size: current		-0.039	
		(0.043)	
Log income x treatment, size: past 14 days			-0.086**
			(0.042)
Observations	1857	1862	1874
R-squared	0.185	0.137	0.142

Note: The sample includes households from the 2011 survey. The dependent variable is log of annual per-capita total consumption. The main independent variable is log of annual per-capita income, interacted with treatment. The definition of household size used to calculate per capita terms is the total number of household members present in at least the past 7 days at the time of the interview in column (1), the total number of household members currently present at the time of the interview in column (2), and the total number of household members present in at least the past 14 days at the time of the interview in column (3). All models control for log of income interacted with 2011 treatment arms and village fixed effects. Standard errors, clustered by village, are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 5: Effect of migration incentives on consumption smoothing, difference-in-difference specification

	Log total consumption		
	(1)	(2)	(3)
Treat × post × Log income	-0.104**	-0.0852	-0.133***
	(0.040)	(0.062)	(0.047)
Village-round FE?	Yes	Yes	Yes
Household FE?	No	Yes	No
Observations	3208	2178	2178
R-squared	0.462	0.791	0.509

Note: The sample includes households from the baseline 2008 survey (round 1, except migration income, which is round 2) and the 2011 survey (round 4) The dependent variable is log of annual per-capita total consumption. The main independent variable is log of annual per-capita income, interacted with treatment and an indicator for the 2011 (post-treatment) survey. All models control for village fixed effects and all other interactions between treatment, post, and log income. Column (2) additionally controls for household fixed effects. Column (3) reestimates the specification in column (1) with the sample in column (2). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 6: Robustness over dynamics and utility shock

	(1) Data	(2) Full model	(3) No mig asset	(4) No utility shock
Targeted moments				
Risk sharing (control)	0.16	0.20	0.20	0.81
Risk sharing (treatment effect)	-0.073	-0.040	-0.041	-0.011
Mean migration rate	0.39	0.64	0.64	0.60
Mig. treatment effect (during RCT)	0.22	0.20	0.20	0.21
Mig. treatment effect (after RCT)	0.094	0.14	0.14	0.024
Migrate neither during/after RCT (control)	0.49	0.12	0.12	0.18
Migrate during and after RCT (control)	0.23	0.40	0.40	0.38
Migrate neither during/after RCT (treatment effect)	-0.17	-0.067	-0.059	-0.015
Migrate during and after RCT (treatment effect	0.15	0.15	0.16	0.020
Mean home income (migrant)	1.80	0.56	0.56	0.95
Std. home income (migrant)	0.67	0.38	0.38	0.31
Mean home income (nonmigrant)	2.13	0.79	0.79	1.54
Std. home income (nonmigrant)	0.56	0.33	0.33	0.28
Mean income of migrants (nonmigrant prior pd.)	0.079	0.11	0.21	-0.23
Mean income of migrants (migrant prior pd.)	0.24	0.21	0.20	-0.017
Std. income of migrants (nonmigrant prior pd.)	0.67	0.98	0.96	0.89
Std. income of migrants (migrant prior pd.)	0.70	0.95	0.95	0.88
Estimated parameters				
CRRA parameter		2.10	2.10	1.79
Opportunity cost of migration		0.10	0.10	0.14
Utility cost of migrating		0.031	0.031	0.048
Utility subsidy		0.040	0.040	
Decay rate of utility subsidy		0.41	0.41	
Mean home income		0.71	0.71	1.28
Std. home income		0.39	0.39	0.39
Mean city income with contact		0.21	0.21	0.12
Std. city income with contact		1.07	1.07	0.97
Prob. get contact		0.96		0.66
Prob. lose contact if migrate		0.47		0.19
Prob. lose contact if don't migrate		0.63		0.65
Set on grid				
Discount factor		0.70	0.70	0.70
Model criterion				
Model criterion		10.3	12.8	272.4

Notes: Utility shock, if estimated, decays over 3 periods. Financial variables measured in annual log (thousands of Taka).

Appendix Table 7: Robustness over discount factor and subsistence

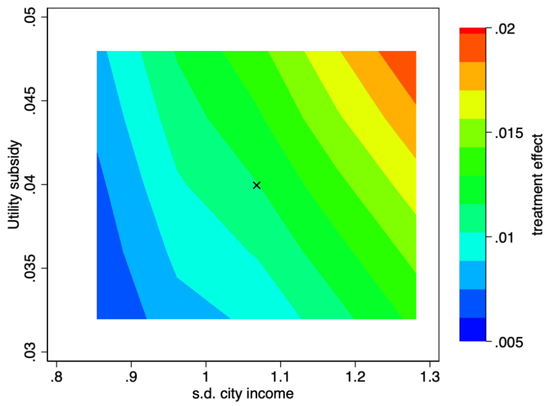
	(1) Data	(2) $\beta = 0.7$	(3) $\beta = 0.8$	(4) $\beta = 0.9$
Risk sharing (control)	0.16	0.20	0.22	0.072
Risk sharing (treatment effect)	-0.073	-0.040	-0.048	-0.013
Mean migration rate	0.39	0.64	0.72	0.34
Mig. treatment effect (during RCT)	0.22	0.20	0.18	0.21
Mig. treatment effect (after RCT)	0.094	0.14	0.15	0.14
Migrate neither during/after RCT (control)	0.49	0.12	0.071	0.44
Migrate during and after RCT (control)	0.23	0.40	0.50	0.11
Migrate neither during/after RCT (treatment effect)	-0.17	-0.067	-0.052	-0.11
Migrate during and after RCT (treatment effect	0.15	0.15	0.24	0.072
Mean home income (migrant)	1.80	0.56	0.79	1.34
Std. home income (migrant)	0.67	0.38	0.43	0.46
Mean home income (nonmigrant)	2.13	0.79	0.87	1.05
Std. home income (nonmigrant)	0.56	0.33	0.35	0.48
Mean income of migrants (nonmigrant prior pd.)	0.079	0.11	-0.071	0.062
Mean income of migrants (migrant prior pd.)	0.24	0.21	0.088	0.17
Std. income of migrants (nonmigrant prior pd.)	0.67	0.98	0.87	1.10
Std. income of migrants (migrant prior pd.)	0.70	0.95	0.85	1.09
Estimated parameters				
CRRA parameter		2.10	1.62	1.61
Opportunity cost of migration		0.10	0.080	0.0099
Utility cost of migrating		0.031	0.046	0.060
Utility subsidy		0.040	0.042	0.040
Decay rate of utility subsidy		0.41	0.18	0.49
Mean home income		0.71	0.87	1.16
Std. home income		0.39	0.43	0.52
Mean city income with contact		0.21	0.18	0.19
Std. city income with contact		1.07	0.92	1.22
Prob. get contact		0.96	0.77	0.87
Prob. lose contact if migrate		0.47	0.29	0.25
Prob. lose contact if don't migrate		0.63	0.64	0.78
Set exogenously				
Discount factor		0.70	0.80	0.90
Model criterion				
Model criterion		10.3	17.1	18.2

Notes: The cost to consume a subsistence level of consumption, estimated to be 600 calories per day, is estimated to be 250 Taka/month (3000 Taka annually). Because the model is measured in income, not consumption, units, we set the subsistence level to 1000 Taka instead of 3000 Taka to match the proportion of people below subsistence as measured by income with the proportion of people below subsistence as measured by consumption.

C Appendix Figures

Appendix Figure 8: Local derivative of relative welfare and migration treatment effects

(a) Relative welfare gain (risk sharing minus autarky)



(b) Migration

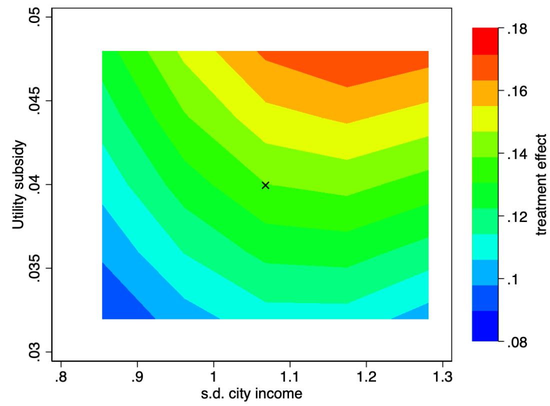
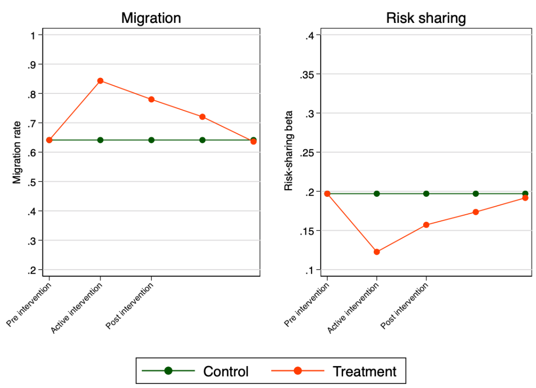


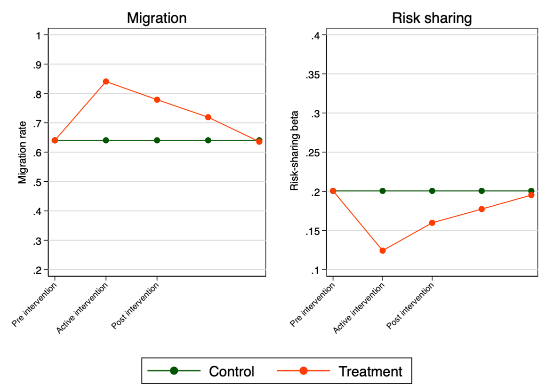
Figure shows simulated data for post-intervention period.

Appendix Figure 9: Dynamics of migration subsidy: Robustness

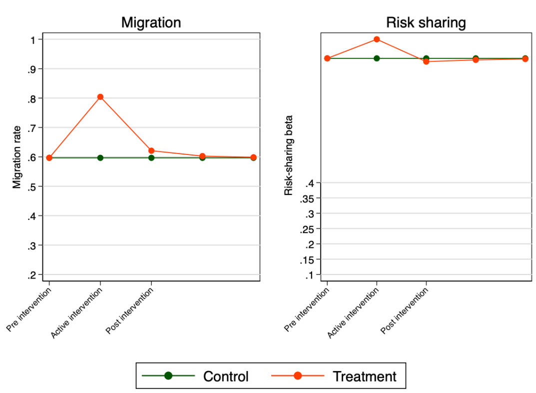
(a) Mig asset ✓; utility sub. ✓



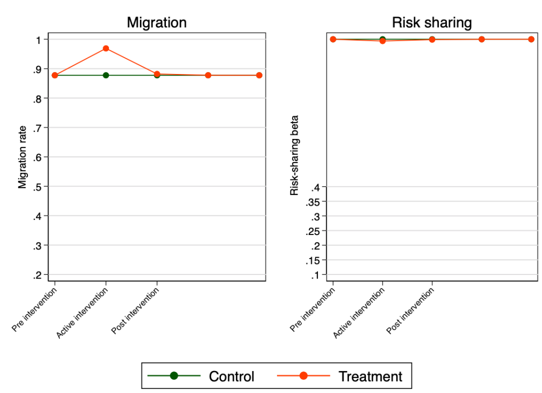
(b) Mig asset X; utility sub. ✓



(c) Mig asset ✓; utility sub. X

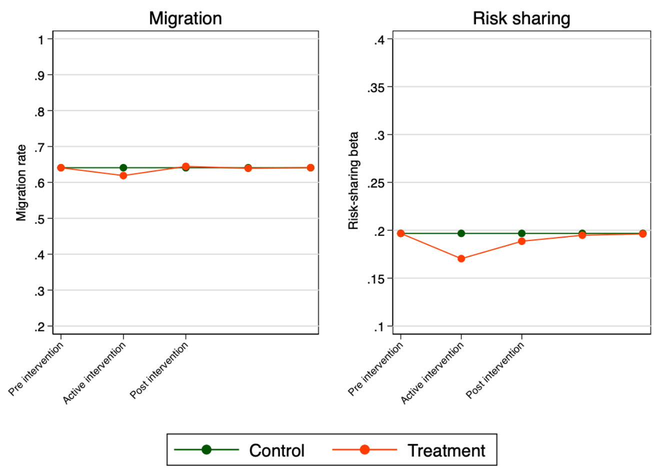


(d) Mig asset X; utility sub. X

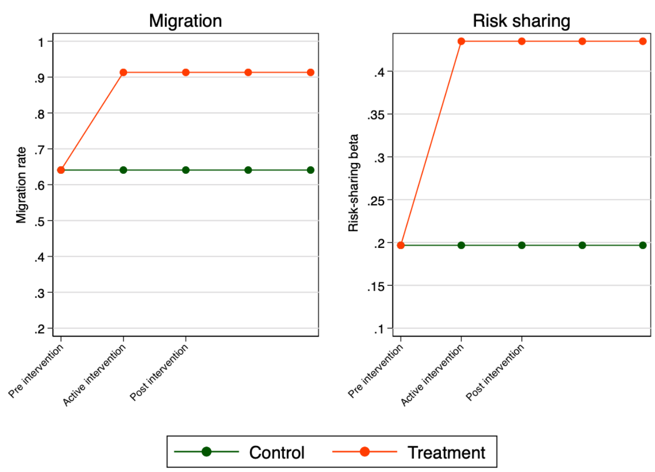


Appendix Figure 10: Temporary vs permanent shock (financial component only)

(a) Temporary



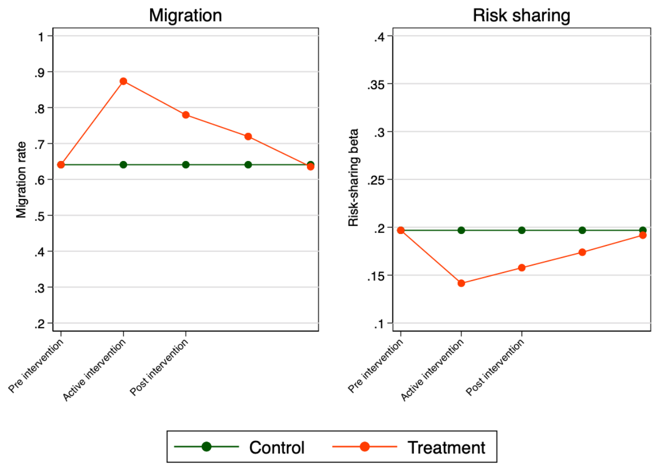
(b) Permanent



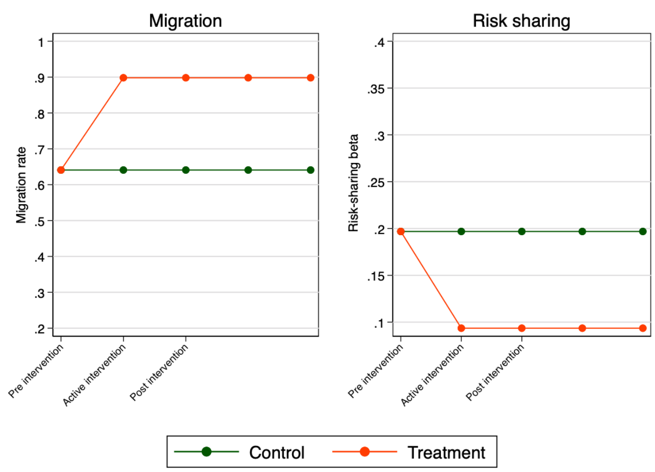
Note: Figure shows the financial component only.

Appendix Figure 11: Temporary vs permanent shock (utility component only)

(a) Temporary



(b) Permanent



Note: Figure shows the utility component only.