Submitted to *Quantitative Economics*

# Remittance Frictions and Seasonal Poverty

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Seasonal migration is a common strategy to mitigate rural seasonal deprivation, but migrants need to remit money during the lean season to family members facing food shortages. We observe counter-intuitively low remittances in rural Nepal during periods of seasonal hunger, and migrants return with remittances later during harvest when food is relatively abundant. To indirectly overcome this appar-

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We thank Caglar Ozden, Imran Rasul, Dean Yang, Kelsey Jack, Anant Nyshadham, Lauren Bergquist, Achyuta Adhvaryu, and seminar participants at the University of Michigan, UCL/LSE, World Bank/DECRG, Chicago-Booth, NOVA-Lisbon, NYU-Abu Dhabi, the Urban Economics Association Annual Conference, Yale-Cowles Foundation, and University of Colorado-Boulder for helpful comments. Priyankar Chand provided excellent research management. Naia Nathan provided excellent research assistance. We thank Bishal K. Chalise for his support. This work was supported by research grants from the IZA/FCDO GLM-LIC program (Project 4-442), the U.S. National Science Foundation (Award 2149418), and Open Philanthropy. The research protocol was approved by Yale University IRB (Protocol No. 2000025621). The trial was registered in the AEA RCT Registry (#5866).

ent constraint in remittance timing, we provide a $90 consumption loan to randomly selected rural households during the pre-harvest lean season. Loan-recipient households increase pre-harvest investments in fertilizer and time spent working on their own farm, smooth consumption, and save more of their migration income to bring it back home. Food security, subjective well-being, rice harvest, and revenues improve. Ninety-eight percent of beneficiaries repay the loan with the increased harvest-period remittance. In a two-period model of household decision-making, we show that remittance frictions—a market failure—are necessary to qualitatively match our experimental results.

KEYWORDS. Remittance frictions, Seasonal migration, Nepal, Seasonal poverty.

JEL CLASSIFICATION. F24, O15.

## 1. INTRODUCTION

Seasonal poverty is ubiquitous in rural, agrarian areas of low-income countries. Some 600 million people experience seasonal hunger (Devereux et al. 2013). Labor demand and wages fall during pre-harvest lean seasons, and this often coincides with elevated prices for staple goods (Bryan et al. 2014). The combination of low wages and high prices undermines caloric intake, as poor households find it difficult to smooth consumption (Khandker 2012, Dostie et al. 2002, Basu & Wong 2015). Households could use credit or savings to inter-temporally smooth consumption (Fink et al. 2020), but moneylender interest rates are high and strong network sharing norms undermine savings (Jakiela & Ozier 2016). Some households spatially diversify by sending migrants to urban areas with better employment prospects during the lean season (Akram et al. 2017). Given the temporary nature of the migration, the high cost of living in cities, and the simultaneous need to manage the farm at home, typically the male household head migrates while his wife and children remain behind in the deprived rural area (Mobarak & Reimão 2020). Addressing seasonal deprivation therefore requires remitting the migrant's earnings back to the rural area during the lean season.

Our research setting is two rural districts in Nepal's western *terai*, where 79% of households send a migrant to cities in Nepal or to India to cope with seasonal deprivation. We collect high-frequency data on the remittance behavior of these Nepali circular migrants and find that remittances are curiously *pro*-cyclical with respect to the crop cycle at home. Migrants appear unable to remit money during the agricultural lean season, when their family members in rural Nepal need financial support the most.

We provide a lean-season consumption loan to those rural family members to indirectly relieve this apparent remittance constraint. We document using a randomized controlled trial that migrant households respond by smoothing consumption, increasing agricultural investment, and increasing harvest-period remittances (with which they repay the loan), without any change in migration income. We infer – by combining these empirical results with a two-period of household consumption, investment and circular migration – that this *remittance friction* appears to be an important market failure that perpetuates seasonal deprivation and food insecurity.

The fact that remittance receipts are lowest when they are needed most - during the pre-harvest "lean season" in rural areas, and highest during the harvest when rural households enjoy greater agricultural incomes – is contrary to the dominant finding in the literature that formal remittances are counter-cyclical (Yang & Choi 2007). This surprising pattern in our data is driven entirely by informal remittances brought back by hand, which typically remain unmeasured in other studies. These remittances arrive only when the circular migrant returns to the village during the harvest. Their return coincides with the harvest – a phenomenon also observed in Bangladesh (Bryan et al. 2014) – because that is when the marginal product of labor rises in the village. Evidently, some migrants find it difficult or expensive to send remittances earlier electronically or travel back to bring the money to their families during the lean season while they are working in India. Other researchers who have collected detailed cost data in Bangladesh find that remitting through such informal mechanisms costs 20-25% of the remit-

tance amount, and are 10 times more expensive than mobile banking transfers (Lee et al. 2021).[1]

Our lean season interest-free loan of approximately $90 USD is designed to give rural household members access to the migrant's destination earnings earlier. So we require repayment four months later when migrants typically return with remittance income. Return migration, income, and consumption are predictably seasonal in our sample - all three peak around the time of rice harvest. Migrant households who receive the offer respond to this intervention as if they face constraints on the *timing* of remittance receipts in the village - increasing pre-harvest investments in fertilizer and time working on their own farm, smoothing consumption, and saving more of their migration income to bring it back home. Loans improve food security and subjective well-being during the lean season, particularly for women who made up the majority of loan recipients. The agricultural investments result in increased rice output and sales during the harvest. Loans increase total remittances received by the household, driven entirely by informal remittances brought back by hand during harvest, with no change in formal electronic remittances or remittances during the lean season. Migrants report increased savings but no change in earnings.

We present a two-period model of household investment, consumption, migration, and remittance behavior to explain how the loan indirectly alleviating remittance frictions can rationalize these treatment effects. The model shows that total remittances would increase in response to such a loan offer *only if* migrants find it difficult or expensive to remit money earlier during the lean season, when the consumption/utility value of those funds would be much higher at home than at the migration destination.

The leading alternative hypothesis is that the loan increases remittances due to a change in the intra-household bargaining power of the non-migrant family

[1] Bryan et al. (2014) notes that internal migrants in Bangladesh make multiple trips within the same lean season. They return home with remittance in the middle of the season and travel again, which is costly. For Nepali migrants, traveling to and from India would be even more expensive.

members (typically the wife and children) who receive the funds. We conduct additional surveys and experiments to test the plausibility of this alternative theory and don't find any significant difference in preferences over the timing of remittances between migrant and non-migrant family members. Intra-household issues appear to be an unlikely explanation for the experimental results. Likewise, the fact that loans do not increase digital or lean-season remittances, but do cause a large increase in remittances by hand and at harvest, is more consistent with remittance frictions than with intra-household mechanisms. Ultimately our experiment identifies the effect of a lean-season loan and we are making an indirect inference about the remittance constraint; we did not conduct any intervention to alleviate the remittance friction directly.

Our first contribution is to identify this remittance constraint as a market failure that exacerbates the problem of seasonal poverty in rural areas of developing countries. Others have documented that seasonality can reduce agricultural investments (Duflo et al. 2011), force inefficient early crop sales (Burke et al. 2019, Dillon 2021), distort labor allocation (Fink et al. 2020), and undermine child development (Christian & Dillon 2018).

Second, we show that easing the remittance transfer process for migrants can produce productivity and welfare gains. The potential gains globally are large, in that many other economies beyond Nepal are heavily dependent on international migration and remittances, including Philippines, Uzbekistan, Mexico, and Pakistan. 3.5% of the world's population are international migrants (Ratha et al. 2022), and they remitted US$ 794 billion in 2022. Remittances are the largest documented financial flow into low- and middle-income countries (LMICs), larger than all foreign direct investment and four times the size of all official development aid (Ratha et al. 2022). Frictions that impede migration and remittances can have large economic and social costs. The issue is potentially very significant in Nepal, because (officially measured) remittances account for 22% of Nepali GDP (World Bank 2022). Actual remittances are likely even higher since the most common form of migration is across the open land border with India,

and remittances along this corridor often remain unmeasured because migrants travel back with the money in hand without using bank transfers.[2]

Third, we identify a specific distortion in remittance timing. The existing literature finds formal remittances are generally counter-cyclical, mitigating the impacts of aggregate shocks (Calero et al. 2009, Yang & Martinez 2006). We find that informal remittances carried by hand – which are typically not tracked in official data – are procyclical. It is important to document the characteristics of informal remittances, because up to 40% of international remittances to developing countries are estimated to be informal (Freund & Spatafora 2005). Internal migrants in LMICs frequently remit by hand,[3] and internal migration is significantly more common than migrants crossing international borders (Lucas et al. 2015). Relatedly, Lee et al. (2021), Batista & Vicente (2020), Riley (2018), and Jack & Suri (2014) show that introducing mobile banking produces analogous benefits in risk sharing, consumption, and resilience to shocks.

Fourth, we relate to the literature on spatial market integration and the incidence of aggregate rural shocks (Jayachandran 2006). Many papers focus on spatial integration of labor markets (Brooks & Donovan 2020) and commodity markets (Aker 2010, Abay & Hirvonen 2017). Our results imply that easing the timing of financial transfers can also aid market integration. We further show that a loan product can be creatively designed to indirectly reduce remittance frictions even in places where mobile banking and remittance technologies are absent.

[2] Over 37% of Nepali migrants travel to India. Some estimates suggest over 90% of that remittance is informal (International Organization for Migration 2019). The open border and India's geographic proximity make this circular migration common. In our data, most migrants from study villages travel to India, remit informally by hand, and 92% of migrants to India return home at least once per year. In that dimension, the migration we study is more akin to the internal seasonal migration studied by Bryan et al. (2014), Meghir et al. (2022), and Akram et al. (2017), than to the multi-year guest-worker visas studied by Clemens & Tiongson (2017), McKenzie et al. (2010), Mobarak et al. (2023), or Naidu et al. (2022).

[3] For instance, in the median of five sub-Saharan African countries, countries, 74% of internal migrants and 55% of within-Africa migrants remit primarily by hand (Plaza et al. 2011).

Fifth, we contribute to the literature on the set of overlapping market failures that create seasonal deprivation. Other papers have examined the importance of liquidity (Lee & Sawada 2010) and credit constraints (Fink et al. 2020, Basu & Wong 2015, Stephens & Barrett 2011), and savings constraints such as poor storage (Burke et al. 2019, Aggarwal et al. 2018, Brander et al. 2021). We introduce yet another market failure: remittance frictions can create seasonal shortages in contexts where households engage in migration to smooth consumption. Our loan product is most closely related to the consumption loans provided by Fink et al. (2020). But theirs is a context without much migration, and their loans instead reduce labor misallocation in Zambia.

Finally, we relate closely to the literature on seasonal migration as a solution to seasonal poverty (Bryan et al. 2014, Akram et al. 2017). This Nepal experiment qualifies this link: seasonal migration reduces seasonal poverty only if migration income can be used at home *during* the lean season. With the remittance friction we document, household members remaining behind in rural areas can experience deprivation even if a member migrates and earns income in the city during that season.

The remainder of this paper is organized as follows: we discuss our sampling frame and data collection in section 2, descriptive characteristics of our study context in section 3, and the details of our field experiment in section 4. We then introduce a model of household decision-making with remittance frictions and derive predictions that are testable in our data in section 5. We present our experimental treatment effects and compare them to model predictions in section 6. In section 7 we discuss an alternative theory, that loans increase remittances through an increase in non-migrant bargaining power, and evaluate it using additional data and a second experiment. In section 8 we discuss why the market does not already address the remitttance friction and people's consumption smoothing needs. In section 9 we conclude.

## 2. Sampling and Data Collection

### 2.1 *Sampling Frame*

Our sample for this study is drawn from 15 of the 17 sub-districts within the districts of Kailali and Kanchanpur in the western Terai (plains) region of Nepal. We identified 73 wards in these sub-districts where the NGO partner who would implement the intervention had the capacity to operate, and from these we randomly sampled 30 wards in which we would conduct the study. We then listed the full set of 303 villages in these wards, and randomly sampled 97 villages. We chose 97 to achieve our desired sample size given our estimates of the number of eligible households in each village. We sampled villages stratifying to include either 3 or 4 villages in each ward, so that we would have coverage over all 30 wards in our experiment. During baseline data collection, we were forced to drop 7 villages from the study because they were inaccessible due to flooding. These villages were dropped from the entire study, leaving us with 90 villages in our study sample.

### 2.2 *Eligible Population*

We wished to target our intervention to rural households facing acute seasonal deprivation, and in practice targeted the bottom half of the wealth distribution. In this sense, our study follows Bryan et al. (2014) more closely than Fink et al. (2020) or Basu & Wong (2015) which were largely un-targeted within villages. We excluded wealthier households in part because these households are more likely to have access to savings and credit and therefore less likely to have binding liquidity constraints. In our model, these overlapping constraints are necessary for remittance frictions to bind. In addition, there were practical constraints to including wealthy households - our NGO partner advised us that wealthy households receiving loans would be seen as unfair, and that randomizing loans within poorer households would have more support amongst villagers.

To this end, we listed all households in each study village and conducted a participatory wealth ranking exercise (PWR) in each village. The PWR involved gath-

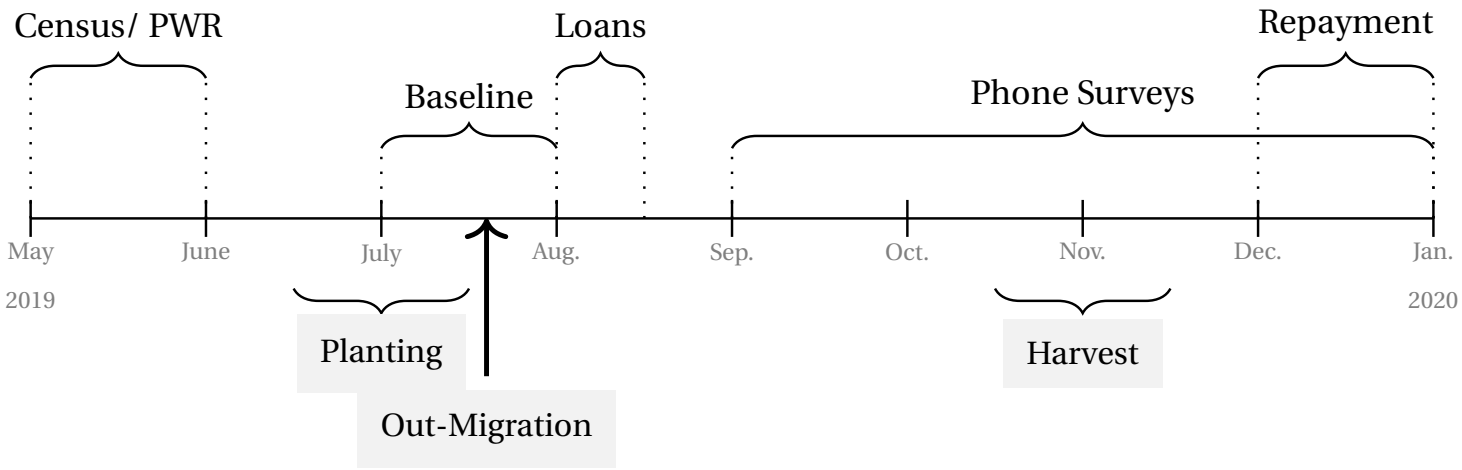
ering a group of 5-12 knowledgeable members of the village, including any traditional village leaders, and developing a set of criteria for placing households into one of four wealth categories. The wealth ranking exercises were facilitated by researchers from our data collection partner, but the criteria for each category were developed independently in each village. Common criteria included threshold amounts of agricultural land holdings, whether someone in the household had a salaried job and/or government job, and the quality of their home. After the completion of this exercise, we selected our intervention sample from the bottom half of the wealth distribution within each village. Our census and wealth-ranking exercises took place in May, 2019. Figure 1a shows the full timeline of our study activities.

While our study wards should represent the majority of rural wards in our study districts, it is worth noting that our sample differs from the population of Nepal for a number of reasons. Our sample is entirely rural. The two districts in our study, Kailali and Kanchanpur, are in the Terai ecological region, with higher density and lower poverty rates than the Mountains or Hills. Our study districts and study sample also have a higher population of "non-caste" indigenous groups than rural Nepal as a whole. Finally, perhaps due to selection into loans, our study sample is significantly more likely to be growing main paddy rice than other rural households in Nepal and the study province: 85% of our sample compared to 63% of rural households in our study province and 56% of rural households in Nepal are growing main paddy rice. These differences can be seen in Supplementary Table 8 (Mobarak et al. 2026), which compares our sample characteristics to those of Nepal using data from the fourth National Living Standards Survey (NLSS-IV) (National Statistics Office (NSO), Nepal 2022).

### 2.3 *Baseline Survey*

We conducted in-person baseline surveys in July, 2019. In addition to basic demographic information, the survey included questions on food security, migration experience, future migration plans, and rice cultivation. Our final sample included 2,037 households in treated villages and 899 households in control vil-

(a) Timeline of Study




(b) Flowchart of Sampling and Experimental Design

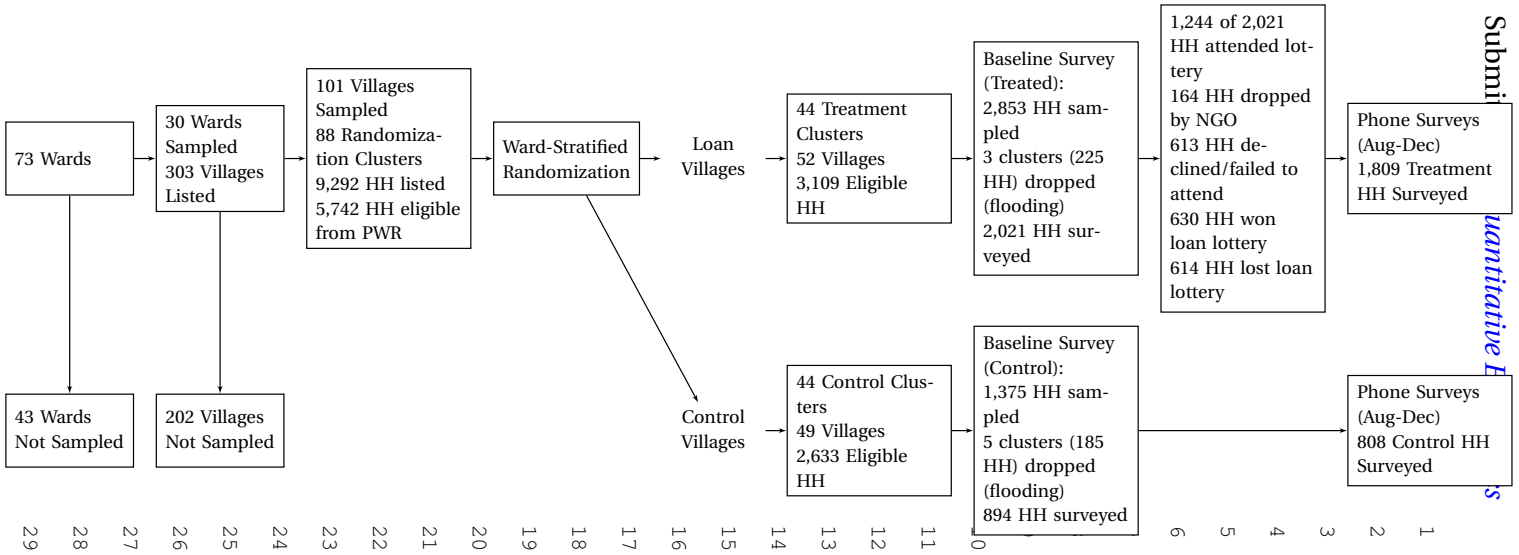



FIGURE 1. Timeline of Activities and Flowchart of Study Design

lages. Given household migration in between census and baseline and tracking challenges due to rice planting activities and the monsoon, we experienced attrition between the census and baseline surveys. We discuss the implications of this attrition in more detail in section 4.4.

### 2.4 *Phone Surveys*

The loan intervention was implemented after the completion of the baseline (see Figure 1a). In the post-intervention period, we conducted five rounds of phone surveys over four months, beginning in late August 2019 and finishing early January, 2020. Response rates to these phone surveys were high: 87% overall and above 85% in every round. Section 4.4 discusses phone survey attrition rates by the experimental arm in more detail. The questions administered varied by survey round. Every survey round asked about labor supply and subjective well-being. Rounds 1, 2, 4 and 5 asked about food security. Rounds 2, 3, 4 and 5 asked about remittance receipts since the time of the previous survey. Rounds 3 and 4 inquired about the migration experience in greater detail because these rounds coincided with the period when most migrants return to the village, and that allowed us to query the migrant directly. Round 2 asked about agricultural input investments, and round 5 asked about agricultural outcomes such the amounts of crop harvested and sold.

### 2.5 *Outcomes Measured*

We measure the effects of the intervention on agricultural investment and agricultural harvest outcomes, non-agricultural labor at home, migration and remittances, food security, and subjective well-being. We measure agricultural investments in terms of (a) hours spent by family members working on the household's own farm, (b) fertilizer purchased, (c) hired non-family farm labor, and (d) pesticides and other purchased inputs. We also add up the total value of these investments. We also measure the number of hours spent by family members on wage work in the village, wage work away from the village but within the same district (local migration), and wage work outside the district.

We use self-reports over the phone to measure the total value of rice harvested, as well as the amount of rice stored and the amount sold at harvest time. We measure agricultural outcomes over the phone instead of in-person to stay within budget, given that we were already using high-frequency phone surveys to measure seasonal variation in other outcomes. Recent research (Anderson et al. Forthcoming) suggests that phone surveys lower statistical precision but treatment effect estimates in agriculture remain consistent across phone and in-person surveys.

All surveys were conducted with household members currently at home in the village. One concern may be that remittances reported by non-migrant household members could be misreported, relative to those reported by migrants. On the one hand, household members at home may not recall all remittance receipts, making remittances subject to negative recall bias. On the other hand, migrants may over-report remittances due to social-desirability bias. Misreporting is likely to reduce variation in remittances, and that can create attenuation bias in our estimates. In Supplementary Table 9 (Mobarak et al. 2026) we check whether self-reported remittances correlate with other objective metrics of migration behavior (e.g. destination choice) in sensible ways, and they do.

During every round of the phone survey, we obtain a report of remittances received by the non-migrant household members. The remittance data is collected for every migrant who is still away or who has recently returned. We ask if the remittances were sent via bank, international money exchange, or carried back by hand by the migrant or sent through a friend. This allows us to construct separate measures of remittances received during the lean season, during harvest prior to loan collection, and after loan collection. We can also examine treatment effects separately for remittances by hand and by bank.

We included questions in our phone surveys related to subjective well-being and food security. After collecting data, we discovered that several of the questions produced little variation within our sample. For example, almost nobody goes a whole day without eating, and very few miss meals entirely. We therefore *ex-post*

select a subset of these items to construct our food security and well-being indices by examining how well that measure is correlated with the lean versus harvest seasons *using data from control villages only*. The logic is that the lean season is well-known to be a period of need, and that known seasonal variation allows us to validate these indicators. An overall index constructed using the first principal component weightings of all food security and subjective-wellbeing items we measure is also strongly associated with the lean season - 0.21 cross-sectional standard-deviations lower during that period ($p < 0.001$). Reassuringly, selected variables have uniformly higher $R^2$ with an index of socioeconomic status (SES) than non-selected variables, despite SES not being used for variable selection. As validation of our overall selection strategy, the correlation between the T-statistic with the lean season and $R^2$ with SES across the full set of variables is 0.84.

Appendix figure **??** shows these correlations and the predictive power of each indicator to identify the pre-harvest lean period, which we used to select variables, and an SES index, which we did not use. Our preferred food-security index uses questions that ask about (a) worrying about running out of food (b) eating less preferred foods, and (c) reducing portion sizes, and omits questions on the number of meals eaten, skipping meals, and going an entire day without eating. The less severe forms of deprivation are more correlated with the lean season in the Nepali agricultural calendar than the more severe measures such as not eating at all, because the latter are (thankfully) relatively rare occurrences in this context. Our preferred psychological well-being index uses questions on the (a) quality of sleep, (b) frequency of anxious thoughts, and (c) frequency of feeling depressed, and omits questions on overall life satisfaction. We use this same subset of questions to measure food security and well-being consistently in all other research that analyzed data from these same surveys to track the effects of the COVID pandemic in Nepal (Egger et al. 2021, Aksunger et al. 2023, Barker et al. 2020).

## 3. Stylized Facts about the Context

The typical household in our study villages is an agricultural household that sends a migrant – often the adult male household head – to India for seasonal

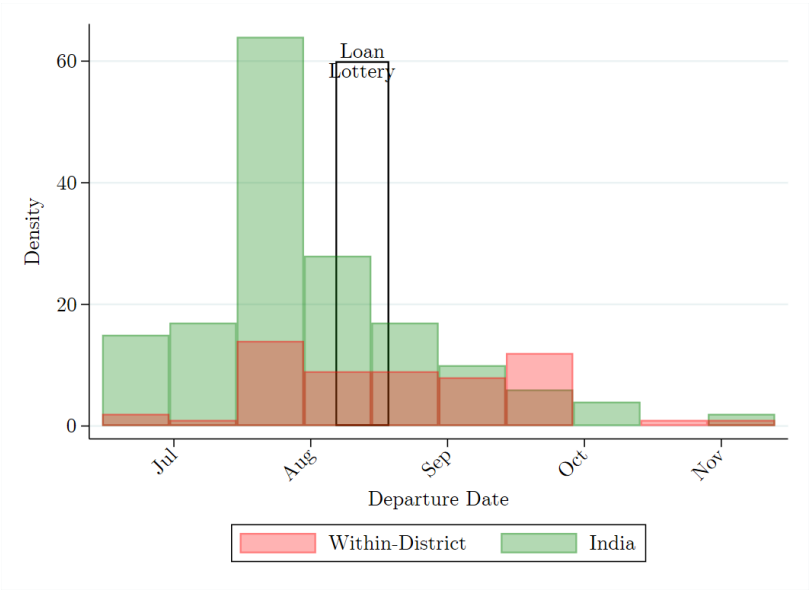



FIGURE 2. Migration Departure Dates Relative to Loan Timing

work to diversify their income sources. 80% of households in our data are engaged in self-employed agriculture, and 72% of households in our control village had a prime-age male migrant away between August and January.

The migrant often travels after planting season in July (see Figure 1a), and stays away during the entire "pre-harvest lean period" at home. Rice planting – the most labor-intensive agricultural activity – occurs in July, and many migrants therefore stay home at this time. Out-migration rates peak immediately after rice planting is finished (see Figure 2). Rice harvest occurs in November near the time of important religious holidays, and this is when most migrants return from India.

Migration timing is therefore closely linked to agricultural crop cycles and seasonal food insecurity. Due to the dominance of paddy rice farming in the economy, we observe high seasonality that cannot be smoothed across growing seasons for different crops. While some households in our setting farm other crops, paddy rice is the major crop of Nepal, accounting for a quarter of GDP. In our study districts, paddy rice represents 68% of cereal production by weight (of Agri-

culture & Livestock Development 2022). Lucrative off-season migration opportunities created by the open border between Nepal and India also lessens crop diversification in the origin villages across seasons. In our data, migration income typically exceeds even the main rice harvest income for the average household.

Figure 3 shows the fraction of households that report worrying about not having enough food and reducing portion sizes due to lack of resources during each month of the calendar year. Food insecurity is highest during the "lean season" preceding the rice harvest, as savings from the last harvest dwindle. Insecurity is lowest when households obtain income from the rice harvest, which coincides with the period when migrants return with remittance income. The strong seasonal patterns in food insecurity suggest that there are important frictions in our context that prevent households from smoothing their consumption over time. This exact same phenomenon has been noted in Bangladesh (Bryan et al. 2014), Indonesia (Bryan et al. 2021), Zambia (Fink et al. 2020), Kenya, and many other low-income regions around the world.

Despite the majority of rural Nepali households sending a seasonal migrant to earn income elsewhere when agricultural opportunities disappear between planting and harvest, seasonal deprivation remains widespread. This motivated us to explore whether difficulties sending remittances back from the migration destination to the village of origin is yet another market failure that contributes to the persistence of seasonal poverty.

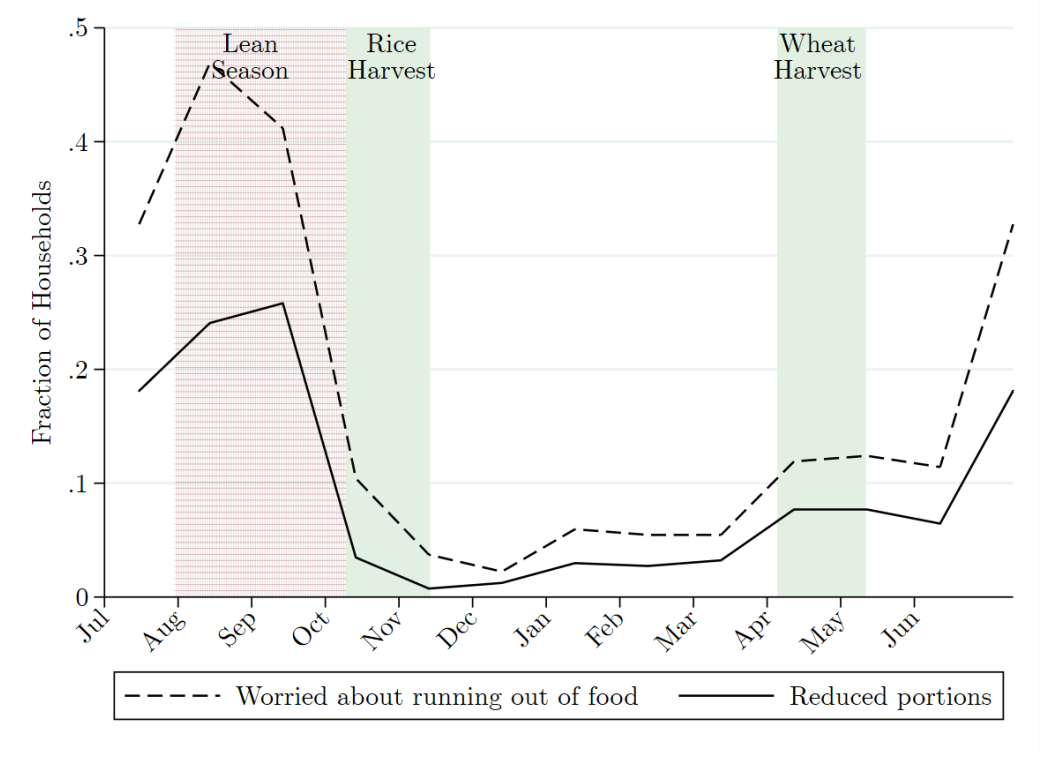



FIGURE 3. Seasonal Timing of Food Insecurity

### 3.1 *The Migration Experience*

64% of migrants in our sample travel to India, with the most popular destinations being Himachal Pradesh (HP), Maharashtra, and Delhi. Of those that remain in Nepal, roughly half migrate within their own district. Outside of our study districts, the most popular destination within Nepal is Kathmandu, capturing 20% of domestic migrants and 7% of migrants overall. One-fifth of migrants work in agriculture (e.g. seasonal apple-picking in HP). The most popular non-agricultural occupations are construction, factory work, and security guards. Among migrants who returned around harvest in our sample, the median migration episode is 2.5 months; 10% of episodes are less than three weeks, and 10% of episodes are over 7 months. Migrants earn on average 14,384 NPR per month, and they report saving 10,439 NPR per month. In contrast, non-migrants earn only 1,544 NPR per month during that time. This is largely due to the fact

that most non-migrants do not report any wage work during the lean season. Conditional on reporting any wage work, the average non-migrant earns around 8,407 NPR per month during the lean season.

### 3.2 *Remittances*

The average household receives 16,402 NPR in remittances over the four months of our follow-up. 53% of households receive remittances, and conditional on receiving any remittance, households receive 34,709 NPR on average over the four months of tracking which sometimes included multiple migration episodes. In comparison, the average rice-growing household harvests 24,607 NPR worth of rice based on our surveys. Migration income therefore forms the majority of household income in migrant-sending households, and it is a significant portion of income in the entire sample.

Figure 4 shows seasonal variation in remittances. They are counter-intuitively *pro-cyclical*: remittances are lowest during the lean season when consumption at home is lowest, and highest during the harvest season when consumption at home is highest. Further, this pattern is entirely driven by seasonality in remittances brought back by hand with the migrant or sent by hand through their network. Migrants in our sample remit both via financial institutions (e.g. bank transfers) and by hand. Across the five rounds of data collection the average remitting migrant remits 1.9 times, and 51% of remittance-sending migrants only remit via hand. Remittances sent digitally via bank transfers or remittance vendors show no seasonal variation. The seasonal patterns indicate that there are frictions that prevent remittance income from being accessed at home during the lean season when it is needed the most.

### 3.3 *Why is it Difficult to Remit Money?*

Anecdotal evidence and qualitative interviews from our context (and others) suggest a number of reasons why remitting is difficult for rural migrants. First, remitting remotely requires real-time coordination between migrants and their families - migrants must tell their households when they send money and how to pick

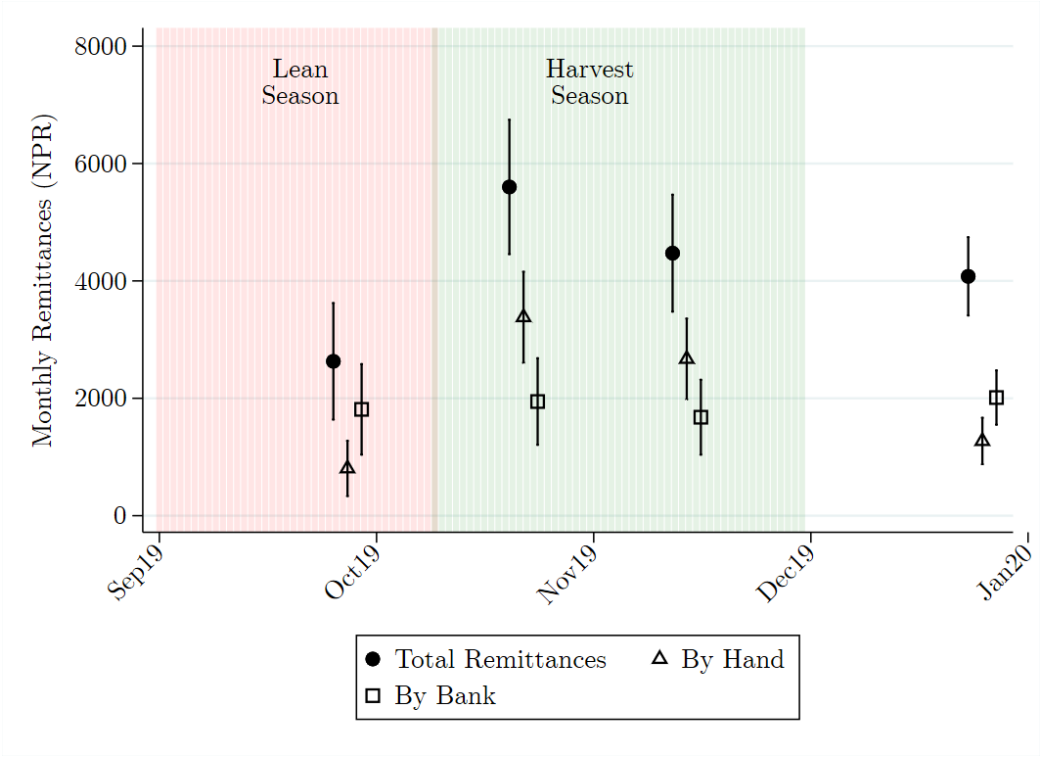



FIGURE 4. Timing of Remittances (Control Group Data Only)

it up. Not all migrants have cell phones, and even when they do, migrants to India are on a different cell/SIM network than their family members in Nepal, creating frictions in communication. In our experience with phone-based data collection from Kathmandu, it is relatively easy to reach non-migrant family members in rural Nepal, but quite difficult to contact migrants while they are away from the village. Cell reception at both the origin and destination can be faulty.

Second, older family members such as parents of migrants may find it difficult to both contact their migrant and navigate the digital remittance system, due to deficiencies in literacy and technical capacity.

Third, remittance points are sparse. Receiving remittances often requires travel to locations far from the village, and sending remittances requires the migrant to travel far from their workplace. This is especially true for migrants working outside major urban centers, such as those to take up apple-picking jobs in Simla.

Fourth, migrants face institutional barriers in India to accessing formal bank-based money transfer systems. This was exacerbated under the recent Indian administration, which has increased enforcement of documentation requirements for migrants to access various services and institutions, including even applying for a SIM card and mobile phone services. For the most part, migrants from Nepal cannot use Aadhaar-card-based remittance services available to Indians. While many have false Aadhaar cards that can be used to gain employment, these counterfeit cards are generally detected by banking services.

Since our loan intervention does not directly address any of these technical barriers to remitting, it is possible that people's reactions to the intervention reflect other behavioral barriers rather than remittance difficulties. Household members may have greater difficulty saving at home, or the migrant may be hiding some destination income from their spouse at home, or employers may be withholding migrants' wages until the end of the season. We explore these possibilities more carefully in sections 7 and 8.

## 4. Field Experiment

### 4.1 *Seasonal Loan Intervention*

Our intervention delivered loans valued at 10,000 Nepali Rupees (roughly $90 USD) to a subset of randomly selected poor households during the peak of the agricultural "lean season' in August. The loans were delivered by Backwards Society Education (BASE), a multi-faceted NGO that runs multiple programs in our study areas, such as disaster relief delivery, STEM education for girls, and facilitating inter-ethnic dialogue and conflict resolution. This consumption loan was an entirely new product introduced into BASE's portfolio of activities.

72% of loan recipients were female. BASE designed some specifics of the loan program to maximize the likelihood of repayment, based on their prior experience with these recipients. These were applied universally to all loan recipients. BASE organized loan recipients into groups of 7-11 borrowers from the same village to create some sense of group-based liability and some group incentives.

Any groups that did not repay in full were told that they would lose eligibility for future BASE programs in the area. As a further incentive, groups that repaid in full by the deadline received 10% of their loan principals back. 98% of loans were repaid in full by the deadline, which, combined with the repayment incentives, meant that 89% of the initial loan amount was repaid. Loans were collected in two installments: the first took place in late November, and the second took place in late December. The timing of collection was chosen to coincide with the timing of rice harvest income.

### 4.2 *Experimental Design*

Our field experiment features two levels of randomization. First, we randomly assigned half of our villages to receive the loan program and half to serve as pure control. We stratified our village-level randomization by ward. Second, within each treatment village, we conducted a public lottery to randomly select recipients from the subset of poor loan-eligible households. Eligibility was determined through the participatory wealth ranking (PWR) exercise described in section 2. We invited all loan-eligible households from the PWR to attend the public loan lotteries. Lotteries were held during a single village meeting and only households who showed up to this meeting were included in the lottery. Households were given a short assessment by our NGO partner prior to the lottery; 9% of households were deemed ineligible for the program prior to the lottery by our NGO partner because they were not growing rice and had no household members of prime earning age. 27% of eligible households were invited but not in attendance at the time of the lottery administration. It is possible that non-attending households did not value the loan, although in our experience other logistical challenges can also prevent even interested households from attending meetings at a specific time in this context. Altogether, 64% of initially eligible households participated in the lottery. Half of the households who participated in the lottery were randomly selected to receive a loan.

## 4.3 *Empirical Strategy*

4.3.1 *Pre-specification* One limitation of our study is the lack of a public pre-analysis plan. Due to the high-frequency nature of our phone surveys, we began collecting (and had access to) our main outcome data less than two months after the completion of baseline surveys. We failed to post a public pre-analysis plan before we gained access to the data, which limits the transparency of our analysis. To increase transparency, we have reported alternative specifications in addition to our preferred specifications in the text and online appendix, and have posted the full questionnaires for our study online.

4.3.2 *Within vs. Between-Village Randomization* We illustrate our experimental design and the groups we compare in our analysis in Figure 5. The unshaded rectangle on the right labeled *D* represents pure control villages where no one received loans. Within the "treated villages", 36% of eligible households do not attend our loan lottery, represented by the upper gray crosshatched portion of the left rectangle. Among those who do attend the lotteries, half are winners and receive loans (region *A*) and the other half are unlucky losers (region *B*).

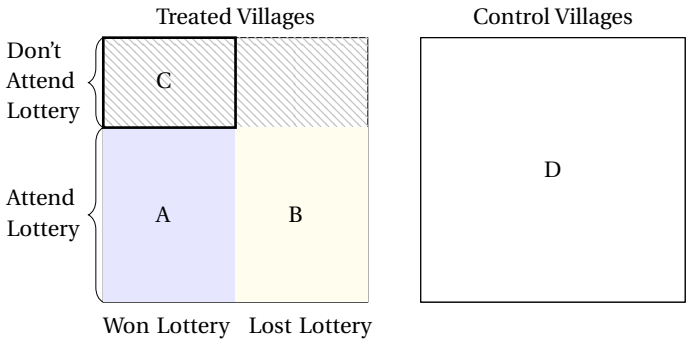



FIGURE 5. Experimental Design

There are two ways to estimate the effects of loans in this design, linked to the two levels of randomization. The simplest is to compare lottery winners (region *A*) to lottery losers (region *B*). We estimate the equation below on the sample of

loan lottery attendees:

$$Y_{iv} = \beta \cdot won_{iv} + \Theta X_{iv} + \epsilon_{iv} \quad (1)$$

Where $Y_{iv}$ is the outcome for household $i$ in village $v$, $won_{iv}$ is an indicator for winning the loan lottery, $X_{iv}$ is a vector of controls selected via double lasso (Belloni et al. 2014), and $\beta$ is our treatment effect of interest.

A second method would be to use our village-level randomization, and to compare lottery winners in treated villages to an equivalent set of households in control villages. However, since only a subset of eligible households chose to attend the lottery in treatment villages (excluding region *C*), we do not have the exact counterfactual in control village that distinguishes *C* from *A*. We don't know who would have attended the lotteries in control villages, whom we would accurately compare group *A* to. In our second specification, we therefore compare lottery winners (region *A*) plus a random half of the households who did not attend the lotteries in treated villages (i.e. region *C*) to all households in pure control villages:

$$Y_{iv} = \alpha \cdot loans_{v} + \Psi X_{iv} + \epsilon_{iv} \quad (2)$$

In this specification, our treatment variable $loans_v$ varies only at the village level. Because 36% of our sample in this regression are effectively non-compliers who did not attend the loan lottery, this should be interpreted as an intent-to-treat (ITT) estimate of the effect of *offering* loans. In contrast, the within-village comparison in Equation 1 of lottery winners to losers is an estimate of the effect of receiving a loan among the loan-eligible who sought a loan. Loan uptake was virtually universal, conditional on winning our loan lottery.[4]

[4] If we assume that non-attendees did not benefit from loans in treated villages, then we can estimate using between-village variation the equivalent of our within-village treatment effect by instrumenting for winning the loan lottery using village treatment status on the same sample discussed in Equation 2. This will effectively scale our ITT estimate by the inverse of the probability of attending the loan lottery in treated villages.

### 4.4 *Attrition and Balance*

One key threat to the internal validity of our study is selective attrition that generates differences in unobserved characteristics between treated and comparison households. Given our two stages of randomization, there are two types of attrition that can bias our results: 1) attrition that occurs after our first stage of randomization at the village-level, but before our within-village lottery, and 2) attrition that occurs after our within-village lottery. The first type of attrition affects only our between-village comparisons, but not our within-village comparisons of lottery winners and losers. The second impacts both types of comparisons.

Supplementary Table 1 (Mobarak et al. 2026) shows differences in tracking rates for our baseline survey and our subsequent phone surveys between loan and non-loan villages. Overall, a large share of households listed in our village census were not tracked during the baseline survey. Attrition was 23% in loan villages and 26% in control villages. The p-value on the difference is 0.11. Our phone surveys had relatively lower attrition: 90% of our baseline sample was successfully contacted in at least one phone survey and response rates were 87% over the five survey rounds. There are no differences in the share of households that respond to at least one phone survey by loan village status or lottery outcome. However, overall response rates using data from all 5 survey rounds are higher for lottery winners than losers by almost 4 percentage points.

We test for balance on baseline variables across our experimental groups two ways: first, we report a test of balance in our estimation sample by regressing an indicator for experimental group (loan village vs. non loan village; lottery winners vs. losers) on a set of baseline characteristics related to our outcomes of interest for respondents. We use the same regression specifications in our balance tests that we will later use for estimating treatment effects: We estimate this for our estimation sample of respondents, include strata fixed-effects, and cluster at the level of the village and household when testing the balance of our village and household-level randomization, respectively. The F-test of joint significance tells us if we can reject balance from our randomization: this test is marginally signif-

icant (p = 0.065) for our village-level randomization and insignificant (p = 0.223) for our within-village randomization.

Second, since any attrition impacting our within-village randomization occurred after baseline surveys were conducted, we have the ability to conduct a more powerful test of our identifying assumptions for our within-village comparisons. This is the test of the IVal-R assumptions needed for internal validity recommended by Ghanem et al. (2023). It amounts to a joint test of the two hypothesis that baseline characteristics are equivalent between 1) treatment and control *respondents* and 2) treatment and control *non-respondents*.

We conduct this test by estimating the following model where $R_i$ is an indicator for responding, $T_i$ is an indicator for residing in the treatment group, and $X_i$ is the baseline characteristic:

$$X_i = \beta_0 + \beta_1 R_i T_i + \beta_2 R_i(1-T_i) + \beta_3(1-R_i)(1-T_i)$$

Ghanem et al. (2023) show that a test the joint hypothesis that $\beta_1 = 0$ and $\beta_2 = \beta_3$ is a sharp test of the assumptions needed for internal validity (IVal-R).

We conduct this for each baseline characteristic in Supplementary Table 2 and present these results in Supplementary Table 3 (Mobarak et al. 2026). We reject the IVal-R assumption at the 5% level for one of our eight baseline variables, log-income (p = 0.028), without accounting for multiple testing. The false-discovery rate we would need to allow in order to reject this hypothesis is 0.227 (Benjamini & Hochberg 1995).

In sum, we cannot reject balance for our estimation sample and generally cannot reject the IVal-R assumptions using the sharp test of Ghanem et al. (2023) when accounting for multiple tests.

We also test for selection into responding overall and differential selection into responding for treated and control households in **??**. There is selection into re-

sponding to phone surveys - respondents have higher income and education and more migrants at baseline. There is some evidence of differential selection for one of our eight variables - baseline income. Respondents are more positively selected on log income in the treatment group than the control group (p = 0.011 without adjusting for multiple testing).

To be cautious, we control for double-lasso selected baseline variables in all our main specifications. None of our main results are sensitive to the inclusion of log income as a control or to weighting by the inverse-probability of responding to phone surveys using baseline characteristics.

Given that there is suggestive evidence of imbalance in our village-level randomization and differential attrition, between loan and non-loan villages in our baseline survey, as well as high overall attrition at the baseline survey that leaves more room for differential selection into the study in loan and non-loan villages, we have more confidence in the validity of our within-village comparisons of lottery winners to lottery losers than in our between village comparisons. One caveat is that there are greater possibilities of spillovers within villages to lottery losers, but existing evidence from the most similar context – seasonal migration loans in Northern Bangladesh – suggests that such spillovers are positive (Meghir et al. 2022), and would therefore attenuate the treatment effects we will report. As a suggestive test for spillovers, we test for heterogeneity in treatment effects along non-random variation in the fraction of households who are eligible for loans in our study villages, Supplementary Table 7 (Mobarak et al. 2026) [5]; we find no evidence of different effects between low and high-eligibility villages. We therefore prioritize within-village estimates in the main text, but report both within and between-village estimates in the appendix. We emphasize results that are consistent across both specifications.

[5] This evidence is only suggestive, since the fraction eligible in a village is mechanically correlated with average village socioeconomic status. There is substantial variation in the fraction eligible due to variation in SES across villages; the fraction eligible ranges from 10% to 100% of households in a village, with mean 0.64 and SD 0.26.

# 5. MODEL

## 5.1 *Model Setup*

As described in section 3, there are three important "states" for the typical household in our sample: (a) migrant at the destination during pre-harvest lean season (state *D1*), (b) other family members remaining at the rural origin during that same lean period (state *O1*), and (c) the whole family reunited at the origin during harvest, when the migrant returns (state *O2*). To reflect this reality, we model a unitary household that maximizes utility from consumption in those three states *D1*, *O1*, and *O2*: period 1 consumption by migrants in the destination, period 1 consumption at home during the lean season, and consumption at home during the harvest season (period 2). We'll denote these $C_d, C_{o1}$, and $C_{o2}$ respectively. Utility is the sum of log consumption in each location/period:

$$u = log(C_d) + log(C_{o1}) + log(C_{o2})$$

This model setup is illustrated in Figure 6.

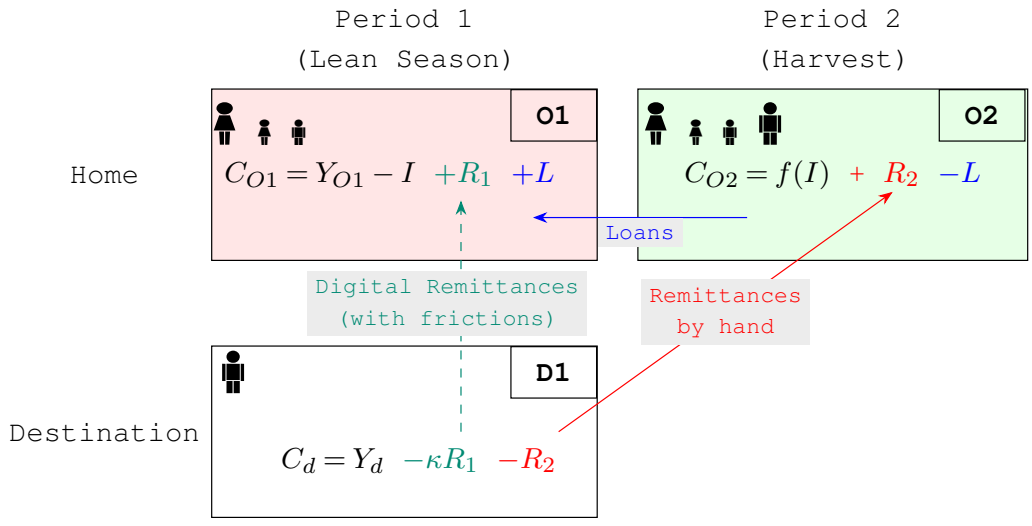



FIGURE 6. Diagram of Model Setup

In period 1, the household earns migration income $Y_d$ at the destination, and income $Y_{o1}$ at home. They can invest $I$ in agricultural inputs such as fertilizer, which pays off in period 2 in the form of $f(I)$ – the value of the rice harvest.

We model our intervention as the household receiving an exogenous zero-interest loan $L$ in period 1 which is paid back in period 2.[6] Migrants can bring back with them any income they didn't consume in the destination as remittances $R_2$ when they return home in period 2. Households do not default on the loan. They optimize utility below:

$$u = log(Y_d - \kappa R_1 - R_2) + log(Y_{o1} - I + R_1 + L) + log(f(I) + R_2 - L)$$

We model the remittance friction $\kappa \geq 1$ as a cost to sending remittances in period 1. This most frequently takes the form of transaction costs associated with using a money transfer technology (including travel to remittance points at both ends, communication costs, learning how to use the technology, acquiring documentation, etc, as described in section 3.3). But it could also represent the cost of returning home with money in hand in the middle of the lean season, or the opportunity cost of taking a job that pays out in increments, instead of a long contract with a wage premium that pays out when the migrant leaves at the end of the season. $\kappa = 1$ represents the case in which sending remittance is costless.

Households in this model choose how much to remit in each period ($R_1$ and $R_2$) and how much to invest ($I$), in order to smooth consumption across the three states, $O1, D1, O2$. Making the loan available changes the household's ability to smooth, thereby potentially changing remittance choices. States $D1$ and $O2$ were already connected via $R_2$, and the migrant can costlessly shift resources between these two states by bringing remittances back in his person when he returns in period 2. The loan further connects states $O2$ and $O1$, thereby giving the household the ability to shift resources from $D1$ to $O1$ in two steps (take the loan and use $R_2$ to repay that loan in state $O2$ using funds from $D1$).

[6] We treat $L$ as exogenous because loan take-up was virtually universal in our estimation sample of households that attended the loan lottery

While our discussion focuses on remittance constraints which have received less attention in the literature, multiple other overlapping frictions are implicit in our model which also prevent households from smoothing. In particular, there is no borrowing or saving in our model outside the loan we offer. We present descriptive evidence and allude to other relevant literature in sections 8.1 and 8.2 to explain the nature of credit and savings constraints in this context. In our data, most households do not have savings accounts and the most common alternative credit has interest rates of 72% annually without compounding. We make the simplifying assumptions to focus on the role of remittance constraints.

We also note that our model treats the decision to migrate as given, and doesn't allow for loans to change migration behavior. This simplification is motivated by the fact that loan lotteries occurred after the majority of out-of-district migration decisions were made (see Figure 2). In support of this, we see no impact of loans on migration hours outside of the study district in Table 1. In Supplementary Table 10 (Mobarak et al. 2026), we also see no impact on migrant return dates.

### 5.2 *Model Predictions*

The model generates several testable predictions. We focus on the predictions that we can test in our data. We highlight the results with basic intuition in this section and relegate the detailed proofs to Appendix section **??**. In this section, we assume $f'(I) > 0$ and $f''(I) < 0$. We assume an interior solution for investment ($I > 0$) since almost all of our households grow rice. For prediction 3, we assume $C_{o2} \geq C_{o1}$ (harvest-period consumption exceeds lean-period consumption), which is the case on average in our data.

Our model gives several straightforward predictions for the effects of the loan experiment when there are interior solutions for remittances and investment ($R_1, R_2, I > 0$). However, 69% of remitting households have corner solutions where either $R_1$ or $R_2 = 0$, and the model's predictions become more nuanced due to these cases. In **??** and **??** we display model predictions for three cases:

$R_1, R_2 > 0$ ; $R_1 = 0, R_2 > 0$; and $R_1 > 0, R_2 = 0$. We summarize these predictions below and qualify in which of the three cases they hold.

**Prediction 1:** $\kappa \geq \frac{\mathbf{C_{o2}}}{\mathbf{C_{o1}}}$, if $R_2 > 0$. *If migrants remit any money in period 2, the ratio of period 2 to period 1 consumption is a lower-bound for* $\kappa$.

**Intuition**: If we observe positive remittances in period 2 despite the fact that consumption during the harvest (state $O2$ in period 2) is greater than consumption during the lean season at home (state $O1$ in period 1), then it must imply that there is some additional cost to remitting money in period 1 i.e., $\kappa > 1$). Otherwise, the migrant would have sent more remittances in period 1 until either $C_{o1} = C_{o2}$ or remittances in period 2 were zero.

The model therefore implies that observing any positive remittance during the harvest ($R_2 > 0$) is itself evidence that there is a remittance friction during the lean season.

**Prediction 2:** $\frac{\mathbf{dI}}{\mathbf{dL}} \geq \mathbf{0}$. [$\frac{dI}{dL} > 0$ if $R_1 = 0 | R_2 = 0$, and $\frac{dI}{dL} = 0$ if $R_1, R_2 > 0$]. *Loans increase investment if either period 1 or period 2 remittances are equal to zero, and have no impact on investment otherwise.*

**Intuition**: The loan increases period 1 liquidity and allows the household to make new investments that pay off in period 2. But if we have an interior solution ($R_1, R_2, I > 0$), then the household will reduce period 1 remittances with the loan and increase period 2 remittances instead of investing those funds. This is because households were already investing until $f'(I) = \kappa$. Additional investment would make $f'(I)$ fall below $\kappa$, so the cost-savings from reducing period 1 remittances ($\kappa$) are larger than the returns to additional $I$.

**Prediction 3:** $\frac{\mathbf{dR}}{\mathbf{dL}} \leq \mathbf{0}$ **if** $\kappa = \mathbf{1}$. *If remitting money is costless, then total remittances* ($R = R_1 + R_2$) *should weakly decrease in response to the loan.*

**Intuition**: If there are no frictions on lean season remittances then total remittances only depend on total income at home across periods 1 and 2, since remittances can flow freely between these two periods. The loan (weakly) increases to-

tal income at home by relaxing liquidity constraints preventing investment, and so should (weakly) decrease total remittances.

**Prediction 4:** $\mathbf{\frac{dR}{dL} > 0}$ **if** $\kappa > 1$, and $[R_1, R_2 > 0$ or $(R_2 > 0$ & $\frac{dI}{dL} f'(I) < 1)]$. *If there is a remittance friction, then providing the loan increases total remittances* $(R = R_1 + R_2)$ *if either a) both period 1 and period 2 remittances are positive, or b) period 2 remittances are positive and the treatment effect on agricultural revenues is less than the loan value*

**Intuition**: If remitting in period 1 is more costly than in period 2 ($\kappa > 1$), the loan allows the household to decrease period 1 remittances and increase period 2 remittances. Since period 2 remittances are less costly, the average cost of remittances declines and households remit more in total at any interior solution, $R_1, R_2 > 0$. At the corner solution $R_1 = 0, R_2 > 0$ the household will increase $R_2$ as long as period 2 liquidity declines in response to the loan - i.e. as long as the increase in harvest revenue $(\frac{dI}{dL} f'(I))$ is not greater than the value of the loan.

Note that Predictions 3 and 4 jointly imply that we would observe a positive treatment effect on total remittances $R = R_1 + R_2$ from our loan experiment *only if* there is a remittance friction ($\kappa > 1$). This will constitute one of our main experimental tests of the remittance friction, because it is difficult for any other competing model to also generate the prediction $\frac{dR}{dL} > 0$ absent a remittance friction.

**Prediction 5:** $\mathbf{\frac{dR_1}{dL} < 0}$ if $R_1 > 0$. *Loans decrease period 1 remittances (if households were remitting in period 1)*

**Intuition**: Loans increase period 1 liquidity, decreasing the need for period 1 remittances.

**Prediction 6**: $\mathbf{\frac{dR_2}{dL} > 0}$ if $R1, R_2 > 0$ or $R_2 > 0, \frac{dI}{dL} f'(I) < 1$. *Loans increase period 2 remittances at the margin if a) there is an interior solution for period 1 and 2 remittances, or b) period 2 remittances are positive and revenues increase by less than the loan value*

**Intuition**: If $R_1$ and $R_2$ are positive households trade $R_1$ for $R_2$ in response to the

loan. If $R_1 = 0$, $R_2$ only depends on net liquidity in period 2 at home relative to the destination. If revenues decrease by less than the value of the loan, period 2 liquidity decreases and households remit more in period 2 to compensate.

**Prediction 7:** $\frac{\mathbf{dC_{O1}}}{\mathbf{dL}} > \mathbf{0}$ if $\kappa > 1$ or $R_1 = 0$ or $R_2 = 0$ . *Loans increase period 1 consumption if there are remittance frictions or if there is a corner solution for period 1 or period 2 remittances. Loans have zero effect on period 1 consumption when there are no remittance frictions and positive period 1 and period 2 remittances*

**Intuition**: Loans allow households to smooth consumption between states and therefore increase consumption in the lean season. If there are no remittance frictions and positive period 1 and 2 remittances, households would have already smoothed consumption between the three states in our model.

This prediction yields yet another test for a remittance friction: the loan should lead to improvements in consumption smoothing and lean-season food security only if the market for remittance transfers was not operating perfectly, or there is a corner solution for either period 1 or period 2 remittances.

## 6. Experimental Results

We combine these model predictions with experimental results on household responses to the loan treatment to infer whether our sample households in rural Nepal face remittance frictions. The cleanest empirical tests are those that use the experimental variation comparing the behavior of lottery winners to lottery losers within villages. We report average treatment effects of the randomly-assigned loan on household choices regarding agricultural input purchases in the pre-harvest lean period $(O1)$ and the resulting harvest-period agricultural outcomes (e.g. value of rice harvest and revenues), household labor allocation at $O1$, measures of food security and subjective wellbeing during lean and harvest periods, and migrants' remittance behavior during the lean and harvest periods, $R_1$ and $R_2$.

Our model yields a few distinct predictions for the lean season (period 1) and the harvest season (period 2), so we collected multiple rounds of data to cover

both. The first two rounds of our 5-round phone surveys were completed before the 2019 rice harvest occurred for any sample households. So we classify the first two rounds of data as lean season or 'period 1'. Rounds 3 and 4 of our phone surveys took place during or soon after the rice harvest, but before loan repayments began. Round 5 phone data were collected after households began repaying the loans. We therefore categorize rounds 3-5 as 'period 2' (harvest season). We also separately examine treatment effects pre- (rounds 3-4) and post-loan collection (round 5) for outcomes such as remittances, food security, and subjective well-being.

We focus on estimates using within-village variation in the body of this paper and report all parallel estimates using between-village variation in Supplementary Tables 4, 5, and 6 (Mobarak et al. 2026). Reassuringly, the between-village estimates are generally qualitatively similar to within-village estimates, unless explicitly noted in the text.

Before turning to experimental results in the next sub-section, we should note that Prediction 1 from the model provides some guidance on what we should observe in the *descriptive* data if households are indeed remittance-constrained during the lean season. Since consumption at the origin is lower during the lean season (see figure 3), migrants should be remitting only in period 1, and not during harvest. But in the data, we observe that 87% of remittances arrive after the onset of the harvest season (in late October). Our model would interpret this fact in itself as evidence of remittance frictions, given Prediction 1.

### 6.1 *Effects on Agricultural Investments and Outcomes*

Prediction 2 from section 5.2 states that households should increase agricultural investments because winning the loan lottery allows liquidity-constrained households to shift resources earlier towards state $O1$. We start by reporting the average treatment effects of loans on household decisions regarding labor allocation and agricultural investment in the origin (state $O1$ in period 1) in Table 1, and the downstream impacts on agricultural outcomes realized in state $O2$. Given

some outliers in the data on rice yields, we report both 99% and 95% winsorized results.

Consistent with prediction 2, households increase agricultural investments on their own farm. Applications of nitrogen fertilizer increase in response to the loan. Loans were delivered roughly 1 month after paddy transplantation, after most productive non-labor inputs have already been applied. The exception is Urea (46% nitrogen) fertilizer, which is recommended to be applied as a top dressing at 4 and 8 weeks after transplant in Nepal (Shrestha et al. 2022). We see that investment in Urea (nitrogen) fertilizer after transplant increases by roughly 17% in response to the loan.

The loan treatment causes household members to spend more time on their own farm, which makes sense because labor is needed for fertilizer application. Weekly labor on the household's own farm increases by 3.4-3.6 hours on a base of 32 hours in control households. Part of this can be accounted for by a reduction in male work at nearby migration destinations, which decreases by 0.7-1.0 hours on a base of 5 hours per week.[7]. Migration to "nearby destinations" (usually the main towns within the districts of Kailali and Kanchanpur) is a form of *ex-post* migration to low-wage destinations to work on others' farms or for short-term wage work, unlike the *ex-ante* longer-distance migration to India or to Kathmandu. There are no significant changes in labor allocated to wage work in the village or at far-away destinations.

The total value of the investment increase on own farm was driven by the increase in labor. If we value labor at the wage rate, the value of increased labor on own farm over the 13 weeks covered by our surveys would be roughly 1300 NPR, and the combined increase in inputs would be around 1500 NPR or 15% of the loan value.

These investments in agricultural inputs in state $O1$ in turn increased rice production in $O2$. Rice production and harvests are very difficult to measure using

[7] In between-village estimates reported in the appendix the decrease is 0.5 hours and insignificant

phone surveys. Our survey asked about the amount of rice harvested that was allocated for specific purposes: sold, stored for food, stored for seed, and paid to landowners as part of a sharecropping agreement. We present the treatment effect on the sum of these four questions, which we believe is a less noisy proxy for total rice harvested.

Rice harvested increases by 12% or 110 kg. Amounts of rice sold, stored for food, and stored for seed increase by 51, 34, and 6 kg respectively. The percentage increase in rice sold is much larger than the percentage increase in overall production. This could be due to either (a) households needing more harvest liquidity to repay the loan, or (b) excess sales after household consumption needs are met. Reason (a) could in theory reduce the benefits of the loan program. Burke et al. (2019) find that payments due after harvest cause farmers to sell produce *earlier* at lower prices rather than storing and selling at higher prices later in the season, reducing overall profits. Unfortunately, later-season rice prices would be conflated with COVID pandemic effects in 2020, so we cannot say for certain whether the loan caused farmers to sell too early. However, the fact that rice stored for food and seed also increases suggests that rice-producing households did well, on net. Indeed, the treatment effect on total revenues from rice during the season was large and positive: an increase of NPR 2616, or about 11%.

TABLE 1. Effects of Winning Lottery on Labor and Agriculture

| | (1) w99 | (2) w95 | (3) N | (4) Control Mean |
|---|---|---|---|---|
| Weekly Farm Hours | 3.79 | 3.32 | 4410 | 31.9 |
| | (1.12) | (1.00) | | |
| Weekly Wage Hours at Home | 0.33 | 0.54 | 4410 | 8.16 |
| | (0.77) | (0.64) | | |
| Weekly Nearby Migration Hours | -1.06 | -0.76 | 4410 | 4.91 |
| | (0.46) | (0.37) | | |
| Out-of-district Migration Hours | -0.58 | -0.55 | 4410 | 24.7 |
| | (0.87) | (0.81) | | |
| Nitrogen Fertilizer (NPR) | 129.3 | 115.9 | 1118 | 702.4 |
| | (61.5) | (45.2) | | |
| Pesticide (NPR) | -11.7 | -20.8 | 1390 | 160.5 |
| | (23.2) | (17.1) | | |
| Ag Investment (incl. labor value) | 2071.0 | 1693.3 | 1118 | 21431.5 |
| | (712.8) | (651.4) | | |
| **Panel B: Agricultural Output** | | | | |
| Rice Harvested - single question (kg) | 48.9 | 65.9 | 943 | 999.3 |
| | (63.7) | (43.3) | | |
| Rice Harvested - sum of questions (kg) | 118.3 | 109.0 | 944 | 967.5 |
| | (49.0) | (42.1) | | |
| Stored for food (kg) | 44.9 | 51.3 | 944 | 660.4 |
| | (36.5) | (25.6) | | |
| Sold (kg) | 58.2 | 34.1 | 944 | 48.5 |
| | (16.7) | (10.2) | | |
| Paid to landowner (kg) | -2.88 | -7.94 | 943 | 204.7 |
| | (21.9) | (15.9) | | |
| Saved for seed (kg) | 8.75 | 5.59 | 943 | 7.09 |
| | (3.09) | (1.35) | | |
| Rice Value - sum of questions | 2862.0 | 2638.6 | 944 | 23413.6 |
| | (1184.8) | (1018.1) | | |

Table shows effects of winning loan lotteries on labor and agricultural outcomes. The dependent variable is listed in the far left column. Column's (1) and (2) show the treatment effects when the outcome is winsorized at the 99th and 95th percentiles, respectively. Columns (3) and (4) report the number of observations and households used in estimation. Column (5) reports the mean of the dependent variable among lottery losers. Labor outcomes control for household size and number of prime-age men. Agricultural outcomes control for land cultivated and planned input use at baseline. Standard errors are clustered at the household-level shown in parentheses.

### 6.2 *Effects on Food Security and Well-Being*

Table 2 shows the effects of loans on food security and subjective well-being separately for men and women, and then pooled across both genders. Since only men typically migrate, the gender-differentiated effects are useful for tracking how these welfare metrics likely changed at home and at the migration destination. Our measures are inverse-covariance-weighted indices of selected subjective well-being and food-security items described in section 2. We report the effects of winning the loan lottery separately for the lean season, the harvest season, the post-harvest season, and then the overall effect pooled across survey rounds. Columns 1-3 show an index of only subjective well-being items. Columns 4-6 show an index of food security items, and columns 7-9 show an index combining items from both categories. Units are cross-sectional standard deviations during the lean season.

Winning the loan lottery consistently improves both food security and subjective well-being measures in our pooled sample (columns 3, 6, and 9) during the *lean season*. This is consistent with prediction 7 from section 5.2. Table 2 reveals several additional patterns. First, female respondents consistently benefit more from the loan intervention than males. Improvements in food security and well-being mostly accrue to the individuals remaining in the rural area during the season of deprivation.

Second, these benefits of the loan intervention are largely limited to the lean season. These effects disappear especially in the post-harvest period when the loans have to be repaid. When we pool all survey rounds, our combined index shows a significant 0.09 SD improvement for women (largely driven by improvements in food security in the early rounds) while the effect for men (-0.03) cannot be distinguished from zero. When pooling over all respondents and time periods, we estimate .06 SD improvement in the index.

6.2.1 *Interpretation* The model in section 5 explains that when there are frictions in the market for remittance transfers during the lean season, the loan provides households with an opportunity to smooth consumption across states.

TABLE 2. Effects of Winning Lottery on Food Security and Subjective Wellbeing

| | Season | Subjective Wellbeing | | | Food Insecurity | | | All | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | (1) M | (2) F | (3) All | (4) M | (5) F | (6) All | (7) M | (8) F | (9) All |
| Lean Season × Won Lottery | Lean Season | 0.0688 | 0.128 | 0.113 | -0.0101 | -0.121 | -0.0859 | 0.0101 | 0.121 | 0.0859 |
| | | (0.068) | (0.051) | (0.042) | (0.064) | (0.054) | (0.043) | (0.064) | (0.054) | (0.043) |
| Harvest × Won Lottery | Harvest | -0.0112 | 0.0348 | 0.0221 | 0.0702 | -0.142 | -0.0802 | -0.0702 | 0.142 | 0.0802 |
| | | (0.074) | (0.055) | (0.045) | (0.059) | (0.057) | (0.043) | (0.059) | (0.057) | (0.043) |
| Post Harvest × Won Lottery | Post-Harvest | -0.0957 | -0.0833 | -0.0779 | 0.0708 | 0.0210 | 0.0307 | -0.0708 | -0.0210 | -0.0307 |
| | | (0.097) | (0.064) | (0.053) | (0.067) | (0.050) | (0.040) | (0.067) | (0.050) | (0.040) |
| **Pooled** | | | | | | | | | | |
| Won Lottery | | 0.00424 | 0.0458 | 0.0380 | 0.0303 | -0.0879 | -0.0546 | -0.0303 | 0.0879 | 0.0546 |
| | | (0.057) | (0.042) | (0.034) | (0.044) | (0.038) | (0.030) | (0.044) | (0.038) | (0.030) |
| N | | 1921 | 3541 | 5484 | 1534 | 2853 | 4407 | 1534 | 2853 | 4407 |

Table shows the effects of winning loan lottery on welfare measures by the sex of the respondent and timing of the survey. The first row shows the effects for the lean season, phone survey rounds 1 and 2. The second row shows effects for phone survey rounds for the harvest period rounds 3 and 4. The third row shows effects for the period after loan collection began, round 5. The bottom row shows effects pooled over all five phone survey rounds. The dependent variable in the first three columns is an inverse-covariance weighted index of mental health items. The dependent variable in columns (4) , (5), and (6) is an index of food insecurity items. The dependent variable in columns (7) (8) and (9) is an index of both food insecurity (positively coded) and mental health items. Columns titled "M", "F", and "All" estimate treatment effects for the sample of male, female, and both male and female respondents, respectively. Standard errors clustered at the household level are shown in parentheses.

The specific pattern of food security and well-being improvements we observe in household members residing in state $O1$ (women in rural areas during the lean season) is entirely consistent with the remittance friction we model.

The gendered pattern of results in Table 2 indicates consumption smoothing, because family members remaining behind are indeed more food-deprived than migrants during the lean season. We measured food consumption for a matched sample of 274 migrants from our study villages at the destination and their household members at home during the 2022 lean season. Table 3 shows the results of regressing log per-capita consumption on the person's location, controlling for household fixed effects and correcting for food price differences between home and the destination.[8] We report per-capita, per adult-equivalent, per-adult, and per square-root of household size measures of consumption in the different columns. Across all measures, migrants at the destination consume much more food (0.26 to 0.95 log points more) than household members at home.

In the appendix, we also present descriptive evidence that migrants do not seem to be sacrificing other aspects of well-being we can measure: sleep quality, feelings of anxiety, and depression. These mental health measures are generally better for migrants than non-migrants, do not decline for migrants when they travel from the village to the destination, and do not improve when migrants return from the destination back to the village.

### 6.3 *Effects of the loan on remittances*

As we discuss in section 5, a key empirical test for the presence of a market failure in remittances is the effect of the loan on remittances. The loan would only increase total remittances in our model if there is a remittance friction. Otherwise, the model predicts that offering loans would weakly decrease total remittances.

[8] We construct a Paasche price index using migrant-reported price differences between the village of origin and the migration destination for goods purchased at each location. Food at various migration destinations indeed costs between 6% and 25% more, with the median destination costing 18% more.

TABLE 3. Lean Season Food Consumption, Destination vs. Home

| | (1) Per Capita | (2) Per Adult Eq. | (3) Per Adult | (4) Sqrt Capita |
|---|---|---|---|---|
| In Destination | 0.93 | 0.61 | 0.54 | 0.25 |
| | (0.063) | (0.062) | (0.065) | (0.061) |
| Constant | 7.32 | 7.63 | 7.70 | 7.98 |
| | (0.045) | (0.044) | (0.046) | (0.044) |
| Observations | 569 | 569 | 569 | 569 |

Table shows differences in log consumption between migrants and non-migrants within households during the lean season. The independent variable is whether the respondent was working as a migrant in the destination or residing at home in the village during the lean season. Each regression includes household fixed-effects. The dependent variables are log consumption adjusted for number of adults and children at home. Column (1) divides consumption by the number of people (adults plus children). Column (2) divides by adult equivalents, defined as 1 + 0.7x(adults - 1) + 0.5x(children). Column (3) divides by the number of adults. Column (4) divides by the square-root of the number of people (adults plus children). Consumption is deflated with a Paasche index constructed using average migrant-reported price differences between each destination and the village for each food item they purchased in the destination. The price index ranges from 1.06 to 1.25 across destinations, with a median of 1.18. Standard errors clustered at the household level are shown in parentheses.

Table 4 shows that total remittances increased by NPR 2700-3100 when households were offered the loan. This represents a 20% increase in remittances relative to the control mean. Combined with predictions 3 and 4 outlined in subsection 5.2, the observed increase in remittances necessarily implies that our sample households must have been facing a remittance friction.

Predictions 5 and 6 in subsection 5.2 provide further guidance on how remittance behavior is expected to change if our loan recipients are indeed facing remittance frictions. Specifically, the theoretical prediction is that remittance-constrained households would increase remittances in period 2 (harvest), but not during period 1 (lean season). In fact, remittances in the lean period could (weakly) decrease (see prediction 6), since the loan pays out in state $O1$.

We test these predictions in two different ways in Table 4. First, the table shows that the total remittance effect was driven entirely by an increase in remittances brought back by hand, which occurs when migrants return during the harvest period. There was no change in (or a weak negative effect on) remittances sent via bank transfer or IME. This is exactly consistent with the theoretical predictions for households facing remittance frictions.

Second, since we collected data on remittances in all five survey rounds, we divide up the total remittance effect by survey timing to explore how remittances changed during the lean season versus the harvest season. We further subdivide the harvest season into two periods: pre- versus post- loan repayment collection by our NGO partner. Exactly as the model predicts, total remittances increase during the harvest season and not during the lean season. There is a small negative impact on remittances after loan collection (but this is a zero effect in the between-village estimates in Supplementary Table 6 (Mobarak et al. 2026)).[9]

The overall increase in remittances, plus these specific patterns of changes in remittances in response to the loan cannot be explained without a remittance friction, within the context of our model. It is possible that other more complex models could explain these changes without remittance frictions, and we discuss such alternatives in section 7.

### 6.4 *Heterogeneity Test for Remittance Frictions*

Prediction 3 in subsection 5.2 states that households that were not facing remittance frictions should not increase remittances in response to the loan offer. We can set up an additional non-experimental test of this prediction, because we observe a subset of households remitting using bank transfers, who presumably face smaller frictions. In Figure 4, we showed that remittances by bank transfers exhibit no seasonal fluctuation.

[9] We explore the intensive and extensive margins of remittances in the appendix. We see no extensive margin effect - i.e. no change in the propensity *to* remit, which is consistent with no change in migration. Instead, the total remittance increase is driven entirely by increases in remittances from migrants who were previously already remitting.

TABLE 4. Effects of Winning Lottery on Remittances

| | (1) w99 | (2) w95 | (3) N | (4) Control Mean |
|---|---|---|---|---|
| Total Remittances by Hand | 2311.9 | 2245.8 | 1126 | 7437.2 |
| | (993.6) | (745.1) | | |
| Total Remittances by Bank | -11.8 | -80.1 | 1126 | 5719.9 |
| | (1034.2) | (636.5) | | |
| Total Remittances | 2675.8 | 3101.0 | 1126 | 14842.3 |
| | (1672.4) | (1105.9) | | |
| Lean Season | 296.9 | 235.5 | 1074 | 1958.4 |
| | (437.2) | (272.4) | | |
| Harvest (pre-collection) | 2979.7 | 2571.9 | 1122 | 8982.4 |
| | (1151.1) | (803.9) | | |
| Post Loan Collection | -861.0 | -883.5 | 1093 | 3349.7 |
| | (548.3) | (372.2) | | |

Table shows effects of winning the loan lottery on remittance outcomes. The dependent variable is listed in the far left column. Columns (1) and (2) show the treatment effects when the outcome is winsorized at the 99th and 95th percentiles, respectively. Columns (3) shows the number of observations used in estimation. Column (4) reports the mean of the dependent variable among lottery losers. Regressions control for whether households expected to receive remittances at baseline and the number of international migrants. Standard errors clustered at the household level are shown in parentheses.

Using those households that received digital remittances at least once as a proxy for being "less remittance-constrained", we re-estimate the loan treatment effects on remittances by adding interaction term for such "bank user" households.[10] Table 5 shows the results. There is suggestive, but statistically imprecise, evidence that bank-user households with lower remittance frictions have a 2000 NPR *smaller* increases in remittances in response to the loan, as predicted by the theory. This is clearly an imperfect non-experimental exercise, since households

[10]We define non-banked or "remittance-constrained" households as those for whom no remitting migrant ever remits digitally. We exclude households for whom we never observe a migrant remit and those with multiple migrants where one ever remits digitally and another does not. The sample for this exercise therefore includes 618 households, 43% of whom are "bank users" and the other 57% of whom are "remittance-constrained".

using bank transfers may be different from those who do not in other unobserved dimensions.

TABLE 5. Heterogeneity in Effects on Remittances by Remittance Frictions

| | | | | Control Mean | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Won Lott | Won x Bank | N | Bank=0 | Bank=1 | P-Value (1) + (2) = 0 |
| Total Remittances | 6352.3 | -2131.5 | 604 | 19729.8 | 25450.8 | 0.12 |
| | (2071.0) | (3376.7) | | | | |
| Harvest Remittances | 5474.6 | -2685.2 | 603 | 13632.1 | 13278.8 | 0.12 |
| | (1666.3) | (2428.9) | | | | |

Table shows heterogeneity in the effects of winning loan lottery on remittance outcomes by usage of remote remittance methods. Column (1) shows the effect on outcomes for households who never remit remotely. Column (2) shows the interaction term for winning the lottery and ever remitting remotely. Column (3) and show the number of observations in the estimation sample. Columns (4) and (5) show the control mean of the dependent variable for households that never remit remotely and ever remit remotely, respectively. Column (6) shows the p-value for the test that the treatment effect for households who ever remit remotely is zero. Standard errors clustered at the household level are shown in parentheses.

## 7. ALTERNATIVE THEORIES

In this section, we discuss possible alternative theories that could rationalize our experimental results without resorting to a remittance friction based explanation. There are actually a large class of alternative models that could explain why offering subsidized credit to rural households increases their food security, well-being, agricultural investments, and output. However, many or most of those models would have difficulty rationalizing the most distinctive empirical result we have: that providing a consumption loan increases the remittances that household receives, and more specifically, it increases remittances during the harvest season and not the lean season. We therefore focus on alternative theories that could possibly explain that remittance result, and do not go into depth discussing other sensible models that cannot: e.g., the zero-interest loan is an implicit wealth transfer, the loan was perceived as limited liability, returns to agriculture are stochastic and the loan serves as insurance, households are

present-biased, the loan reduces savings constraints by "earmarking" household funds, etc.

### 7.1 *Non-unitary Households*

The leading alternative theory that could qualitatively match our experimental results is that these are non-unitary households, and that migrant and non-migrant family members with different preferences engage in some collective bargaining over household resources. In contrast, our model of remittance frictions assumes households are unitary.

In a non-unitary household, offering the loan to the household member remaining behind at the origin (typically, the wife) when the male migrant is away may change the wife's relative bargaining position. She consumes and invests more at home during the lean season, and if she is successful in making the migrant feel responsible for saving more of his destination income to repay the loan when he returns in period 2, that could explain the remittance result. In this model, each person cares more about their own consumption than that of their spouse.

A straightforward test of this alternative theory is to ask whether the migrant and his spouse have systematically different preferences over *her* consumption at home. The non-unitary bargaining logic requires that the migrant does not value his family members' consumption at home during the lean period as much as his spouse does, even when he knows that his family members (including children) remaining behind at the origin are food-deprived.

We conducted an additional experiment with our participants during the 2022 lean season to test for such differences in preferences. In phone surveys, we gave both migrant and non-migrant members of the same household the choice of when and where to receive a transfer of roughly $9 USD, or around 1.5 days of wages. Respondents could either have these transfers sent to the migrant in the destination via a phone "top-up" credit, or have them delivered to the household member remaining in the origin during the lean season (the week after their survey was completed), or delivered to the household in person during the harvest

season (after the migrant is expected to return). The key test is whether the migrant and his spouse systematically differ in when and where they choose the money to be delivered to.

Overall, 73% of all respondents requested for the transfer to be delivered home during the lean season, 21% requested for the funds to be delivered later during the harvest season, and 6% requested the mobile phone top-up. Table 6 regresses this choice on the respondent's location and finds that there is no significant difference between the migrant and non-migrant family members' choices on where the money is delivered. Migrants are 6 percentage points less likely to request the money be delivered to the spouse immediately during the lean season, relative to the spouse's own choice, but this small difference is not statistically different from zero (p = 0.166). The regression controls for household fixed effects, so the identification is based on the migrant's choice relative to his wife's. Based on the estimated coefficient and standard error, we can reject that less than 62% of migrants choose to send the money home during the lean season. The fact that the majority of migrants would willingly cede control of the funds so that family members can use them during the lean season makes it unlikely that the non-unitary household model is the key explanation for our main experimental results.

A second (indirect) testable implication of the non-unitary household model is that under that formulation, migrants would have an incentive to systematically under-report the income they earn at the destination to their family members remaining behind, so that they can retain more of the income for themselves. Baseler (2021) and McKenzie et al. (2013) both show that rural Kenyans and Pacific Islanders significantly under-estimate migrant income, and attribute this to migrants choosing to under-report income to relatives to moderate remittance demands.

We ask both migrants at the destination as well as non-migrant members of their households about the migrants' monthly earnings. The distribution of reported earnings for both sets of respondents is shown in Figure 7. We find that there is in-

TABLE 6. Differences Between Migrant and Non-Migrant Household Member's Preferences on Timing of Transfers

| | (1)<br>Deliver Transfer Home Now |
|---|---|
| Respondent in Destination | -0.060<br>(0.043) |
| Constant | 0.76<br>(0.031) |
| Observations | 378 |

Table shows differences in preferences for receiving transfers at home during the lean season between migrants and non-migrants within households. The dependent variable is whether the respondent chose to send a transfer of $9 USD to the household during the lean season, as opposed to sending the transfer home during the harvest season or as a top-up to the migrant's phone in the destination. The regression controls for household fixed-effects. Standard errors clustered at the household level are shown in parentheses.

deed a small average difference: non-migrants believe that migrants earn around 8% (or 1,600 NPR) less than the migrant's own report. While statistically significant, this is not a very large difference, especially relative to the differences reported in Baseler (2021) and in McKenzie et al. (2013). Either there is less scope for hiding income in our context, or these Nepali households behave in a relatively more unitary fashion, perhaps because their migration is circular and shorter-term compared to the other contexts.

As subsection 6.2 shows, the loan experiment shifts intra-household consumption towards women and family members generally more food-deprived and less powerful. 90% of household heads are male, and 46% of household heads are migrants. Our qualitative data shows that the most common reason cited by potential loan recipients – most of whom were female – for declining our loan offer was that the migrant who could authorize such financial decisions was not present. If intra-household bargaining is part of the explanation, then the loan treatment served to tilt decision-making power and consumption towards female

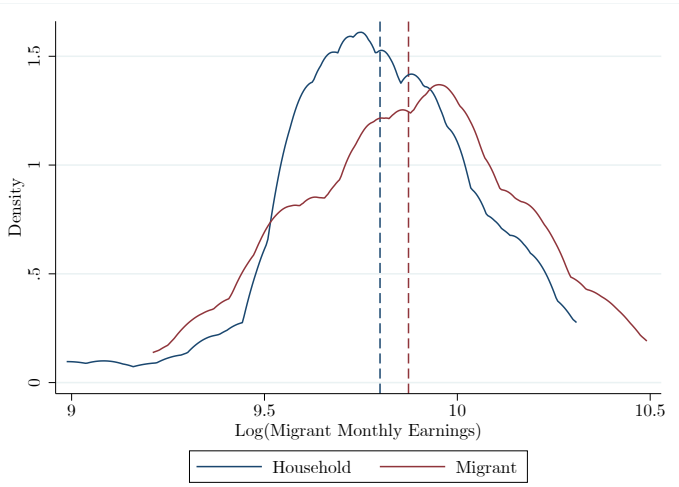



FIGURE 6. Figure shows the distribution of the log reported migrant income by 360 migrants and non-migrants in the same households. Surveys were conducted in August of 2022 while the surveyed migrants were away in the destination. Income is winsorized at the 2% level within groups. The average difference between migrant and non-migrant reported log income is 0.074 (p = 0.003).

FIGURE 7. Beliefs of Migrants and Non-Migrants about Migrant Earnings

family members who were relatively more deprived, which constitutes an additional benefit from this intervention.

## 8. WHY HASN'T THE MARKET SOLVED THE REMITTANCE FRICTION?

While our paper has focused on remittance frictions, our model implicitly includes a number of other overlapping market failures that must be present for seasonal deprivation to be sustained year after year. For example, either frictionless savings or credit markets would allow households to access their harvest income (including remittances) during the lean season, making our loan unnecessary. Here we describe why households in this context (and rural agrarian areas more generally) are likely also credit- and savings-constrained, which makes the remittance frictions we highlight especially harmful.

### 8.1 *Credit Markets*

Households in our sample do have some access to credit. The most common source of credit as reported in our baseline survey are informal money lenders in the village who charge interest rates of 6% *per month* on average, or 72% over a year without compounding. Such high interest rates are not at all unusual for that region. Mallick (2012) reports annual interest rates of over 100% among moneylenders in Bangladesh. So a short answer is that households are constrained by the cost of credit, and our interest-free loan bypasses that problem.

A longer answer – based on our qualitative fieldwork – is that local microfinance institutions (MFIs) are unwilling to extend loans to much of our sample. Government regulations cap those interest rates at 17.5%, but our sample households still rely on high-interest-rate informal moneylenders as their primary source of credit. In conversations with multiple MFIs we discovered that they have repeat-relationships with small groups of trusted borrowers in specific villages, and they all cite repayment rates of 99% or above. The MFIs generally perceived our loan with a 4-6 month grace period on repayments as riskier than their standard contract that requires monthly repayments that begin immediately after loan disbursement. Getting them to add a universal seasonal loan product to their portfolio would require further convincing. They may be rationally reluctant because the cost of administering small loans in these remote villages is inherently high (Aleem 1990), although Karlan & Mullainathan (2007), Field et al. (2013), and others have shown that introducing greater flexibility in microcredit products could be profitable for the lender.

### 8.2 *Savings*

Another way households could mitigate seasonal deprivation is by saving their harvest income to consume during the next lean season. The literature has identified many reasons why households struggle to save in such rural, low-income contexts. Lack of access to formal bank accounts, risk of theft, kinship taxation, and present-bias all make saving for the future difficult in the types of poor rural communities we study (Jakiela & Ozier 2016, Casaburi & Macchiavello 2019,

Riley 2020). On the other hand, the credit-based solution we offer to address the remittance gap does require the migrant to save at the destination and then return home with funds to repay the loan. Why would it be easier for the migrant to save at the destination than tosave their prior year's harvest at home?

If demands from relatives to share resources is an important savings deterrent (Jakiela & Ozier 2016), then it is sensible that it easier to save at the destination, away from kin. Migration destinations also offer better opportunities to hide income from their social network (Kinnan 2022, Baseler 2021). Our qualitative interviews with migrants suggest that many develop innovative ways of overcoming savings constraints in the destination such as asking their employer to hold on to their earnings, to counter their temptations to spend before they return home – a similar mechanism as that observed with Kenyan dairy farmers (Casaburi & Macchiavello 2019).

Remittance technologies and informal risk sharing are two other mechanisms or markets that these families could use to smooth consumption and mitigate seasonal deprivation. Section 3.3 explains why remitting money is difficult. *Western Union*-like technologies are absent in this area, which makes cross-border transfers from India to Nepal very difficult. And as described in section 3, the pre-harvest lean season in agrarian areas is an aggregate shock that affects most or all households, which limits the potential for localized informal insurance and risk-sharing.

## 9. Policy Implications and Conclusion

We present a combination of experimental results and a two-period model of consumption, migration, remittance, and agricultural investments to argue that remittance frictions undermine rural households' ability to smooth consumption and mitigate the effects of seasonal deprivation. High transaction costs have been shown to impede remittances in a cross-country study (Ahmed et al. 2021), which estimates that remittances increase by 1.57% for every 1% reduction in transaction costs. Remittances are the single most important financial flow into

developing countries, so creative solutions to overcome these frictions hold important potential consequences for development. Not only would migrants save billions of dollars in fees, but it would encourage migrants to transfer crucial foreign exchange into developing countries.

A lot of macro data suggests that remittance frictions are globally economically relevant. In 2023 alone, the United Nations, the International Monetary Fund and the World Bank all released reports highlighting the high costs of sending remittance. Globally, currency conversion and fees represent 7% of the amount sent (United Nations Department of Economic and Social Affairs 2024). Costs can be incredibly large in specific cases. For example, it costs more than $70 to send $200 from Tanzania to Uganda (Ratha 2023). Remittance costs in South Asia, the setting of our experiment, are actually lower on average than in the rest of the developing world (The World Bank 2023) - which means that the friction we identify through our experiment may be an even more significant source of inefficiency in other parts of the developing world. The IMF report estimates that reducing remittance costs by just 5 percentage points would cut costs globally by nearly $30 billion a year, with most of the savings accruing to poor migrants from developing economies (Ratha 2023). These estimates generally rely on observed formal remittances; informal remittance channels captured in our data are likely even more costly.

Our experiment shows that regardless of the underlying cause of the remittance friction, a well-timed consumption loan provided during the lean season can allow households to access the post-harvest remittance income earlier, which in turn increases agricultural investments and facilitates consumption smoothing. These results –combined with the theory – allow us to establish a market failure in remittance transfers, with a clear implication that there are potential welfare gains from designing policies or technologies to address this friction. Remittances are the largest documented financial flow into LMICs, and account for over 10% of GDP of many Asian countries, so the gains from removing frictions that impede remittance flows can be very large.

The ideal policy or technology design depends on how one interprets the meaning of "remittance friction". It could mean the literal absence of functioning remittance technology which raises the cost of remitting money during the lean season. The most direct policy response to this would be to design a "Western Union"-like system for money transfers between India and Nepal. However, difficulty remitting during the lean season could also take the form of employers withholding migrant workers' wages until the end of the season – either as a condition of employment, or at the request of the migrant facing temptations to spend. In such cases, the appropriate policy response might be a consumption loan like the one we designed. But offering the loan at zero interest like we did for the research may not be scalable as a policy. Future research will need to explore household willingness for lean season consumption loans at positive interest rates.

One way to improve on our research design would be to implement our intervention earlier, repeatedly, or at larger scale. We delivered the loan *after* the migration decision was made and most agricultural investments occurred. This simplified some of our analysis because we could study downstream outcomes holding migration decisions fixed. But changes to migration destinations or duration are important outcomes to track in any future work on lean season consumption loans. At a larger scale, impacts on labor supply via migration could also impact other markets in general equilibrium. For instance, there may be a decrease in local labor supply in agriculture, decreasing the returns to complementary agricultural investments. This could, in theory, change the effect of the program on agricultural investments and output.

Another possible improvement would be to test interventions that directly target remittance frictions by introducing a remittance technology. With rapid developments in mobile phone technologies and increased mobile penetration, this should be feasible going forward. Another limitation of this study is that, due to time constraints and the immediate collection of high-frequency outcome data a few months after baseline, we weren't able to finish a public pre-analysis plan before we received outcome data, which limits the transparency of the analysis.

Some additional methodological limitations of our paper include our imprecise measurement of agricultural outcomes through phone surveys, and that we only track short-run outcomes for 5 months post-treatment.

Seasonal deprivation is widespread in rural, agrarian areas around the world, and seasonal migration is a common response to mitigate the adverse effects of seasonal poverty. But this strategy only works if migrants can remit income back to their family members remaining behind during the lean season. Addressing any market friction in remittance transfers through policy or technology development can hold large consequences for very poor rural families who rely on that migration income during periods of food insecurity. More broadly, given the dependence of so many developing countries on remittance income from their diaspora, easing the process of remittance transfers can be highly productive, even beyond periods of seasonal deprivation.

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