Eliminating Uncertainty in Market Access: The Impact of New Bridges in Rural Nicaragua*

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Abstract

We estimate the impact of new bridges in rural Nicaraguan villages that face seasonal flooding risk. Floods unpredictably eliminate access to outside markets. We work with a partner NGO to build footbridges designed to eliminate this risk. Identification exploits small engineering requirements that preclude construction in some villages, despite their need for a bridge. We collect detailed annual household surveys over three years and weekly telephone followups with a subset of households for sixty-four weeks, both before and after construction. Bridges generate lower continuation rates in both farming and wage work, implying substantial reallocation of economic activity. Relatedly, we find a 39 percent increase in labor market earnings outside the village. Lastly, we find an indirect effect on farming, where households increase fertilizer spending and yields despite no changes in prices, along with a decrease in crop storage. In a model of risky farm investment we show that this is a rational response to lower income risk.

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1 Introduction

In recent years, researchers have devoted a great deal of attention to the consequences of misallocation across production units (Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009). However, this misallocation is generally measured indirectly through model decompositions. Direct evidence on misallocation is limited, and understanding both how it arises and its effect on productivity are critical to policy design (Restuccia and Rogerson, 2016).

In this paper we study directly the role of physical barriers to market access in generating misallocation in rural Nicaragua. In particular, along with a partner NGO, we build footbridges for villages that face seasonal flash flooding risk. Every year between May and October, flooding occurs unpredictably that cuts off access to outside food, product and labor markets. During these floods, income decreases and joblessness increases. We provide evidence on both the extent of this reallocation and the underlying mechanisms by conducting household-level surveys over three years before and after bridge construction. Our identification strategy is based on the fact that there are many villages that need bridges, but some cannot be completed due to engineering requirements. These requirements are small from the perspective of households in the village, but critical for safely constructing a footbridge. We discuss this further in Section 3 and show that these features are orthogonal to any relevant household or village characteristics. In addition to detailed household-level surveys, we conduct weekly telephone surveys for 64 weeks with a subset of households to capture the high frequency changes related to flooding and the introduction of a bridge.

A major barrier to studying transportation infrastructure as an intervention is the high construction cost of these interventions.[1] This is true in our context as well, where bridge costs approximately \$40,000. As such, our study includes only 15 villages. Therefore, since we have a small number of clusters, we use the wild bootstrap cluster-t procedure from Cameron et al. (2008). Despite this, we find economically and statistically significant effects. We find that the bridges induce factor reallocation

[1]This generally implies a difficult identification issue, as these expensive projects are targeted toward areas with the largest impact. We sidestep this by using engineering-related requirements, similar in style to Dinkelman (2011). Moreover, our close involvement with the data collection and construction allows us to collect detailed data before and after construction along a number of dimensions.

along several dimensions. There are a number significant effects on labor markets. First, we find that households earn nearly 30 percent more from wage work in response to a bridge, and this is driven due to increases in days worked outside the village while wages remain the same. The result is driven by two effects related to misallocation. First, individuals who had wage earnings at baseline shift from within-village wages to outside-village earnings and increase earnings. Second, those who had no earnings at baseline enter the market. Relative to those in villages without a bridge, those who receive a bridge have 39 percent higher earnings. Decomposing these results, new entrants account for two-thirds of the average effect. Thus, the bridge allows households better access to outside labor markets.

The reallocation is not limited to wage work. We find substantial changes in the likelihood of engaging in both farming and wage work in response to the bridge. Agricultural households are 30 percentage points less likely to be engaged in farming after a bridge, while households with market earnings at baseline are 13 percentage points less likely to have market earnings afterward.[2] The gap between these two changes suggests that the bridge causes a shift toward labor market work, but there is still a substantial fraction of households that are constrained away from agricultural production.

Lastly, we find an effect on agricultural intermediate input (fertilizer and pesticide) use. Standard theories of infrastructure development usually change agricultural decisions through changes in prices.[3] The short-term duration of the flood shocks make it unlikely that these price effects would occur here. While rivers flood quite often – during our high frequency survey, in one-third of weeks, at least one village was flooded – the floods can be short. As long as crops are storable over a matter of weeks, price changes are unlikely. Indeed, we find no changes in prices in response to bridges. Despite that, we find a large positive impact on farmers, in addition to the aforementioned effect on wage work. Farmers increase intermediate spending by nearly 70 percent in response to a bridge. Interestingly, unlike the impact on labor

[2] This should be interpreted with a caveat. As we show in our high frequency data, nearly all households receive some market income, and mostly differ in the intensity of use. The question we ask is about wage income in the previous month. Thus, the quoted number conflates extensive and intensive margin changes, and should properly be interpreted as labor market earnings in the previous month.

[3] See Suri (2011) and Donaldson (2013), for example, along with the quantitative theories proposed in Adamopoulos (2011), Gollin and Rogerson (2014), and Van Leemput (2015).

market earnings, the effect on continuing and new farmers is roughly the same. Thus, these bridges lower misallocation in different sectors and different inputs, which is an important policy consideration given the wide variety of income-generating activities in rural areas (World Bank, 2008).

To rationalize this indirect effect on farmers, we provide a model of risky agricultural choice. Farmers divide their harvest into the part that they sell in the market that can then be used to buy agricultural inputs, and the part that they store at home as a hedge against uncertainty in future income. A bridge reduces uncertainty in labor market income that reduces the need for precautionary storage. This allows the household to sell more of their harvest in the market and buy more agricultural intermediates. The data supports these model predictions. First, using the high frequency data, we find that nearly all households have some labor market earnings. Thus, the labor market is used even by households primarily engaged in agricultural production, highlighting its potential use as a smoothing technology, which has been emphasized in other context as well (e.g. Kochar, 1999, in India) Second, we find a significant decrease in crop storage. Households keep 8 percentage points less of both maize and bean harvest (the two staple crops) in response to a bridge, with no other changes in savings or debt. This is consistent with the model prediction of the need for a smaller buffer stock of savings.

1.1 Related Literature

The study of infrastructure benefits is large and varied. A recent literature has combined quantitative models with detailed data to provide evidence on the impact of trade costs and new construction (Alder, 2013; Asturias et al., 2016). More closely related are those papers who explicitly highlight the rural-urban link in their study of trade, such as Adamopoulos (2011), Gollin and Rogerson (2014), and Van Leemput (2015). They find large gains from reducing the cost of movement across regions, as firms are able to more able to specialize in their comparative advantage. However, both the high costs and benefits of infrastructure make identification difficult, as infrastructure tends to be targeted toward areas with the largest benefits. Recently, a number of important papers have taken advantage of policy changes and natural experiments to identify the effects of infrastructure development, including Suri (2011) and Donaldson (2013). These papers primarily focus on the impact on prices, which are not present here. More closely related is Asher and Novosad (2016) who show that new roads in India generate movement out of agriculture using discontinuities in the policy design. Dinkelman (2011) finds similar results, due to electrification in rural South Africa, and uses a similar "engineering-related" identification strategy based on land gradients. Relative to these papers, our close involvement in the actual construction of these bridges allows us to conduct detailed household-level surveys before and after construction to provide additional insight into the underlying mechanisms.

We lastly find that the bridge increases on-farm productivity, but not through any direct changes to prices. Instead, the bridges allow for increased consumption smoothing through labor markets, which in turn endogenously promotes risk taking on farms. This is consistent with a growing literature linking consumption risk to farm investments, including experimental evidence from Mobarak and Rosenzweig (2012) and Karlan et al. (2014) (among others), while Donovan (2016) highlights the importance of this channel for aggregate income differences. We show that lack of consistent labor market access limits agricultural productivity through this channel, which Restuccia et al. (2008) and Gollin et al. (2014) point out is substantially lower than nonagricultural productivity. Relatedly, Bryan et al. (2014) and Bryan and Morten (2015) also highlight constraints to the spatial allocation of labor as a component of this agricultural productivity gap based on the misallocation of talent across sectors.

2 Model

We begin with a model of the rural economy, which includes off-farm labor and agricultural decisions.

The model is comprised of a continuum of infinitely-lived households that are endowed with a technology called a farm. We refer to households as farmers for simplicity throughout. Time is discrete, and is comprised of two different types of periods.

Every T periods, a farmer harvests her crops from the last season. She also chooses investment in fertilizer to be harvested T periods in the future. In those T periods between planting and harvesting, the farmer chooses how to divide time between farm and off-farm labor. Below, we formalize this idea and lay out the problem recursively.

Harvesting and Planting Every T periods, a farmer enters the "harvesting and planting" period, t^H. This involves two things: harvesting the crops planted T periods ago at $t^H - T$, and also planting the crops that will be harvested T periods from today at $t^H + T$.

In a harvesting period, a farmer enters with some resources X from the previous period, and the farm produces output Y. Using her new resources $X + Y$, she decides how much to invest in intermediates I for the next harvest, i.e "planting." The rest gets stored in the household. If a household invests I units on the farm, then T periods later, the farm will produce output according to the production function

$$Y = ZI^{\alpha}N^{1-\alpha} \tag{2.1}$$

where Z is a random shock, I is again the investment (e.g. fertilizer), and N is the stock of labor services that have been accumulated, which we detail to in the next section.

Labor Allocation Problem The second type of period are those in the interim between the planting and harvesting of crops. Once the farmer delineates resources between farm investment I and storage S, they are left with periods $t = 1, \ldots, T$ in which they must wait for the harvest to occur. During this time, a farmer affects eventual farm output by working. If she chooses to work a fraction n_t of their time on the farm at t, they contribute to the stock of labor N in the production function (2.1) through the constant elasticity of substitution aggregator

$$N = \left(\sum_{t=1}^{T} \varepsilon_t^{\frac{1}{\sigma}} n_t^{1-\frac{1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}}. \tag{2.2}$$

Here, ε_t is a shock to the marginal benefit of n_t for producing output. With the time $1 - n_t$ not utilized on the farm, the household works in the spot labor market for stochastic wage w_t. The processes for these two shocks $\{\varepsilon_t\}_{t=1}^T$ and $\{w_t\}_{t=1}^T$ are arbitrary, including any correlation between the two.

2.1 Recursive Formulation

Given the timing described above, we can write the household problem recursively. Denoting s^t as the sequence of shocks from periods $1, \ldots, t$ and $\pi(s^t)$ as the probability of receiving that sequence of shocks, the value of entering a planting period with resources X is given by

$$V(X) = \max \sum_{t=1}^{T} \sum_{s^t} \pi(s^t)\beta^t u\left(c(s^t)\right) + \sum_{s^T} \pi(s^T)\beta^T V(X'(s^T))$$

subject to:

$$\begin{aligned}
\forall s^t : c(s^t) \quad &\leq \quad (1-n(s^t))w(s^t) + (1-\delta)S(s^{t-1}) - S(s^t) \\
X \quad &\geq \quad S(s^0) + I \\
\forall s^T : X'(s^T) \quad &\leq \quad S(s^T) + Z(s^T)I^{\alpha}N(s^T)^{1-\alpha} \\
\forall s^T : N(s^T) \quad &\leq \quad \left(\sum_{t=1}^{T} \varepsilon(s^t)^{\frac{1}{\sigma}} n(s^t)^{1-\frac{1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}} \\
\forall s^t : S(s^t) \quad &\geq \quad 0
\end{aligned}$$

Throughout we will assume that u is strictly increasing, strictly concave and has a positive third derivative. The choice variables c, S and n are measurable with respect to the history of shocks up to that period.[4] However, the choice of productive investment I is made in the planting period and is irreversible. Also, Z is a random shock whose realization is only known in the period when output is realized.[5]

[4] Note also that our formulation implies that the sequences of shocks s^T are independent of one another. This assumption is not critical for the results, but simplifies exposition.

[5] An alternative model would be one in which Z becomes less and less uncertain as the period when output is realized approaches. That model is equivalent to one in which the process on ε changes over time, which is a special case of this formulation. Therefore, modeling Z as a one-time realization of uncertainty is without loss of generality.

2.2 Discussion

Before characterizing the model, we briefly digress to highlight the channels in the model that map to the data. We view flooding (or the weather that generates flooding) as the governing forces behind the shocks to the marginal product of farm labor, ε, and the outside wage, w. Intuitively, a flood eliminates the ability to access the outside labor market. In the context of the model, this is a realization of a low wage w. A bridge is then the elimination of the left tail of w realizations, which is both a decrease in variance and an increase in the mean realization. At the same time, substantial flooding may also make it more difficult for farmers to work on the farm. This would imply a positive correlation between the wage shocks w and the farm shocks ε. While the theoretical results do not depend on any relationship between w and ε, the magnitudes of the effects will be larger the more positively correlated the shocks. In the following sections, we show how changes to the distribution the shocks affect the key margins of interest: production, investment, and savings.

2.3 Savings Decision

The critical decision that households make is how to divide their farm output between two types of savings: low return storage and productive investment. Storage is subject to depreciation, but is safe. Therefore, households may find it optimal to accumulate a buffer stock to help maintain consumption levels when bad shocks are realized. Investment has higher expected returns, but cannot be accessed until the following harvest period and its return is uncertain at the time when the investment is made. Therefore, if a sequence of bad shocks are realized, households may experience sharp declines in consumption.

This choice of how to allocate agricultural output can, after some manipulation of the household's first order conditions, be written as

$$(1-\delta)^T + \sum_{t=1}^{T}\sum_{s^t}(1-\delta)^{t-1}\frac{\rho(s^t)}{\sum_{s^T}\eta(s^T)} = \alpha\sum_{s^T} Z(s^T)\left(\frac{I}{N(s^T)}\right)^{\alpha-1}\frac{\eta(s^T)}{\sum_{s^T}\eta(s^T)} \quad (2.3)$$

where $\rho(s^t)$ is the Lagrange multiplier on the non-negativity constraint for storage, and

$$\eta(s^T) = \beta^T \pi(s^T) V' \left(S(s^T) + Z(s^T) I^\alpha N(s^T)^{1-\alpha} \right). \tag{2.4}$$

Equation (2.3) simply states that households equate the marginal value of both types of investment. The value of an additional unit of storage is that it makes the household less likely to reach its non-negativity constraint and, therefore, lose their ability to mitigate consumption losses from negative shocks. Also, if the non-negativity constraint is not binding in period T, an additional unit of storage has (depreciated) value in the following season.

The marginal value of a unit of productive investment is the expected value of its marginal product. Importantly, risk enters its marginal value explicitly as variation in the derivative of the continuing value function. For example, although previous period realizations of the wage rate w do not enter this equation explicitly, they determine the final value of storage $S(s^T)$. Likewise, the farm productivity ε realizations affect the value of both $S(s^T)$ and $N(s^T)$.

Therefore, the type and degree of risk faced by households may affect their choice to allocate savings to higher expected return (but risky) investment or low expected return (but safe) storage. Intuitively, if households have a reduction in the risk they face, they may shift their savings from storage into investment. We formalize this idea in Proposition 1.

Proposition 1. *If the variance of w decreases or expected value of w increases, then for every X, the household increases farm investment I and decreases storage $S(s^0)$*

Proof. TO BE WRITTEN ■

Intuitively, the proof makes use of the fact that the main value of storage is to keep households away from the non-negativity constraint. Reaching that constraint is less likely when bad shocks are less severe or more unlikely. Both reductions in the variance of shocks and increases in the mean accomplish this, and therefore reduce the value of storage relative to investment. This links the construction of a footbridge to on-farm investment. High frequency variation in access to the labor market limits longer-horizon investment since it limits households' ability to self-insure. This increased investment has an intuitive effect on farm yield.

Corollary 1. *If the variance of* w *decreases or expected value of* w *increases, average farm yield* $\mathbb{E}\Big[Z(s^T)I^{\alpha}N(s^T)^{1-\alpha}\Big]$ *increases.*

Proof. Follows from Proposition 1. ■

Despite the fact that yields and investment increase, labor shifts off the farm. As the wage gets higher, farmers substitute toward investment for two reasons. First, there is the direct effect that the price of labor increases relative to the price of intermediates. The second force reenforces this first effect. Because the wage increases, households can better insure consumption. This further decreases the implicit price of intermediate investment.

Proposition 2. *If the variance of* w *decreases or expected value of* w *increases, average off-farm labor* $1 - N(s^T)$ *increases.*

Proof. TO BE WRITTEN ■

We now turn to testing the predictions of this model. We lay out our empirical treatment and strategy, and then use this to test the theoretical predictions described above.

3 Background and Village Selection

Our treatment directly confronts the theoretical predictions in the last section by building footbridges in rural Northern Nicaragua. These villages are located in mountainous areas that face seasonal flooding during the rainy season each year (May to November). During these periods, streams and rivers that are usually passable on foot rise very rapidly and may stay high for days or weeks. This flooding is unpredictable in its timing or intensity. Rainfall in the same location is a poor predictor of flooding, as rains at higher altitudes may be the cause of the flooding. Moreover, this period is also the main cropping season. Crops are planted at the beginning of the rainy season in May, and harvested in late October and early November. As in the model, the time between planting and harvesting is also the time in which the flooding shocks occur.

During these periods, some villages are cut off from access to outside markets. In particular, many villages have a river located between themselves and a larger, nearby city where agricultural markets and labor markets operate. When the river rises substantially, market access would require swimming across the river, which may be prohibitively dangerous and inhibit transportation of goods, or a long journey on foot to reach the market by another route.

We therefore investigate the impact of building footbridges that traverse these rivers. We do so by partnering with the non-governmental organization Bridges to Prosperity (B2P), that works to construct footbridges in these rural communities to solve some of the problems associated with flooding risk. Bridges to Prosperity provides engineering design, construction materials, and skilled labor to the village, as well as training in bridge maintenance. They ask members of the village to provide unskilled labor for construction, such as digging out the foundation of the bridge deck. Since a large part of the construction materials must be funded internally by Bridges to Prosperity, the number of bridges that can be constructed each year is limited.

Bridges to Prosperity takes requests from local village organizations and governments, then evaluates these requests on two sets of criteria. First, they determine whether the village has sufficient need. That is, are there enough people that live in the village and that would use the bridge to justify the expense of the project. These decisions are made by an in-country manager employed by the organization who inspects each site.

If the village passes the needs assessment, the country manager personally goes to the site to do an engineering assessment. The purpose of this assessment is to determine if a bridge can, in fact, be built at the proposed site. To be considered feasible, the required bridge cannot exceed a maximum span of 30 meters, and the banks of the river on each side must be of similar height (a differential not exceeding 3 meters). Moreover, the estimated high water mark (maximum height of the river when flooded) must be at least two meters below the proposed bridge deck. The assessment makes other considerations as well: bridges cannot cross power lines, and they avoid building in places where the river bends (as river bends may indicate a river changing its course).

We are comparing communities that passed both the feasibility and the needs assessments, and therefore received a bridge, to those that passed the needs assessment, but failed the feasibility assessment. The second group makes for an ideal comparison group for two reasons. First, the fact that both groups have similar levels of need is crucial, as need is both unobservable and is likely to be highly correlated with the treatment effects. Second, failure of the feasibility assessment is very unlikely to be correlated with any relevant village characteristics. For observable differences, we show that villages that do and do not receive bridges are balanced.

We study a total of fifteen villages. Of these, six passed both the needs and feasibility assessments, and therefore received bridges. The other nine passed only the needs assessment and did not receive a bridge. These villages are located in the provinces of Esteli and Matagalpa in northern Nicaragua.[6]

4 Data Collection and Design Validity

4.1 Data Collected

We conducted two types of data collection. First, we conducted in-person household-level surveys with all households in each of the fifteen villages. The first such wave took place in May 2014, just as that year's rainy season was beginning. This survey was to designed to give us an early indication of balance, and also to sign households up for the high frequency survey. In this May survey, for those that agreed to participate, we conducted followups every two weeks by phone. The more critical surveys covering the main rainy season were conducted in November 2014, November 2015, and November 2016. Bridges were constructed in Spring of 2015. Therefore for all villages we observe three rainy seasons. For those that receive a bridge, we observe one rainy season without a bridge and two rainy season with a bridge. We will primarily focus on these three surveys, as the first survey in May 2014 was primarily designed to (1) assess balance across groups and (2) register households for the phone surveys. We do include it when we consider the validity of our identification strategy.

To collect the in-person household surveys, we employed local Nicaraguan enumerators. Our strategy was to survey all households within three kilometers of the

[6] One might be concerned that a control village may be treated if they are sufficiently close to a treatment village. That is, if the control villagers are sufficiently close to a bridge to access it. This is not the case in any of the fifteen villages. They are all sufficiently far from one another to eliminate this issue.

proposed bridge site on the side of the river that was intended to be connected. In many villages, this implied a census of village households.

Participation in the first round of the survey was very high in general, with 97% of households agreeing to participate. This is true even though we offered no incentive for participation. Enumerators and participants were told that the purpose of the study was to understand the rural economy. We did not disclose our interest in the bridges because we suspected that would bias their answers, or may make them feel they are compelled to answer the survey when they would not otherwise want to participate. The number of households identified in each village varied widely, from a maximum of 80 to a minimum of 24.

Survey questions covered household composition, education, health, sources of income, consumption, farming choices (including planting, harvests, equipment and inputs), and business activities.

The second data collection was high frequency surveys. Because the floods are a high frequency and short term event, we also wanted to include these surveys to provide supporting evidence to the more detailed annual surveys and also validate the fact that flooding (and the bridge) was having an effect on income generating activities. We therefore carried out these surveys for 64 weeks, covering the rainy season before construction, along with the first dry and rainy seasons after construction. During the first wave, we solicited participation in cell phone followup interviews. Each household was called every other week, so that the maximum number of responses per household is 32. This high frequency survey covered income-generating activities, livestock purchases and sales, and food security questions over the past two weeks.

4.2 Balance and Validity of Design

As discussed above, we base our analysis on a comparison of villages that pass both the needs and feasibility assessment with those that pass only the needs assessment. The identification assumption is that the features required to pass the feasibility test are independent of any relevant household or village-level statistics. Using the first two waves of data, we run the regression

$$y_{ivt} = \alpha + \beta B_{vt} + \eta_t + \varepsilon_{ivt}$$

where $B_{vt} = 1$ if village v gets a bridge between $t = 2$ and $t = 3$. We consider a number of different outcomes, and show that households show no observable differences across the two groups. Table 1 produces the results, and we find no difference across households in build and no-build villages.

Table 1: Pre-Bridge Differences

	Constant	Bridge
Household Composition		
HH head age	43.34***	1.39
	(0.00)	(0.18)
HH head yrs. of education	6.40***	0.33
	(0.00)	(0.22)
No. of children	1.30***	-0.03
	(0.00)	(0.70)
HH size	4.18***	0.15
	(0.00)	(0.19)
Occupational Choice		
Agricultural production	0.47***	0.01
	(0.00)	(0.76)
Off farm work	0.58***	0.03
	(0.00)	(0.54)
Total wage earnings (C$)	865.14***	46.94
	(0.00)	(0.74)
Farming		
Corn harvest	16.66***	0.43
	(0.00)	(0.88)
Bean harvest	12.09***	-1.79
	(0.00)	(0.26)
Plant corn?	0.17***	0.01
	(0.00)	(0.62)
Plant beans?	0.16***	-0.03
	(0.00)	(0.23)

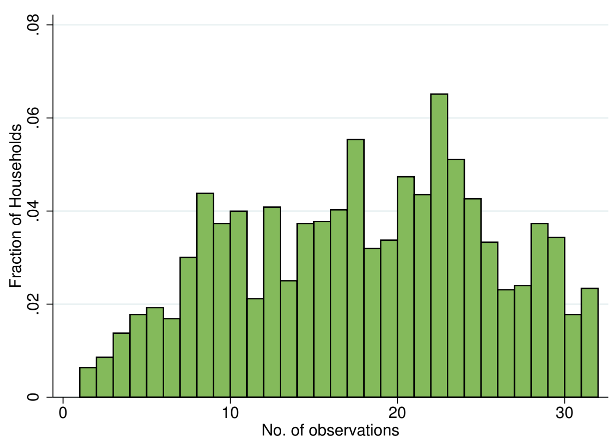
p-values in parentheses. Here, we do not cluster the standard errors as to give the model the greatest chance of finding a difference between the two groups.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5 High Frequency Effects of a Bridge

We begin by assessing the immediate affect of flooding and the impact of a bridge. To do so, we use the high frequency data to considering income realizations during floods. Before moving forward, two issues are worth highlighting. First, the high frequency data is not representative of the villages under study as not every individual has a cell phone. However, the households that participate are extremely close to population averages except for household head age. As one might suspect with a cell phone-based survey, those that agreed were slightly younger. The average age of a household that agreed to participate was 37 years old, compared to the average of 43 in the population as a whole. On other margins – occupation, farming, etc. – there is no statistical difference between those that participated and did not. Second, it is an unbalanced panel of individuals as not everyone answered the phone each time. Figure 1 plots the histogram of the number of observations per household in the high frequency data. The minimum is 1, the maximum is 32 (also the maximum possible number of responses), and the average is 12.

Figure 1: Number of Observations per Household



Despite these issues, the data still provides useful information on high frequency outcomes. To assess the impact of flooding on different outcomes, we run regressions of the form

$$y_{ivt} = \alpha + \beta B_{vt} + \gamma \Big(B_{vt} \times F_{vt} \Big) + \theta \Big(NB_{vt} \times F_{vt} \Big) + \eta_t + \delta_i + \varepsilon_{ivt}. \tag{5.1}$$

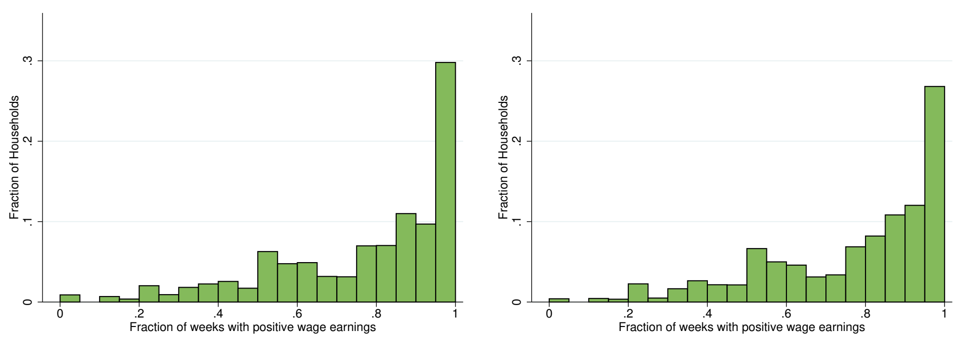
The variable $B_{vt} = 1$ if village v has a bridge in week t, while $NB_{vt} = 1 - B_{vt}$ is the "no bridge" variable. The variable $F_{vt} = 1$ if village v is flooded at week t, while η_t and δ_i are week and individual fixed effects. Throughout, we use a wild bootstrap cluster at the village level.

5.1 Changes in Income Realizations

We begin by considering changes in income. First, labor income is ubiquitous even among farming households. Figure 2 is a histogram counting the share of weeks each household receives positive labor market income. Indeed, despite the fact that about half of households farm some kind of crop, most are also active in the labor market. When we rank households by the share of periods we observe positive income, even the fifth percentile household receives labor market income in 21 percent of the periods we observe it.[7]

Figure 2: Fraction of weeks with labor market income

(a) All households (b) Only households with ≥ 10 observations



We therefore ask how income realizations change during flooding episodes, and how the bridge changes the results. We use two measures of income in regression (5.1): amount earned in the past two weeks and an indicator equal to one if no income was earned.

Table 2 illustrates the effects of flooding on contemporaneous income realizations. First, having a bridge in the absence of a flood does not increase income relative to

[7]One possibility is that survey non-response is correlated with realizations of zero income, thus biasing our results toward observing positive income. This would be the case if heavy rains strongly reduced cell coverage, for example. In Appendix A we show that there is no relationship between flooding and the likelihood of response to surveys.

Table 2: Effects of Flooding on Income

	Household Income	Household Income	No Income Earned	No Income Earned
Flood × No Bridge	-69.949	-141.518*	0.052	0.070**
	(0.487)	(0.097)	(0.214)	(0.041)
Flood × Bridge	169.560	65.453	-0.091***	-0.038*
	(0.195)	(0.592)	(0.004)	(0.082)
Bridge	57.815	186.278**	0.060*	0.061*
	(0.750)	(0.033)	(0.091)	(0.082)
Constant	1236.573***		0.182*	
	(0.000)		(0.059)	
Observations	6756	6756	6756	6756
Individual F.E.	N	Y	N	Y
Week F.E.	Y	Y	Y	Y

p-values in parentheses computed using the wild cluster bootstrap-t with 1000 simulations, clustered at the village level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

households in villages without a bridge. This is shown by the insignificant effect on the bridge variable. However, when there is a flood, this changes. Income drops by C$184 ($p = 0.03$) during a flood in the absence of a bridge, a decrease of nearly 20 percent of its no-flood baseline. This effect is not present in villages with a bridge. Here, a flood has no statistical effect on the average household income realization. That is, the flood has no effect on average income realizations in the presence of a bridge, but a negative effect without one.

The same pattern holds when one considers the fraction of people who earn no income in the preceding two weeks. The likelihood of earning no income increases by 13 percentage points ($p = 0.00$) when a flood occurs in villages without a bridge, from 0.21 to 0.33. In villages with a bridge, the fraction is 0.21 regardless of whether or not there is a flood. This seems the critical margin that the bridge affects. Figure 3 plots the density of income realizations before and after bridge construction, both during periods of no flooding (left panel) and during flooding (right panel). The critical difference is the drop in zero income realizations during floods, suggesting that the bridge limits the inability to access labor markets during a flood.

5.2 Food Security

Sometimes households are not allowed to buy as much of a staple crop as they desire. This occurs when vendors are worried about running out of stock. Table 3 shows

Figure 3: Density of Income Realizations

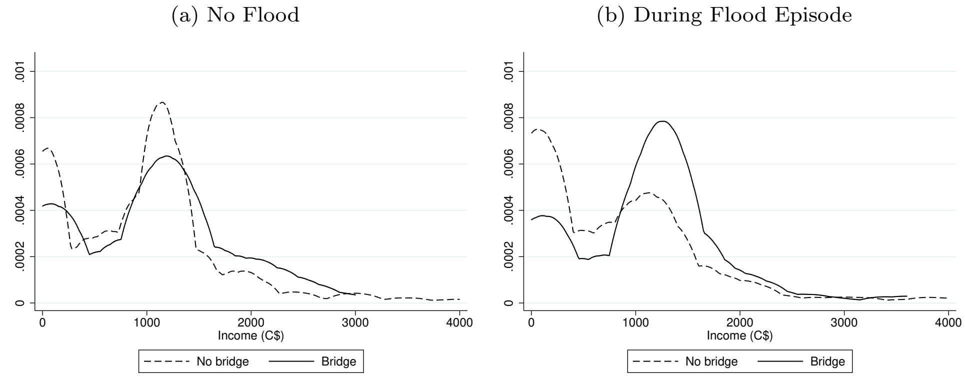


Table 3: Food Rationing During Floods

	Corn Rationed	Corn Rationed	Bean Rationed	Bean Rationed
Flood × No Bridge	0.125***	0.110**	0.142***	0.147***
	(0.000)	(0.013)	(0.000)	(0.002)
Flood × Bridge	0.000	0.014	-0.004	0.027
	(0.876)	(0.760)	(0.308)	(0.525)
Bridge	-0.104***	-0.052	-0.117***	-0.051*
	(0.004)	(0.033)	(0.004)	(0.069)
Constant	0.108***		0.126***	
	(0.000)		(0.000)	
Individual and Week F.E.	No	Yes	No	Yes
Observations	6250	6250	6250	6250

p-values in parentheses computed using the wild cluster bootstrap-t with 1000 simulations, clustered at the village level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

that this behavior is concentrated in flooding episodes. This may occur because food vendors want to prevent a "run" on the food supply when future access to food is uncertain. Once a village has a bridge, this rationing ends abruptly. For instance, there are no observations of households being rationed in corn purchases in any village that has a bridge. Moreover, the frequency of bean and rice rationing is also greatly mitigated by a bridge. This is consistent with less worry about access to food during flooding times, because households and food vendors are able to access outside food supplies when they are necessary.

5.3 Discussion and Relation to Model

Taken together, the results highlight that critical margins required for the theory posed in Section 2 also hold in the data. First, flooding shocks lower income. Second, a bridge mitigates the negative effect on income and also increases food security. Given the model assumptions confirmed here, the model predicts that these high frequency events translate into lower frequency changes on the farm. Namely, an increase in farm investment and a decrease in storage of crops. We next turn to assess whether these outcomes hold in the data.

6 Longer Run Impacts from Annual Surveys

From the high frequency data, we know two things. First, most households use the labor market in some capacity. Second, income drops in response to a flood, though a bridge mitigates this effect. We next turn to our larger, annual surveys to assess the impact on longer-term outcomes. Section 6.1 covers the labor market results, while Section 6.3 covers on-farm decisions. Throughout, we use the three surveys conducted at the end of the rainy season from 2014 to 2016. We refer to them as $t = 0, 1, 2$ throughout this section. Our baseline regression specification is

$$y_{ivt} = \alpha + \beta B_{vt} + \eta_t + \delta_i + \varepsilon_{ivt} \tag{6.1}$$

where $B_{vt} = 1$ if a bridge is built, η_t and δ_i are time and individual fixed effects, and standard errors are clustered at the village level using a wild cluster bootstrap-t.

6.1 Labor Market Impact

We begin by considering wage earnings. Panel A of Table 4 shows the results for total income, along with its components of the daily wage rate and days worked. First, earnings increase by C$308 ($p = 0.06$). This is almost entirely accounted for by an increase in income earned outside the village, consistent with the bridge providing better access to outside markets. Earnings outside the village increase by C$295 ($p = 0.00$), while earnings inside the village decrease slightly by C$42 ($p = 0.72$).

These results are accounted for by changes in days worked, not by changes in the daily wage rate. Households work 1.25 extra days outside the village ($p = 0.00$), and 0.33 days less inside the village ($p = 0.41$). We find no statistically significant effects on realized wages either within or outside the village.

Panel B of Table 4 distinguishes between intensive and extensive margin changes by interacting the bridge indicator with an indicator for positive earnings at baseline. In terms of total earnings, we see a significant movement of households into the labor market. Households with no baseline labor market earnings see an increase of C$405 ($p = 0.01$) compared to a statistically insignificant increase of C$221 ($p = 0.38$) among households with positive earnings. Again, this is driven by changes in days worked. Those with no baseline earnings increase days worked by 1.60 ($p = 0.00$), while those with baseline earnings increase days worked by a statistically insignificant 0.45 ($p = 0.53$). These results are consistent with households shifting from labor markets inside the village to outside the village. Indeed, among those with positive baseline earnings, we see a C$362 ($p = 0.00$) increase in earnings and a 1.36 increase in days worked outside the village, but also a decrease in earnings of C$205 ($p = 0.31$) and 1.02 days ($p = 0.10$) within the village. On the other hand, new entrants into the labor market more strongly move toward earnings outside the village, where we find an increase of C$ 295 ($p = 0.00$) and 1.72 days ($p = 0.00$), and smaller changes within the village.

Table 4: Effects on Market Income, by Source

Panel A:	Total Earnings			Earnings Outside Village			Earnings Inside Village		
Earnings	Earnings	Wages	Days	Earnings	Wages	Days	Earnings	Wages	Days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Build	307.59^{*}	-21.25	1.00^{*}	295.24^{***}	-24.84	1.25^{***}	-41.76	-54.75	-0.33
	(0.064)	(0.359)	(0.062)	(0.000)	(0.361)	(0.000)	(0.717)	(0.293)	(0.405)
Constant	1025.73^{***}	275.77^{***}	3.52^{***}	295.00^{***}	168.36^{***}	1.72^{***}	661.11^{***}	263.43^{***}	1.65^{***}
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Panel B: Intensive and	Total Earnings			Earnings Outside Village			Earnings Inside Village		
Extensive Margins	Earnings	Wages	Days	Earnings	Wages	Days	Earnings	Wages	Days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Build × Pos. Earnings	221.12		0.45	362.44^{***}		1.36^{***}	-205.18		-1.02^{*}
	(0.380)		(0.532)	(0.000)		(0.006)	(0.305)		(0.098)
Build × Zero Earnings	404.65^{**}		1.60^{***}	220.33^{**}		1.12^{***}	140.04		0.45
	(0.010)		(0.002)	(0.022)		(0.002)	(0.151)		(0.107)
Constant	1025.73^{***}		3.52^{***}	295.00^{***}		1.72^{***}	661.11^{***}		1.65^{***}
	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)		(0.000)

Table notes: Pos. Earnings is an indicator for positive baseline labor market earnings, either inside or outside village. *Zero Earnings* is 1-*Pos. Earnings*. Wages are not included in Panel B since the zero earnings group has no defined wages in baseline. p-values in parentheses are clustered using the wild cluster boostrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

6.2 Occupational Choice and Misallocation

Table 4 shows that the impact on earnings is in large part driven by new entrants into the labor market. In our surveys, we asked individuals about their primary and secondary occupations, and use them to categorize households into four broad economic activities. Households are consider agricultural households if they only operate a farm, wage work households if they only have wage income (either on someone's farm or in a non-agricultural firm), both, or neither. Consistent with the previous results, we see substantial amounts of movement between farming and wage work, suggesting that entry and exit of employment activities are important parts of the economy. While both activities are persistent, they are not perfectly so. Thirty percent of households with wage income at baseline have no wage income in periods one and two, and the same number holds true for farming. Moreover, twenty-five percent of households with no wage employment at baseline have some wage income at periods one or two, and similarly 29 percent engage in farming in periods one or two despite no farming at baseline.

We therefore begin by assessing the impact of the bridge on the persistence of sectoral employment. We ask whether individuals engaged in wage or agricultural work are more or less likely to remain engaged in these activities in response to the bridge. In particular, we run the regressions

$$\begin{aligned}
\mathbb{1}[Wage]_{ivt} &= \alpha + \beta\Big(B_{vt} \times \mathbb{1}[Wage]_{iv0}\Big) + \gamma\Big(B_{vt} \times (1 - \mathbb{1}[Wage]_{iv0})\Big) + \eta_t + \delta_i + \varepsilon_{ivt} \\
\mathbb{1}[Agr]_{ivt} &= \alpha + \beta\Big(B_{vt} \times \mathbb{1}[Agr]_{iv0}\Big) + \gamma\Big(B_{vt} \times (1 - \mathbb{1}[Agr]_{iv0})\Big) + \eta_t + \delta_i + \varepsilon_{ivt}
\end{aligned}$$

where $\mathbb{1}[Wage]_{ivt} = 1$ if a household is engaged in labor market activities at time t, while $\mathbb{1}[Agr]_{ivt} = 1$ if the household is engaged in agricultural activities.[8] We wish to see whether baseline engagement in the sector predicts post-treatment engagement in the sector. The results are presented in Table 5. Interestingly, we find that the bridge makes households significantly less likely to engage in both agricultural and labor market activities. Among households not engaged in farming at baseline, those who receive a bridge are 20 percentage points more likely to begin farming ($p = 0.01$),

[8] Note that these are not mutually exclusive, as households can be engaged in both or neither.

and similarly, those not engaged on wage work are 29 percentage points more likely to being doing so ($p = 0.00$). Correspondingly, those engaged in both farming and wage work at baseline are less likely to remain so after the bridge is built.

Table 5: Effects on Persistence of Activities

	Agriculture		Labor Market	
	(1)	(2)	(3)	(4)
Build × Engaged	-0.311***	-0.165**	-0.134**	-0.026
	(0.000)	(0.020)	(0.012)	(0.642)
Build × Not engaged	0.202**	0.050	0.285***	0.155**
	(0.013)	(0.515)	(0.000)	(0.043)
Engaged		0.712***		0.752***
		(0.000)		(0.000)
Constant	0.501***	0.139***	0.551***	0.137***
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	1,601	1,601	1,601	1,601
Time F.E.	Y	Y	Y	Y
Household F.E.	Y	N	Y	N

Table notes: Engaged = 1 if the household is engaged in the relevant activity at baseline, and *Not engaged* is the converse. p-values in parentheses are clustered using the wild cluster boostrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

To assess this in more detail, we decompose the occupational space into the four mutually exclusive groups – only agricultural production, only labor market earnings, both, and neither – and rerun the above regressions

$$o_{j,ivt} = \alpha + \sum_{j=1}^{4} \beta_j \Big(B_{vt} \times o_{j,iv0} \Big) + \eta_t + \delta_i + \varepsilon_{ivt} \qquad \text{for } j \in \{1, 2, 3, 4\}$$

Here $o_{j,ivt}$ is an indicator that household i is engaged in activity $j \in \{1, 2, 3, 4\}$ defined above. The results are in Table 6, and a number of results emerge. First, the bridge induces households to engage in market economic activity. For those currently engaged in no economic activity the bridge has a strong positive effect on engaging in either agriculture or wage work, and a strong negative effect (-0.547, $p = 0.00$) on engaging in no market activity. Second, the bridge allows households to specialize, whether it be in farming or wage work. For households engaged in both farming and wage work (e.g. "both"), there is a strong positive effect of the bridge on the likelihood of engaging in *only* farming or wage work. Moreover, the effect of the bridge on engaging in both is negative and significant (-0.505 with $p = 0.00$). Lastly, as in the previous set of results in Table 5, the bridge generates substantial switching across these categories. To see this, one can simply read the negative, statistically significant effects off the diagonal of Table 6. For any current economic activity, a bridge makes it significantly less likely that a household is engaged in that same activity post-treatment. Again, this is consistent with misallocation across households.

Table 6: Effects on Persistence of Activities, Mutually Exclusive Categories

	Agriculture only	Wage work only	Both	None
	(1)	(2)	(3)	(4)
Build × Agr only	-0.366***	0.225***	0.062*	0.080*
	(0.000)	(0.001)	(0.133)	(0.069)
Build × Wages only	0.044	-0.218***	0.126*	0.049
	(0.224)	(0.001)	(0.066)	(0.175)
Build × Both	0.178***	0.275***	-0.505***	0.052
	(0.009)	(0.000)	(0.000)	(0.417)
Build × None	0.268**	0.204***	0.076	-0.547***
	(0.024)	(0.000)	(0.201)	(0.000)
Constant	0.346***	0.396***	0.156***	0.103***
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	1,601	1,601	1,601	1,601
Time F.E.	Y	Y	Y	Y
Household F.E.	Y	Y	Y	Y

Table notes: Engaged = 1 if the household is engaged in the relevant activity at baseline, and *Not engaged* is the converse. p-values in parentheses are clustered using the wild cluster boostrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

6.3 On-Farm Imapct

We first consider intermediate input use on farms, with results presented in Table 7. We consider intermediate input (fertilizer plus pesticide) expenditures, and also the two components individually. In odd columns, we provide the average effect of the bridge, while in even columns we decompose the treatment effects based on whether or not the household is operating a farm at baseline.

First, we see a substantial increase in intermediate expenditure, mostly driven by changes in fertilizer. Intermediate expenditures increase by C$646 ($p = 0.01$) on a baseline of C$934, and its components fertilizer and pesticide increase by C$438 ($p = 0.01$) and C$153 ($p = 0.29$) respectively. Interestingly, however, when we decompose the results based on baseline farming, we find that there are similar changes on both continuing and new farmers. Intermediate expenditures increase by C$674 ($p = 0.13$) among continuing farmers and C$614 ($p = 0.04$) among new farmers, relative to those in villages without a bridge. Similar results are found when considering fertilizer. Continuing farmers increase fertilizer expenditure by C$465 ($p = 0.03$) and new farmers by C$407 ($p = 0.03$). We do see some difference in changes in pesticide spending, however, where changes across treatment and control is primarily driven by new farmers. Note that these results are in contrast to changes labor market earnings in Table 4, which were primarily driven by new entrants to wage work. Thus, in addition to allowing individuals to better sort into their preferred occupation, it also allows farmers who do not switch to invest more on their farms.

Table 7: Farm Input Usage

	Intermediate Spending		Fertilizer Spending		Pesticide Spending	
	(1)	(2)	(3)	(4)	(5)	(6)
Build	646.48**		437.81***		152.94	
	(0.013)		(0.005)		(0.286)	
Build × Farming		674.72		464.66**		65.02
		(0.134)		(0.029)		(0.777)
Build × No farming		614.18**		407.22**		253.95**
		(0.042)		(0.034)		(0.045)
Constant	934.25***	934.14***	598.10***	597.99***	344.31***	344.67***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	1,601	1,601	1,601	1,601	1,601	1,601
Time F.E.	Y	Y	Y	Y	Y	Y
Household F.E.	Y	Y	Y	Y	Y	Y

Table notes: Farming = 1 if the household is engaged in any crop production at baseline. p-values in parentheses are clustered using the wild cluster boostrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

We therefore next consider changes in harvest for maize and beans, measured in total quintales (100 kilograms) harvested.[9] The results are in Table 8. While the point estimates imply that harvest amounts increase in response to the bridge, we do not

[9] In Appendix A.2, we show that the bride has no effect on crop selection by farmers, hence our focus directly on yields here.

observe any statistically significant effects. This could be due to the relatively small number of clusters or unobserved aggregate shocks that limit the return to investment, a point recently emphasized in Rosenzweig and Udry (2016). In Appendix A.3, we consider farm yields of the same crop, measured in quintales per manzana (approximately 1.73 acres). This eliminates a substantial fraction of observations, as those with no harvest have no land in production, and thus the notion of yield is undefined. Nevertheless, we find positive significant increases in yield in that regression.

Table 8: Harvest of Staple Crops

	Maize		Beans	
	(1)	(2)	(3)	(4)
Build	1.63		1.06	
	(0.234)		(0.123)	
Build × Farming		2.11		0.96
		(0.380)		(0.363)
Build × No farming		1.05		1.18
		(0.164)		(0.206)
Constant	2.89***	2.88***	1.64***	1.64
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	1,601	1,601	1,601	1,601
Time F.E.	Y	Y	Y	Y
Household F.E.	Y	N	Y	N

Table notes: Farming = 1 if the household is engaged in any crop production at baseline. p-values in parentheses are clustered using the wild cluster boostrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

6.4 Savings Decisions

Consistent with the model developed in Section 2, households have both higher off-farm income and invest more on their farms. The last prediction of the model is that households decrease the amount of crop stored in the household. This is the main form of savings in these rural villages. To define storage, we asked first about the amount harvested of each crop. We then asked what part was sold, used to pay debt, gifted, or given as land payment. We define storage as the fraction of the harvest not distributed, as this is the correct model counterpart. That is, harvest net of sales, debt payments, gifts, and land payments.[10] Any household with no crop production is given a value of zero in this regression. Table 9 shows how bridges affects savings behavior.

Table 9: Farm Savings Choices

	Maize		Beans	
	(1)	(2)	(3)	(4)
Build	-0.071**		-0.083**	
	(0.032)		(0.026)	
Build $\times$ Farming		-0.072		-0.101
		(0.138)		(0.121)
Build $\times$ No farming		-0.069**		-0.061**
		(0.038)		(0.027)
Constant	0.942***	0.942***	0.928***	0.928***
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	1,601	1,601	1,601	1,601
Time F.E.	Y	Y	Y	Y
Household F.E.	Y	N	Y	N

Table notes: Farming = 1 if the household is engaged in any crop production at baseline. p-values in parentheses are clustered using the wild cluster boostrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Consistent with the model, farmers store a significantly smaller proportion of their harvest. In build villages, farmers save 7 percentage points less of corn harvest than those in non-build villages ($p = 0.03$), and 8 percentage points less ($p = 0.03$) of bean harvest. This affect is found in both continuing and new farmers, though the higher variance among continuing farmers implies that only new farmers have a significant effect from the bridge. Among new farmers, the bridge induces a 7 percentage point decrease in maize storage ($p = 0.04$) ad a 6 percentage point decrease in bean storage ($p = 0.03$). Among continuing farmers, we find similar decreases of -0.07 ($p = 0.14$) and -0.10 ($p = 0.12$), and though we slightly miss statistical significance at the 10 percent level.

We also consider other household financial transactions. For example, households could substitute storage for debt if there was any associated change in (formal or

[10]In the Appendix we present the results when we define storage as the amount of each crop currently held in the household. The results are quite similar. However, this measure is net of any already-consumed harvest and thus is not the total measure of harvest stored. For this reason, we prefer the in-text measure of storage.

informal) financial markets. We therefore consider debt positions in the household, including to banks, businesses or any other institutions or individuals. Table 10 includes evidence on household debt levels. Regressions 1-4 are outstanding debt levels in córdoba, while regressions 5-8 are indicators for any outstanding debt. Consistent with the increased wealth level of households that receive a bridge, we find a decrease in outstanding bank debt, both on the intensive and extensive margins. Bank debt decreases by C$573 ($p = 0.03$) among households in villages that receive a bridge, and they are 7.6 percentage points less likely to have debt owed to a formal financial institution ($p = 0.09$). Along other dimensions, including to local businesses or other debt, we find no change in either likelihood of outstanding debt or the amount.

Table 10: Household Debt

	Outstanding Debt				Any outstanding?			
	Total	Bank	Business	Other	Total	Bank	Business	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Build	-788.18***	-573.08**	-41.65	-0.98	-0.090	-0.076*	-0.036	-0.029
	(0.005)	(0.025)	(0.538)	(0.916)	(0.292)	(0.088)	(0.614)	(0.743)
Constant	1291.00***	936.63	285.91***	29.43***	0.413***	0.169***	0.297***	0.299***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	1347	1347	1347	1347	1347	1347	1347	1347
Time F.E.	Y	Y	Y	Y	Y	Y	Y	Y
Household F.E.	Y	Y	Y	Y	Y	Y	Y	Y

Table notes: Total is the sum of bank, business, and other debt. p-values in parentheses are clustered using the wild cluster boostrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

7 Conclusion

We consider the impact of new footbridges in rural Northern Nicaragua. The villages that we study are subject to sporadic seasonal flooding that cuts off households from local markets. Working with an NGO partner, we construct footbridges to link these villages back to markets, and use the small but critical engineering requirements to identify the effect. Despite the fact that we construct only 6 bridges in 15 villages, we identify a number of important changes among households. First, the bridge eliminates any change in income realizations during floods. When we consider longer run outcomes, the bridges induce substantial changes in economic activity, as the persistence of both farming and wage work decrease. Moreover, farmers increase fertilizer and pesticide investment, while storing less. This is consistent with the bridge as a income smoothing technology.

The bridge induces a reduction in both extensive and intensive margin misallocation. Finding evidence of these multiple channels is important for policy, given the variety of income-generating activities in rural areas (World Bank, 2008). Given the relatively small sample, however, we have little to say about general equilibrium effects here. This is an important component of understanding the full effect of such interventions, and we leave this to future research.

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A More Results and Robustness

A.1 How high frequency survey response rates change during floods

Figure 2 in the text shows that almost all individuals in the high frequency survey use the labor market to some degree. However, our survey is biased toward finding that result if floods decrease the likelihood of answering the survey. To show that this is not the case, we run the regression

$$\mathbb{1}[answer]_{ivt} = \alpha + \beta Flood_{vt} + \eta_t + \delta_i + \varepsilon_{ivt}.$$

where $\mathbb{1}[answer]_{ivt} = 1$ if an individual answers the survey in week t, and is zero otherwise. The results are in Table 11. We find no statistically different effect of flood on the response rate, and the point estimate is small. If we remove time fixed effects we are able to generate a negative response to flooding, but again, the point estimate is quite small.

To further emphasize this point, Figure 4 reproduces Figure 2 in the main text with one key difference. Here, we assume that every period a household does not answer the survey, they received zero income that period. That is, we replace all missing values with zeros. This extreme assumption generates the lowest possible bound on the results driven by the unbalanced nature of the panel.

Table 11: Effect of flooding on survey response

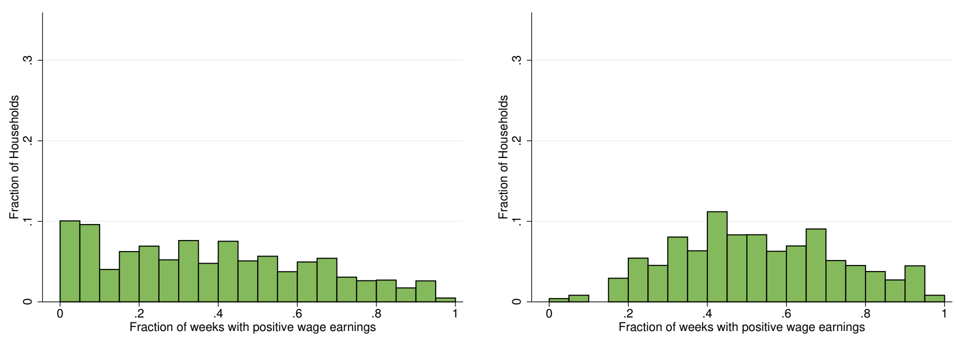
	(1)	(2)
Flood	0.026	-0.025**
	(0.151)	(0.035)
Constant	0.580***	0.498***
	(0.000)	(0.002)
Observations	13,705	13,705
Individual F.E.	Y	Y
Week F.E.	Y	N

Table notes: p-values in parentheses are clustered using the wild cluster boostrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 4: Fraction of weeks with labor market income

(a) All households

(b) Only households with $\geq$ 10 observations



Naturally, this shifts the distribution toward zero. However, even when considering all households, the fifth percentile household still receives labor market income in 3 percent of its observations. The median household receives labor market income in 36 percent of weeks. Thus, individuals are still utilizing the labor market to varying degrees of intensity. When we condition on households that have at least ten observations, the numbers look quite similar to the text. The fifth percentile household receives labor market income in 21 percent of weeks. Thus, even under the most extreme assumptions about non-response, the labor market is still an important part of most households income strategy.

A.2 Crop Planting Decisions

We look at planting decisions, where we consider the two key staple crops maize and beans along with the main cash crop in Northern Nicaragua, coffee.[11] The outcome variable here is an indicator equal to one if the crop is planted (not necessarily harvested), and the results are in Table 12.

Table 12: Planting Decisions

	Maize		Beans		Coffee	
	(1)	(2)	(3)	(4)	(5)	(6)
Build	0.003		0.080		0.004	
	(0.606)		(0.178)		(0.766)	
Build × Farming		-0.034		0.045		-0.003
		(0.679)		(0.598)		(0.863)
Build × No farming		0.047		0.123**		0.127
		(0.159)		(0.012)		(0.523)
Constant	0.217***	0.218***	0.272***	0.272***	0.018***	0.018***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Observations	1,601	1,601	1,601	1,601	1,601	1,601
Time F.E.	Y	Y	Y	Y	Y	Y
Household F.E.	Y	Y	Y	Y	Y	Y

Table notes: Farming = 1 if the household is engaged in any crop production at baseline. p-values in parentheses are clustered using the wild cluster boostrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.3 Farm Yields in Response to Bridge

A.4 Using "current storage" as a direct measure of stored crops

Table 14 shows storage levels using a direct measure of storage. The measure of storage used here is

$$\frac{\text{Current Quantity Stored in Household}}{\text{Total Quantity Harvested}}.$$

This measure does not measure the total amount of harvest stored, as some was consumed prior to the survey wave. Nevertheless, the results are similar to those in the main text.

[11] We considered other cash crops as well, and find similar results to coffee.

Table 13: Harvest of Staple Crops

	Maize		Beans	
	(1)	(2)	(3)	(4)
Build	13.47**		3.26***	
	(0.019)		(0.003)	
Build × Farming		15.07**		2.89**
		(0.018)		(0.022)
Build × No farming		6.27		6.48
		(0.112)		(0.181)
Constant	9.10***	9.28***	4.36***	4.22***
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	313	313	324	324
Time F.E.	Y	Y	Y	Y
Household F.E.	Y	N	Y	N

Table notes: Farming = 1 if the household is engaged in any crop production at baseline. p-values in parentheses are clustered using the wild cluster boostrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 14: Farm Savings Choices

	Fraction Corn Saved		Fraction Beans Saved	
	(1)	(2)	(3)	(4)
Build	-0.10*		-0.10*	
	(0.08)		(0.06)	
Build × Near		-0.12*		-0.12**
		(0.10)		(0.04)
Build × Far		-0.08		-0.08
		(0.38)		(0.24)
Far		-0.01		0.00
		(0.94)		(0.90)
Constant	0.85***	0.85***	0.90***	0.90***
	(0.00)	(0.00)	(0.00)	(0.00)
Observations	926	926	926	926

p-values in parentheses computed using the wild cluster bootstrap-t with 1000 simulations, clustered at the village level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$