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

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Abstract

We estimate the impact of new infrastructure in rural Nicaraguan villages facing seasonal flooding risk that unpredictably eliminate access to outside markets. We build bridges 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 eliminate uncertainty in market access driven by floods: during flood episodes in control villages labor market earnings decrease by 15 percent, while there is no change in treatment villages. We also find substantial reallocation of activities between farming and wage work, increased fertilizer spending and yields on farms, and lower savings. We show that these results are outcomes of a model with occupational choice and risky farm investment, where bridges act as a consumption smoothing technology by providing more consistent off-farm labor market access.

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1 Introduction

A large fraction of households in the developing world live in rural areas that are less productive than urban areas in the same countries (Restuccia, Yang and Zhu, 2008; Gollin, Lagakos and Waugh, 2014). A growing literature has pointed out large gains from the reallocation of factors from both rural to urban areas and within rural economies.[1] One response to these issues is building infrastructure, which allows for easier movement of goods and people across space. However, assessing the full impact of infrastructure is difficult. Placement tends to be endogenous, as most infrastructure studies focus on large-scale government programs. Moreover, the heterogeneity of income-generating activities in rural areas require detailed data to understand the complete impact of new infrastructure (Foster and Rosenzweig, 2007).

In this paper, we directly consider the impact of one type of new infrastructure – bridges – in rural Nicaragua. We build footbridges that connect rural villages to urban centers. These bridges traverse rivers that are subject to seasonal flooding that routinely and unpredictably eliminates access to outside food, product, and labor markets, a common issue in the developing world.[2] We conduct household-level surveys over three years before and after bridge construction, along with 64 weeks of phone surveys with a subset of households. This allows us to focus on multiple margins affected by the bridge, and assess the underlying channels at work.

Our identification strategy is based on the fact that there are many villages that need bridges, but some cannot be constructed 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 2 and show that the topographical features that allow for construction are orthogonal to any relevant household or village characteristics. A major barrier to studying transportation infrastructure as an intervention is the high construction cost. This is true in our context as well, where each bridge costs approximately $40,000. As such, our study includes 900 households from 15 villages surveyed over three years. Since we have a

[1] See, for instance, work by Restuccia and Santaeulalia-Llopis (2017) on land misallocation and Adamopoulos et al. (2017) on inefficient selection across sectors.

[2] This issue is considered a major rural development hurdle by both international policy organizations and citizens of Nicaragua (World Bank, 2008*a*). More broadly, seasonal flooding or monsoons in the tropics have long been discussed as a contributor to poverty. See Kamarck (1973) for an early study on agriculture and health issues in the tropics.

small number of clusters, we use the wild bootstrap cluster-t procedure from Cameron, Gelbach and Miller (2008) throughout.

Despite the small number of clusters, we find economically and statistically significant effects. We first show using high frequency data that the bridge eliminates market access risk during flood episodes. During a flood, average labor market earnings decrease by 15 percent in the absence of a bridge. In villages with a bridge, there is no change in average labor market earnings during a flood. Similar results hold when we consider the proportion of households earning no income in a given period. We confirm these results in the annual surveys, as households earn nearly 30 percent more from wage work in response to a bridge. The result is entirely driven by changes in days worked outside the village, while wages remain the same. These results show that (1) floods generate uncertain access to labor markets and (2) bridges eliminate this uncertainty and thus increase earnings in the labor market.

This labor market access has important effects that spill over into the agricultural sector. First, we find that farmers spend nearly 50 percent more on intermediate inputs like fertilizer and pesticide in response to a bridge. Moreover, yields increase by 60 percent on staple crops. One obvious explanation for this result would be that the bridges decreased trade costs. However, this is unlikely to be the case here bridges do not change market access outside of flooding periods. To the extent that crops and farm inputs are storable over a number of days, the price effects common in standard models of trade are unlikely to hold here. Consistent with this, we find no changes in agricultural output or input prices in villages that receive a bridge.

We therefore build a dynamic model that links these two results together, and derive testable predictions to confirm the underlying mechanisms. The basic idea is predicated on the fact that there is substantial risk – unaffected by the bridge – that negatively affects agriculture.[3] In particular, farmers are required to choose fertilizer investment before the realization of the shock. The absence of insurance implies that larger *ex ante* investment generates lower *ex post* consumption in the event of a low shock realization. Farmers internalize this fact and limit their exposure to *ex post* consumption risk by limiting *ex ante* investment. Once a bridge provides labor mar-

[3] Rainfall variation, for example, limits fertilizer spending (Mobarak and Rosenzweig, 2012; Karlan et al., 2014). A bridge has no direct impact on rainfall variation.

ket access, however, farmers can adjust labor in response to bad farm shocks, in the spirit of empirical work by Kochar (1999). This encourages *ex ante* fertilizer expenditures in treatment villages, as it limits the downside risk of investment. On average, the increased insurance provided by the bridge increases agricultural investment and yields.

The model provides a number of testable implications that confirm the intuition above. First, and most critical, the model predicts that a bridge generates two-way flows between wage work and farming. That is, it predicts that a bridge increases occupational flows from farming to wage work (as the wage increases), but also wage work to farming. The latter effect follows a similar logic to work on financial frictions and occupational choice (Banerjee and Newman, 1993; Buera, Kaboski and Shin, 2011; Midrigan and Xu, 2014). Constrained households may be forced to exit agriculture if their wealth is too low to properly utilize their agricultural skill. By allowing for an additional income stream from labor markets, a bridge allows those households to return to agricultural production. We test this result in the data and find that there are substantial flows in both directions, consistent with the model. Importantly, we show that flows toward agriculture are true *only* when the bridge lowers agricultural distortions. In the absence of such distortions, flows occur only toward wage work, as the bridge increases the expected wage for all households. Our empirical results are inconsistent with this pure sectoral selection theory.

While the previous test confirms that the bridge lowers agricultural distortions, the second test distinguishes between different two commonly assumed agricultural distortions, risk and cash-in-hand constraints. We show that our model (which assumes risk is the key agricultural distortion) predicts a decrease in savings among farmers. Intuitively, this is due to the fact that the labor market provides a substitute smoothing technology. Since most agricultural savings is held as storage, subject to high depreciation rates, farmers rationally substitute toward the less costly smoothing technology. If farmers require sufficient cash to purchase fertilizer (i.e. no credit markets exist) the opposite should hold. That is, farmers should increase savings to better overcome credit constraints. We test this result in our data and find support for risk. Agricultural storage among farmers decreases from 90 percent of harvest to

80 percent of harvest in response to a bridge, consistent with the bridge operating as a substitute smoothing technology.

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 from major infrastructure projects (Donaldson, 2013; Alder, 2017; Asturias, Garcia-Santana and Ramos, 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), Van Leemput (2016), and Sotelo (2016). Recently, a number of important papers have taken advantage of policy changes and natural experiments to identify the effects of infrastructure development, including Casaburi, Glennerster and Suri (2013) and Asher and Novosad (2016). The latter is closest to our work, as they find that new Indian roads generate movement out of agriculture. 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 and multiple channels through which the bridge affects households. Moreover, while a number of these papers equate the rural economy with the agricultural economy, we show an important relationship between on- and off-farm outcomes within rural villages.

We find that farm productivity increases because bridges allow for increases consumption smoothing through labor markets. 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), while Donovan (2016) highlights the importance of this channel for aggregate income differences. We show that self-insurance through labor market access can also generate increased fertilizer use and yields, qualitatively similar to results from formal insurance contracts highlighted in this literature. Moreover, our results have the policy implication that this self-insurance channel can be improved through better infrastructure. Relatedly, Bryan, Chowdhury and Mobarak (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. Our results show the link between access to labor markets and on-farm agricultural productivity.

2 Background and Description of Intervention

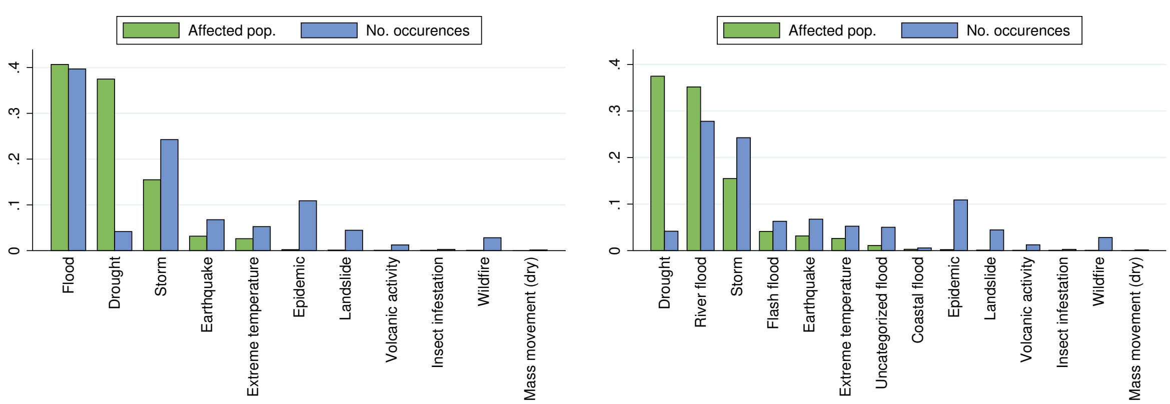
2.1 Flooding Risk

Around the world, flooding – and especially river flooding – affects a disproportionate number of people relative to other natural issues. Using the EM-DAT (2017) International Disaster Database, we compile worldwide disasters from 2000 to 2016.[4] Figure 1a shows the fraction of occurrences accounted for by various types of emergencies, along with the fraction of people affected by each type. They are ordered according to affected population. Flooding accounts for over 40 percent of the people affected by disasters since 2000, followed only by drought. Figure 1b breaks "Floods" into four categories: river floods, flash floods, coastal floods, and uncategorized floods and reproduces Figure 1a in finer detail. River floods alone are the second largest factor affecting individuals, and lag only slightly behind droughts.

Figure 1: World Disasters (2000-2016)

(a) Different Disaster Types

(b) Finer Decomposition of *Flood*



[4]A disaster is included in the dataset if it meets one of the following conditions: 10 or more dead, 100 or more people affected, declaration of state of emergency, or a call for international assistance. See EM-DAT (2017) for further details.

This issue is also salient in Northern Nicaragua, where our study takes place. Both policy makers and citizens cite flooding and the resulting isolation when combined with poor infrastructure as a critical development constraint (World Bank, 2008*a*). 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 not necessarily a good predictor of flooding, as rains at higher altitudes may be the cause of the flooding, a feature of flooding in other parts of the world as well (e.g. Guiteras, Jina and Mobarak, 2015, in Bangladesh). During these periods, some villages are cut off from access to outside markets. When the river rises substantially, market access would require swimming across the river, which may be prohibitively dangerous and inhibit transportation of goods or people, or a long journey on foot to reach the market by another route. 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.

2.2 Intervention and Identification Strategy

We 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.

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.

We compare 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.

Because of the expense of the bridges ($40,000), the number of bridges that can be funded is limited. 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 Estelí and Matagalpa in northern Nicaragua.[5]

3 Data Collection and Design Validity

3.1 Data Collected

We collect two types of data. 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 (discussed below). In this May survey, for those that agreed

[5]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.

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 covers the dry season which is not a major cropping period.[6] 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 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) affects 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

[6]Anticipating the results somewhat, none of the empirical results change if we include this first survey in our regressions.

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.

3.2 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.

3.3 High Frequency Sample Selection

Because the high frequency data was collected over the phone, two issues are worth highlighting before turning to the empirical results. First, the high frequency data is not representative of the villages under study as not every individual has a cell phone. Table 16 in Appendix C shows how high frequency respondents compare to the overall populations in the study. As one may suspect with a cell phone-based survey, respondents tend to be younger (the average household head is six years younger) and slightly more educated (one additional year of schooling). Overall, however, there are only small differences between those who participate in the high frequency survey and those who do not. Moreover, within the high frequency sample, there are no statistical differences between those in villages that receive a bridge and those that do not except for household head age.

The second issue is that the survey is an unbalanced panel as not everyone answered

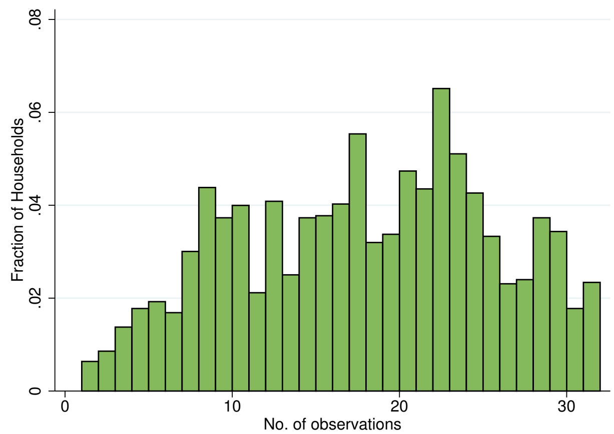
Table 1: Pre-Bridge Differences

	Constant	Bridge
Flooding Intensity		
Days flooded	1.60***	-0.07
	(0.00)	(0.83)
Flood likelihood	0.31***	0.02
	(0.00)	(0.73)
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)

Table notes: Flood intensity measures as measured from high frequency data and refer to the previous two weeks during rainy season only. p-values in parentheses. We do no clustering procedure here as to give the regression the greatest chance of finding a statistically significant difference between the two groups.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

the phone each time. Figure 2 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. We discuss the importance of this, and provide robustness checks, when discussing the results in Section 4.

Figure 2: Number of Observations per Household



4 Empirical Results

Using both the high and low frequency data discussed above, we show three main results in this section. First, labor market earnings increase in response to a bridge. Second, agricultural outcomes change, including increased fertilizer expenditures and yield on staple crops. Lastly, we show substantial switching between labor market work and agricultural production across households. Many theories, however, can rationalize the first two results. In Sections 5 and 6, we build and quantify a model designed to provide testable implications of different theories. We show that occupational switching relates the first two results to a decrease in agricultural distortions.

4.1 Labor Market Earnings

We begin by assessing the direct impact of the bridge on labor market access. We first do so in both the high frequency data (Section 4.1.1), where we can assess the relationship between flooding and contemporaneous income realizations. In Section 4.1.2, we then ask whether the larger annual surveys also show higher labor market earnings. In both cases, we find that the bridge increases access to labor markets, and thus increases earnings.

4.1.1 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. To assess the impact of flooding on different outcomes, we run regressions of the form

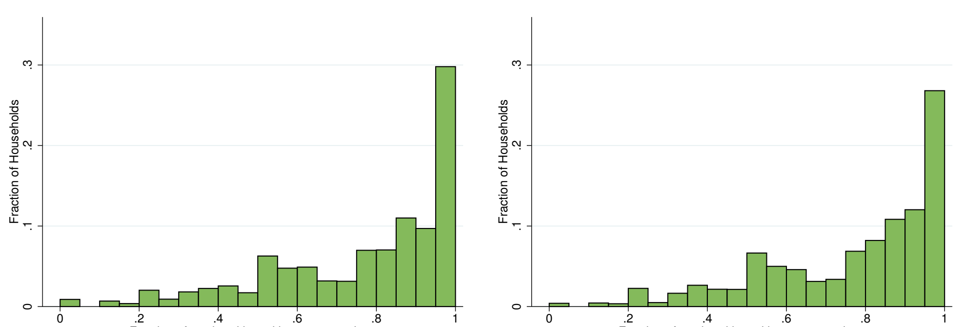
$$y_{ivt} = \alpha + \beta B_{vt} + \gamma\Big(B_{vt} \times F_{vt}\Big) + \theta\Big(NB_{vt} \times F_{vt}\Big) + \eta_t + \delta_i + \varepsilon_{ivt}. \tag{4.1}$$

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.

Figure 3 is a histogram counting the share of weeks each household receives positive labor market income. 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 3: Fraction of weeks with labor market income

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



[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 C we show that there is no relationship between flooding and the likelihood of response to surveys. Moreover, we take an extreme stance and assume every missed call implies zero income. This naturally affects the intensive margin of periods with income, but not the extensive margin.

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 (4.1): amount earned in the past two weeks and an indicator equal to one if no income was earned.

Table 2: Effects of Flooding on Income

	Household Income	No Income Earned
Flood × No Bridge	-143.659**	0.070**
	(0.022)	(0.040)
Flood × Bridge	5.047	-0.038**
	(0.970)	(0.048)
Bridge	159.495***	0.061
	(0.002)	(0.110)
Control mean	934.244	0.249
Observations	6443	6756
Individual F.E.	Y	Y
Week F.E.	Y	Y

Table notes: p-values computed using the wild cluster bootstrap-t with 1000 simulations are in parentheses, clustered at the village level. Household income has the top and bottom 5% trimmed. Control mean is average dependent variable over entire time horizon for households in villages that never receive a bridge. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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 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$144 ($p = 0.02$) during a flood in the absence of a bridge, a decrease of about 15 percent of its no-flood baseline.[8] 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 7 percentage points ($p = 0.04$) when a flood occurs in villages without a bridge. In

[8] The Nicaraguan currency is the córdoba, denoted C$. The exchange rate is approximately C$29 = 1 USD.

villages with a bridge, the fraction actually increases slightly by 3.8 percentage points ($p = 0.05$). This seems to be the critical margin that the bridge affects. Figure 4 plots the density of income realizations in villages without a bridge (left panel) and with a bridge (right panel) during periods of flooding and no flooding. Among villages without a bridge, flooding shifts the distribution closer to zero. Once a bridge is constructed, the distributions track either other closely, regardless of flooding.

Figure 4: Density of Income Realizations

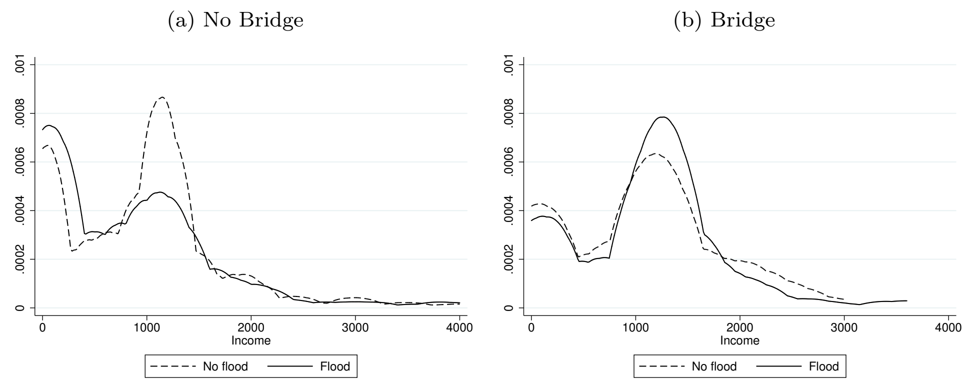


Figure notes: Figure 4a includes all village-weeks without a bridge, including those villages that eventually receive a bridge. Figure 4b includes all village-weeks post-construction.

4.1.2 Longer Run Impacts from Annual Surveys

We ask how the short-run change in income and consumption risk generated by bridges translates into longer-term effects on labor market income among rural households. For that, we utilize our larger, annual surveys. 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

$$y_{ivt} = \alpha + \beta B_{vt} + \eta_t + \delta_i + \varepsilon_{ivt} \tag{4.2}$$

where $B_{vt} = 1$ if a bridge is built, η_t and δ_i are time and household fixed effects, and standard errors are clustered at the village level using a wild cluster bootstrap-t. Panel A of Table 3 shows the results for total labor market income, along with its components of the daily wage rate and days worked. First, earnings increase by C\$308 ($p = 0.09$). 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 by a statistically insignificant C\$42 ($p = 0.76$). 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 fewer days inside the village ($p = 0.41$), though the latter cannot be statistically distinguished from zero. We find no statistically significant effects on realized wages either within or outside the village.

Panel B of Table 3 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 ($p = 0.47$) 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 ($p = 0.51$). 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 ($p = 0.00$), but also a decrease in earnings of C\$205 ($p = 0.42$) and 1.02 days ($p = 0.18$) 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\$220 ($p = 0.06$) and 1.12 days ($p = 0.01$). The point estimates suggest that these new workers also begin working inside the village, perhaps in response to existing workers move outside the village, but we find no statistically significant effects on within-village earnings or days worked.

Table 3: Effects on Market Income, by Source

Panel A: Earnings	Total Earnings			Earnings Outside Village			Earnings Inside Village		
	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
	(0.092)	(0.416)	(0.072)	(0.002)	(0.458)	(0.000)	(0.756)	(0.346)	(0.412)
No-Build Average	1025.73	275.77	3.52	295.00	168.36	1.72	661.11	263.43	1.65
Panel B: Intensive and Extensive Margins	Total Earnings			Earnings Outside Village			Earnings Inside Village		
	Earnings	Wages	Days	Earnings	Wages	Days	Earnings	Wages	Days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Build $\times$ Pos. Earnings	221.12		0.45	362.45***		1.36***	-205.15		-1.02
	(0.470)		(0.512)	(0.004)		(0.002)	(0.418)		(0.176)
Build $\times$ Zero Earnings	404.65**		1.60***	220.33*		1.12**	140.04		0.45
	(0.014)		(0.002)	(0.062)		(0.012)	(0.160)		(0.122)
No-Build Average	1025.73		3.52	295.00		1.72	661.11		1.65

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 bootstrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These results constitute the immediate effects of the bridge. Taken together, they show that bridges increase access to labor markets. In particular, the high frequency results show that bridges eliminate the uncertainty related to flash flooding, and thus allow households to access the market even during floods. Moreover, using our more comprehensive annual surveys, we confirm that bridges generate an increase in labor market income.[9] The remaining question is how these changes in market access generate treatment effects in other aspects of the economy, including occupational choice and agricultural decisions.

4.2 Impact on Agricultural Outcomes

Bridges allow households to access labor markets. However, nearly half of household economic activity in the survey is accounted for by agricultural production. We therefore ask whether the bridge has any impact on agricultural outcomes. We begin with intermediate input use on farms, using regression (4.2), with results presented in Table 4. 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\$484 ($p = 0.00$) on a baseline of C\$890, and its components fertilizer and pesticide increase by C\$274 ($p = 0.02$) and C\$151 ($p = 0.00$) respectively. The even-numbered regressions decompose the results by continuing farmers and those who did not farm at baseline. We see that the results are roughly evenly split between the two groups, as their estimates are roughly similar for total intermediate spending and its components.

We therefore next consider changes in harvest for maize and beans, measured in total quintales (100 kilograms) harvested.[10] The results are in Table 5. Harvest quantities increase by 1.06 quintales ($p = 0.20$) and 0.93 ($p = 0.01$) for maize and

[9] Of course, income risk only matters to the extent it translates into consumption risk. In Appendix **??**, we use our high frequency data to show that households are more likely to be constrained from purchasing sufficient food during a flood in villages without a bridge. Similar to the results on income realizations, the bridge eliminates this risk.

[10] In Appendix C.4, we show that the bride has no effect on crop selection by farmers, hence our focus directly on yields here.

Table 4: Farm Input Usage

	Intermediate Spending		Fertilizer Spending		Pesticide Spending	
	(1)	(2)	(3)	(4)	(5)	(6)
Build	484.33***		274.32**		150.71***	
	(0.004)		(0.016)		(0.000)	
Build × Farming		482.86**		226.21**		115.07
		(0.046)		(0.042)		(0.106)
Build × No farming		485.80***		321.58***		190.00***
		(0.000)		(0.004)		(0.000)
Control mean	889.56	889.56	607.43	607.43	303.48	303.48
Observations	1,492	1,492	1,493	1,493	1,492	1,492
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 bootstrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

beans (the two main staples) respectively. The numbers are similar when we consider yields, though the sample size decreases because yield is only defined for those actively farming.

Table 5: Harvest and Yield of Staple Crops

	Maize		Beans	
	Harvest Quantity	Yield	Harvest Quantity	Yield
	(1)	(2)	(3)	(4)
Build	1.06	7.36*	0.93***	2.71*
	(0.198)	(0.054)	(0.006)	(0.050)
Control mean	2.49	12.29	1.50	4.59
Observations	1,492	359	1,499	356
Time F.E.	Y	Y	Y	Y
Household F.E.	Y	Y	Y	Y

Table notes: Harvest quantity is measured in quintales harvested. Harvest quantity equals zero for any non-farming households. Yield is quantity harvested per manzana (1.73 acres) of land cropped, and is therefore not defined for non-farmers. p-values in parentheses are clustered using the wild cluster bootstrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These results, coupled with the impact on labor market earnings, show that bridges have important effects on the two critical income generating processes in rural Nicaragua, wage work and agricultural production.

4.3 Occupational Choice

We lastly turn to the impact of the bridge on occupational choice. While perhaps not immediately obvious, these results will help us broadly distinguish two classes of explanations. The first is selection out of agriculture. If wage work becomes uniformly more profitable, then remaining farmers must be extremely productive. On average, this would generate an increase in agricultural productivity. However, as we show in next section, this theory predicts one-way flows out of agriculture and toward wage work in response to a bridge. Alternatively, the bridge may indirectly affect agricultural distortions. This result is consistent with both flows form farming toward wage work, but also from wage work toward farming. As we show here, the empirical results are consistent with the second.

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.[11]

We begin by assessing the impact of the bridge on the persistence of sectoral employment. Figure 5 plots the simple averages of households engaged in agricultural production and labor market work in treatment and non-treatment villages. Note that these are not mutually exclusive categories, as households can be engaged in both agricultural production and earn wages in the labor market. The results show a remarkably stable aggregate fraction of households in both types of work across both treatment and control villages. Moreover, there is no obvious treatment effect from bridges.

To assess this more formally, we run a series of regressions. We define $O^j_{ivt} = 1$ if a household i in village v is engaged in activity $j = 1, 2$ (agricultural production and wage-earning activities, respectively) at year t. We interact the treatment with baseline activities.

[11] We refer to wage work and farming as "occupations" throughout, for simplicty.

Figure 5: Fraction of Households Engaged in Agricultural Production and Labor Market Earnings

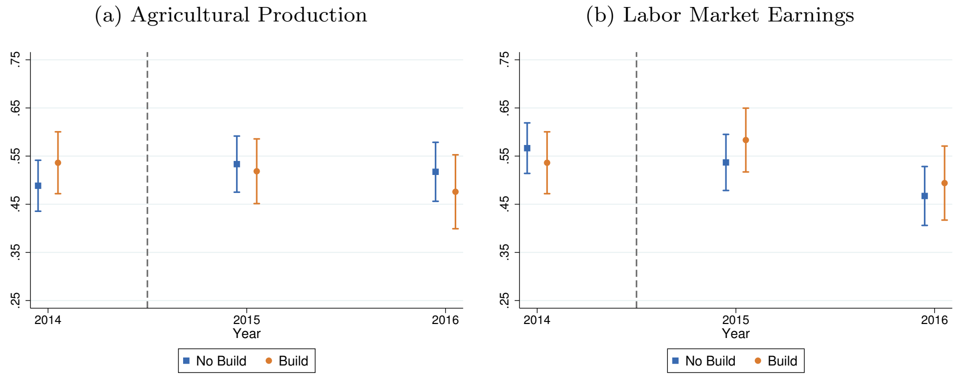


Figure notes: The dashed line indicates when bridges were constructed. The 95 percent confidence intervals are denoted by the bars surrounding the point estimates.

$$
\begin{aligned}
O^j_{ivt} &= \alpha + \beta B_{vt} + \eta_t + \delta_i + \varepsilon_{ivt} \\
O^j_{ivt} &= \alpha + \beta(B_{vt} \times O^j_{iv0}) + \gamma(B_{vt} \times (1 - O^j_{iv0})) + \eta_t + \delta_i + \varepsilon_{ivt}
\end{aligned}
$$

The results of both regressions are in Table 6. Regressions 1 and 3 show slight movement toward labor market earnings and away from agriculture, though neither effect can be statistically distinguished from zero. While the net effect is indeed small, regressions 2 and 4 show that it masks substantial heterogeneity. When we interact the bridge treatment with baseline activity, we find substantial movement across both types of work. Baseline farmers are 31 percentage points less likely to farm ($p = 0.00$), while baseline wage earners are 13 percentage points less likely to earn wages ($p = 0.01$) once a bridge is constructed. Thus, the aggregated results in Figure 5 are a result of roughly offsetting movements into and out of each occupation. This result is consistent with the idea that a bridge indirectly eliminates some agricultural distortion, as previously constrained individuals move into agriculture.

To assess this reallocation in more detail, we decompose the occupational space into the four mutually exclusive groups – only agricultural production, only labor

Table 6: Effects on Persistence of Activities

	Agriculture (1)	Agriculture (2)	Labor Market (3)	Labor Market (4)
Build	-0.076		0.062	
	(0.248)		(0.280)	
Build × Engaged		-0.311***		-0.134**
		(0.002)		(0.010)
Build × Not engaged		0.202**		0.285***
		(0.010)		(0.002)
Control: Fraction of HH engaged	0.488		0.538	
Control: Engaged – Engaged		0.799		0.853
Control: Not engaged – Engaged		0.192		0.193
Observations	1,507	1,507	1,507	1,507
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* = 1 if the household is not engaged in the relevant activity at baseline. p-values in parentheses are clustered using the wild cluster bootstrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

market earnings, both, and neither – and run the 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 7, 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 (-0.55, $p = 0.00$) 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 (-0.51 with $p = 0.00$). Lastly, as in the previous set of results in Table 6, 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 7. For any current economic activity, a bridge makes it significantly less likely that a household is engaged in that same activity post-treatment.

Table 7: 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
	(0.002)	(0.002)	(0.146)	(0.104)
Build × Wages only	0.044	-0.218***	0.126*	0.049
	(0.244)	(0.006)	(0.074)	(0.264)
Build × Both	0.178***	0.275***	-0.505***	0.052
	(0.000)	(0.006)	(0.002)	(0.520)
Build × None	0.268	0.204***	0.076	-0.547***
	(0.126)	(0.010)	(0.194)	(0.002)
Control: From Agr	0.775	0.171	0.032	0.023
Control: From Wage	0.069	0.751	0.122	0.059
Control: From Both	0.168	0.189	0.613	0.029
Control: From None	0.185	0.147	0.014	0.653
Observations	1,507	1,507	1,507	1,507
Time F.E.	Y	Y	Y	Y
Household F.E.	Y	Y	Y	Y

Table notes: The interaction terms are the activity of the household at baseline. p-values in parentheses are clustered using the wild cluster bootstrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The rest of this paper builds and quantifies a model to show that these occupational switching results are informative about the underlying channels that link labor market earnings to agricultural outcomes.

5 Model and Rationale for Findings

We have shown three main results: labor market earnings increase, farm expenditures and yields increase, and there is substantial reallocation of activity between wage work and farming. In this section, we develop a model that links these results together. Critically, we show that occupational switching – in particular movement *toward* agriculture – is consistent with the idea that the bridge decreases distortions in the agricultural sector through its effect on labor markets. This has the additional implication of increasing fertilizer investment and yields, consistent with our results. We formalize that idea here. Note however that this result does not depend on the exact form of the agricultural distortion (e.g. risk versus credit constraints), only that it decreases with the additional income provided by labor market access. We therefore also show that the model provides further testable implications that allow us to differentiate these two channels.

5.1 Model Set-Up

The model is in discrete time and in partial equilibrium. Households are infinitely lived and consume a single good to maximize utility

$$\mathbb{E}_0\Big[\sum_{t=0}^{\infty}\beta^t\frac{(c_t-\overline{c})^{1-\sigma}-1}{1-\sigma}\Big],$$

where $\beta \in (0,1)$ is the discount factor and $\overline{c}$ is a subsistence requirement. The good is storable between periods as savings b, though it does not accrue any interest. It cannot be borrowed against, so savings are subject to the constraint $b \geq 0$. Households are endowed with one unit of time.

5.1.1 Occupational Choice

A household can choose to be a worker w or a farmer a (for agriculture). Households are endowed with stochastic ability in each occupation The ability of a household in each occupation, given by the vector $\mathbf{z_t} = (z_{at}, z_{wt})$ with transition function $Q(\mathbf{z}_{t+1}, \mathbf{z}_t)$. A household with savings b_t therefore chooses their period t occupation to maximize expected utility. This choice is represented recursively by the function

$$v(\mathbf{z}, b) = \max\ \Big\{v^a(\mathbf{z}, b), v^w(\mathbf{z}, b)\Big\} \tag{5.1}$$

The value functions v^a and v^w are the values of choosing to be a farmer or worker conditional on ability $\mathbf{z}$ and savings b. These are discussed in detail below.

5.1.2 Working

A household that chooses to become a worker uses its entire time endowment in market work. Their earnings are therefore equal to wz_w, where w is a wage per efficiency unit of time. In recursive format, the implies that the value of being a worker with shock $\mathbf{z}$ and savings b is

$$\begin{aligned} v^w(\mathbf{z}, b) &= \max_{c,b'} \; u(c) + \beta v(\mathbf{z}, b') \\ s.t. \quad & c + b' - b = z_w w \\ & b' \geq 0 \end{aligned}$$

Note the continuation value v implies that the household can re-optimize its occupational choice each period given shock realizations and savings decisions.

5.1.3 Farming

The farm technology is given by the production function

$$f(z_a, s, x, n) = s z_a x^\theta n^\eta$$

Farm output is driven by farming ability z_a, a random farm shock s, intermediate inputs (fertilizer and pesticide) x, and labor n. The shock s is i.i.d. across households and time, with associated cumulative distribution function $G(s)$.[12]

Before this shock is realized, farmers must choose the quantity of intermediate inputs on their farm, consistent with the importance of risk for this decision.[13] Once they choose intermediates, the shock is realized. After the shock, farmers can choose how to delineate their time between working on the farm and working in the market for wages, which are taxed at gross rate $\tau \in (0, 1)$, along with their consumption and savings decisions.[14]

[12] The i.i.d. assumption is not relevant for the qualitative results, but simplifies the exposition significantly.

[13] See the growing literature on rainfall insurance, such as Mobarak and Rosenzweig (2012) and Karlan et al. (2014), among many others.

[14] The reason for the tax is simply to guarantee that some households choose to be farmers. If farming households can costlessly the labor market, the Inada conditions on the farm technology guarantees that all households will farm. Note also that we assume no hired labor, as 93 percent of our sample hires no labor.

This timing implies a two-step problem for farmers. After the shock z_r is realized, the value of being a farmer with ability $\mathbf{z}$, savings b, and an intermediate choice x is given by

$$\begin{aligned} \widetilde{v}^a(\mathbf{z}, s, b, x) &= \max_{c,n,b'} u(c) + \beta v(\mathbf{z}, b') \\ s.t. \quad & c + b' - b = s z_a x^\theta n^\eta - x + \tau z_w w(1-n) \\ & n \in [0, 1] \\ & b' \geq 0. \end{aligned}$$

This defines decision rules as a function of x, $\widetilde{b}'(\mathbf{z}, s, b, x)$ and $\widetilde{n}(\mathbf{z}, s, b, x)$. Working backwards, the value of entering the period as a farmer is given by

$$v(\mathbf{z}, b) = \max_{x \geq 0} \int_s \widetilde{v}^a(\mathbf{z}, s, b, x) dG(s)$$

which defines the decision rule $x(\mathbf{z}, b)$, and thus implicitly the decision rules $b'(\mathbf{z}, s, b) := \widetilde{b}'(\mathbf{z}, s, b, x(\mathbf{z}, x))$ and $n(\mathbf{z}, s, b) := \widetilde{n}(\mathbf{z}, s, b, x(\mathbf{z}, x))$.

5.1.4 Discussion and Link to Bridges

Before characterizing the model, we briefly discuss the link between the model and empirical results in previous sections. The timing of the model above implies that the counterpart to wage income $z_w w$ is total seasonal income. We assume that the direct effect of a bridge is easier access to labor markets, which is consistent with our empirical results in Section 4.1. We therefore model a bridge as an exogenous increases in the parameter w, which increases labor market earnings for those who wish to access the labor market. Our goal in the following sections is to show that, in addition to changing labor market earnings, this change in w changes agricultural outcomes and occupational choice, consistent with our results in Section 4.2 and 4.3.

5.2 Analytical Characterization

To show the link between these channels in the clearest way possible, we begin by considering a simplified version of the model. First, we assume that the model is static. Second, we assume the constraint $n \leq 1$ never binds (or that households can hire farm labor). These two assumptions allow us to analytically prove a number of results related to occupational switches. Our goal is to show the following: the inability to insure agricultural shocks (1) lowers intermediate input expenditures relative to the profit-maximizing amount and (2) generates flows *toward* agriculture once a bridge is constructed. Moreover, if these shocks are insured, no such flows occur.[15]

5.2.1 The Distortionary Effect of Risk

We begin with the first result: that the lack of insurance lower intermediate input use relative to the efficient level. This result can be seen by comparing the first order conditions between the model and a counterfactual model in which farm shocks s are fully insured. The first order condition for a farming household is

$$z_a F'(x) \int_s s^{1/(1-\eta)} \left(\frac{u'(c(\mathbf{z}, s, x))}{\mathbb{E}_s[u'(c(\mathbf{z}, s, x)]} \right) dG(s) = 1 \tag{5.2}$$

where $F(x)$ is the production function after solving for n. If shocks s were insured, the first order condition would be

$$z_a F'(x) \int_s s^{1/(1-\eta)} dG(s) = 1 \tag{5.3}$$

These two first order conditions are the same except for the introduction of risk neutral probabilities in (5.2). Intuitively, the inability to insure *ex post* consumption forces farming households to consider how their *ex ante* investment affects *ex post* utility realizations. An application of Jensen's inequality immediately implies that the optimal choice of intermediates is lower in the world in which shocks are uninsured, conditional on ability vector $\mathbf{z}$. Put differently, farmers are concerned that high investment will lower consumption if they are hit with a bad shock. One available channel to insure against this outcome is to earn wages in the labor market. A bridge, then, increases the efficacy of this insurance channel by increasing the realized wage on average.

[15] See Donovan (2016) for the formalization of agricultural risk as an intermediate input market distortion. He shows that uninsured risk is isomorphic to a tax on intermediate input purchases in a model where farmers maximize profit.

5.2.2 How a Bridge Affects Occupational Changes

We next turn to how this insurance channel impacts occupational choice, and its link to bridge construction. To begin, for any value of z_w define $z_a^*(z_w)$ as the solution to

$$v^a(z_a^*(z_w), z_w) = v^w(z_w).$$

This is the value of z_a that makes a household indifferent between the two occupations, with the understanding that it also depends on the parameter w. This cutoff is guaranteed to exist because v^a is continuous in both arguments, $v^a(0, z_w) < v^w(z_w)$, and there exists a z_a such that $v^a(z_a, z_w) > v(z_w)$ since v^a is unbounded in the first argument. Proposition 1 links occupational switching to labor market access.

Proposition 1. *Consider prudence given by $-u'''(\cdot)/u''(\cdot)$. In the model without insurance,*

1. *If prudence is sufficiently large, then $z_a^*(z_w)$ is decreasing in w.*
2. *If prudence is sufficiently small, then $z_a^*(z_w)$ is increasing in w.*

With insurance, $z_a^*(z_w)$ is increasing in w for all prudence levels.

Proof. See Appendix A ■

Proposition 1 relates the prudence of households to the occupational switching results. The notion of prudence is closely related to the notion of absolute advantage in the static model used here, given that it is decreasing in c. Low ability households are on average poorer, and thus have lower c and higher prudence. Put differently, poorer households have stronger precautionary motives than rich households.

In terms of empirical predictions, Proposition 1 states that with full insurance, increasing the mean of z_w serves only to push people toward wage work. This is inconsistent with our results on occupational switching in Section 4.3. However, the elimination of insurance breaks this result. At sufficiently low levels of ability, increasing the mean worker ability draw generates shifts into agriculture. That is, the ability to insure *ex post* consumption through labor allows for some households to take advantage of their agricultural skill. A bridge, therefore, causes them to return to farming. As we show in the next section, this results also holds in the full dynamic model, and also provides further tests that we can take to the data. We therefore now turn back to quantifying the full dynamic model.

6 Quantifying the Model and Other Testable Predictions

We now turn back to the full quantitative model to study the impact of bridges on distortions in the agricultural sector. We quantify the model, as the borrowing constraints and discrete occupational choice make a full analytical characterization difficult. However, the main results and intuition are derived from the static model characterized above. We first calibrate the model so that the stationary equilibrium matches a number of moments in the control group. We then vary w to match the increase in labor market earnings in the treatment, then compare a number of predicted model moments to the empirical results.

We assume that $\log(\mathbf{z})$ follows a VAR(1) process of the form

$$\log(\mathbf{z}_{t+1}) = \begin{bmatrix} F_0^w \\ F_0^a \end{bmatrix} + \begin{bmatrix} \rho_w & 0 \\ 0 & \rho_a \end{bmatrix} \log(\mathbf{z}_t) + e_t$$

where

$$\mathbb{E}[e_t e_t'] = \begin{bmatrix} \sigma_w^2 & \sigma_{aw} \\ \sigma_{aw} & \sigma_a^2 \end{bmatrix}$$

6.1 Calibration Procedure

There are fourteen parameters in the model. The utility function requires a subsistence requirement $\bar{c}$, elasticity of intertemporal substitution σ, and discount factor β. The production technology requires exponents θ and η, along with tax τ. The ability process requires constants F_0^j, persistence ρ_j, standard deviation σ_j for $j \in \{a, w\}$ and covariance σ_{aw}. The idiosyncratic farm shocks require a choice for the variance of these shocks. Some of these parameters we set exogenously to standard values. On the utility side, we set $\bar{c} = 0$, $\sigma = 2$, and $\beta = 0.95$. We set the exponents of the production function to $\theta = 0.40$ and $\eta = 0.40$. Lastly, we normalize $F_0^w = 0$.

That leaves eight parameters which we jointly choose to match moments from the control group. Table 8 summarizes the model moments, the parameter choices, and fit. [*This calibration hits our targets well, but a more complete calibration exercise is forthcoming.*]

Table 8: Target Moments from Control Group

Description of data moment	Value	Model-Generated Moment	Model Parameter	Value
Share of HH in wage work only	0.41	0.41	σ_r	0.98
Share of HH in wage work + farming	0.19	0.19	σ_a	0.50
Share of HH in farming only	0.40	0.40	σ_w	0.50
Persistence of HH wage work	0.76	0.77	ρ_a	0.65
Persistence of HH farming	0.75	0.74	ρ_w	0.90
c.v. yield on staple crops	1.18	1.24	F_0^a	0.10
c.v. total labor market earnings	1.04	1.05	τ	0.60
Ratio of earnings, non-farming/farming HHs	1.97	2.03	σ_{aw}	0.00

Table notes: The model parameters are chosen so that the stationary equilibrium matches the listed data moments. The model parameters do therefore not match one-to-one to the targeted moment on the same row, and should not be construed at such.

6.2 Creation of Model Dataset

With the calibrated model in hand, we compute a dataset of 50,000 individuals for three periods, consistent with our empirical time series. We then are left to compute a treatment group. To do so, we hold the calibration fixed and increase w so that the regression of earnings on the bridge indicator implies a 22 percent increase in total earnings. This is consistent with our empirical evidence in Section 4. We then start the treatment group from the stationary distribution of the control, and trace the transition path in response to a surprise increase in w for three periods. We rely on this model-generated dataset to test our empirical predictions against the empirical results.

6.3 Testing Model Implications Against the Data

Table 9 shows the model predictions for occupational choice in both the complete insurance and baseline world. These are the model-generated counterparts to the empirical results in Table 6. With insurance, households stop farming and access the labor market. These are in regression 3 and 4. As in the data however, without insurance, we see flows both toward farming and toward wage work. The intuition for this result is in the previous section – the availability of self-insurance through the labor market allows some households to enter agricultural production and its associated riskiness. That is, increased labor market access through the construction of a bridge lowers the distortion faced by agricultural households.

Table 9: Effects on Persistence of Activities in Model

	Self-Insurance Only		Full Insurance	
	Agriculture	Labor Market	Agriculture	Labor Market
	(1)	(2)	(3)	(4)
Build × Engaged	-0.333***	-0.211***	0.021	0.435***
	(0.000)	(0.002)	(0.153)	(0.010)
Build × Not engaged	0.178 **	0.158**	-0.461***	0.028
	(0.021)	(0.014)	(0.000)	(0.211)

Table notes: Engaged = 1 if the household is engaged in the relevant activity at baseline, and *Not engaged* = 1 if the household is not engaged in the relevant activity at baseline. p-values in parentheses are clustered using the bootstrap on model time series with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

[*More results forthcoming.*]

7 Alternative Explanations

In this section, we discuss a number of potential alternative explanations and provide robustness for the results.

7.1 Credit Constraints vs. Risk

An alternative hypothesis to the one provided in the previous section is credit constraints. That is, households gain access to the labor market, which provides them more resources to purchase fertilizer. Indeed, this theory is also consistent with our occupational switching results. We test whether this idea here. First, a model of credit constraints implies that savings should increase, while the model in the previous sections implies that savings should decrease. We test that prediction here, by considering the amount of harvest stored by the household. This is the main form of savings in these rural villages, yet is a high cost savings technology. In the baseline pre-bridge data, 38 percent of households store their crops in plastic bags, 36 percent use plastic barrels, and 23 percent use small personal silos.[16] These technologies are prone to substantial spoilage and infestations (Grolleaud, 2002; Hodges et al., 2010), thus making storage a high cost method to move goods across time. The model predicts that access to the labor market should substitute for this savings technology, as households can more easily insure consumption through labor market earnings.

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. Storage is defined as harvest net of sales, debt payments, gifts, and land payments.[17] Any household with no crop production is given a value of zero in this regression. Table 10 shows how bridges affects savings behavior.

Table 10: Farm Savings Choices

	Maize		Beans	
	(1)	(2)	(3)	(4)
Build	-0.085**		-0.088**	
	(0.028)		(0.046)	
Build × Farming		-0.087		-0.113
		(0.148)		(0.152)
Build × No farming		-0.082**		-0.058*
		(0.018)		(0.068)
Control mean	0.942	0.942	0.928	0.928
Observations	1,507	1,507	1,507	1,507
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 bootstrap-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. Farmers in build villages save 9 percentage points less of corn harvest ($p = 0.03$) and bean harvest ($p = 0.05$). This affect is found in both continuing and new farmers, though the higher variance among continuing farmers implies that only new

[16] The remaining three percentage points are split between doing nothing and more complicated storage technologies, such as crop cellars.

[17] 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, "amount currently stored" 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.

farmers have a significant effect from the bridge. Among new farmers, the bridge induces a 8 percentage point decrease in maize storage ($p = 0.02$) and a 6 percentage point decrease in bean storage ($p = 0.07$). Among continuing farmers, we find similar decreases of -0.09 ($p = 0.15$) and -0.11 ($p = 0.15$), and though we miss statistical significance at the 10 percent level for both.

As a second test, we also note that a necessary condition for a credit constraints theory is that households with labor market income are the same ones that purchase fertilizer. Unlike the theory based on risk, where the *availability* of the labor market is critical, the credit constraints theory relies on *realizations* of labor market income. We break the data into four groups depending on whether or not anyone claims to be working for wages either before or after the bridges are constructed. These four groups are {NN, NY, YN, YY} where the first N/Y stands for whether or not the household has a wage earner at baseline, while the second N/Y stands for whether the household has a wage earner in either of the post-construction periods. Table 11 shows the results.

Table 11: Fertilizer Spending and Wage Earnings

	Intermediates	Intermediates	Intermediates	Intermediates
	(1)	(2)	(3)	(4)
Build	619.28*	337.77	504.61**	543.63***
	(0.096)	(0.468)	(0.036)	(0.000)
Control mean	898.77	1117.51	487.19	381.81
Observations	431	189	144	664
No. households	179	73	60	262
Earnings Pre-Build?	N	N	Y	Y
Earnings Post-Build?	N	Y	N	Y

Table notes: Earnings pre-period is defined by an indicator = 1 if the household claims to have any wage earners at wave 2, while *Earnings Post-Period* defined as any household a self-reported wage earner at waves two or three.

Of course, these effects are not meant to be interpreted causally, as occupational choice is endogenous. However, the correlations are cast doubt on the importance of credit constraints generating the results. Regressions (1) and (3) are those in which households have no wage earners after bridge construction. In both cases, we still find a substantial increase in intermediate expenditures associated with a bridge. The only subgroup in which we find no statistically significant effect are among those who access the labor market for the first time after the bridges are constructed. Moreover, the change of intermediate expenditures and change in earnings among treated households is 0.04. Again, this suggests that available cash holdings is not critical for generating more intermediate spending. All of these results suggest that it is the availability of the labor market, not necessarily the *use* of the labor market that is critical to generating the results.

7.2 Prices Changes

An alternative explanation for these results is that prices change. Input prices may become cheaper as the cost of trade declines, or alternatively output prices may increase if bargaining power with intermediaries increases. Both of these are unlikely given the villages under consideration. Floods last for days or weeks, not entire seasons. To the extent that input or output purchases and sales can be delayed for a week or two during a flood episode, it is unlikely that a bridge would affect prices.[18] Table 12 tests whether sale prices change for staple crops, and also delineates by how far households are from the bridge, as this may affect market power or access. We find no evidence of differential price changes across build and no-build villages.

7.3 Land Consolidation

A final theory that may generate changes in occupational switching is farm consolidation. This theory relies on complimentarity between farm skill and farm size. If high skilled farmers are relatively unproductive (in a comparative advantage sense) managing small farms but more productive on large farms, then to the extent the bridge facilitates farm consolidation, households may begin to farm in response to the bridge. This would be consistent with our results in Section 4.3. Table 13 tests whether total cropland or rentals change in response to the bridge, and we find no evidence of such changes.

[18] This is, of course, not a generic statement about infrastructure and trade costs, but a function of the scenario we study here. However, even in the absence of classic infrastructure benefits of price convergence, we still find substantial changes driven by these bridges.

Table 12: Output Prices

	Corn Price	Corn Price	Bean Price	Bean Price
	(1)	(2)	(3)	(4)
Build	-36.44		-45.96	
	(0.32)		(0.92)	
Build × Near		-51.32		67.55
		(0.24)		(0.78)
Build × Far		-32.79		-149.76
		(0.42)		(0.36)
Far		-40.80		108.36
		(0.34)		(1.00)
Control mean	176.07	176.07	916.87	916.87

Table notes: Near = 1 if the household is above village-median distance to the (potential) bridge site. p-values in parentheses are clustered using the wild cluster bootstrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 13: Land Use and Farm Size

	Total Land Owned	Total Land Cropped	Rent out any land?
	(1)	(2)	(3)
Build	0.062	-0.012	-0.015
	(0.712)	(0.886)	(0.494)
Control mean	2.636	1.074	0.067
Observations	1,601	1,601	1,601
Time F.E.	Y	Y	Y
Household F.E.	Y	Y	Y

Table notes: Regressions one and two are measured in manzanas (1.73 acres), while regression three is an indicator for whether or not you rent land to someone else, including formal and informal arrangements. p-values in parentheses are clustered using the wild cluster bootstrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

8 Conclusion

We study 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. We identify a number of important changes among households. First, the bridge eliminates the decrease in contemporaneous income realizations during floods. Second, agricultural investment in fertilizer and yields on staple crops both increase. We then build a model that links these results together, in which bridges facilitate consumption smoothing through more consistent labor market access. This model provides a number of testable implications that are consistent with the data. We find substantial reallocation of households between age and farm work, in both directions. Second, we find that savings decreases among farmers, consistent with bridges providing a substitute smoothing technology.

Finding evidence of these multiple channels is important for policy, given the variety of income-generating activities in rural areas (World Bank, 2008*b*). We find no evidence of price convergence between urban and rural areas, implying that benefits from infrastructure development extend beyond the ability to more efficiently move goods across space. Other work focused on larger projects (e.g. Asher and Novosad, 2016), however, find important implications for structural transformation and off-farm migration, including these price effects. An important avenue for future work is to utilize these detailed results to better understand the underlying impact between infrastructure, trade, and structural transformation.

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A Proofs

A.1 Two Additional Lemmas

We require two additional lemmas to prove the result.

Lemma 1. *Suppose $Var(z_r) > 0$. If*

$$-\frac{u'''(wz_w)}{u''(wz_w)} > -\frac{u''(wz_w)}{u'(wz_w)}$$

and

$$u(wz_w) = \int u(c(s))dF(s),$$

then

$$\frac{u'(wz_w)}{\int u'(c(s))dF(s)} < 1,$$

and the magnitude of the inequality is strictly increasing in $u'''u'/u''^2$.

Proof. This lemma follows directly from Pratt's Theorem. Let:

$$E(u(c)) = u(E(c) - \rho_1) = u(az_w) \tag{A.1}$$

$$E(-u'(c)) = -u'(E(c) - \rho_2) \tag{A.2}$$

Pratt's Theorem is applicable as both u and $-u'$ are strictly increasing and concave. If $u'''u'/u''^2 > 1$, then Pratt's Theorem implies that $\rho_2 > \rho_1$. Therefore, since u' is decreasing:

$$E(u'(c)) = u'(E(c) - \rho_2) > u'(E(c) - \rho_1) = u'(az_w) \tag{A.3}$$

or

$$\frac{u'(E(c) - \rho_1)}{E(u'(c))} = \frac{u'(az_w)}{E(u'(c))} < 1 \tag{A.4}$$

Moreover, the difference between ρ_1 and ρ_2 is increasing in $u'''u'/u''^2$. ■

Lemma 2. *If*

$$-\frac{u'''(az_w)}{u''(az_w)} \leq -\frac{u''(az_w)}{u'(az_w)}$$

then

$$\frac{u'(az_w)}{\int u'(c(s))dF(s)} \geq 1$$

Proof. The proof of this is the same as above, but now $\rho_2 \leq \rho_1$, which reverses the result. ■

A.2 Proof of Proposition 1

Proof. Solving for the optimal value of x among farming households implies:

$$x^{\frac{1-\eta-\theta}{1-\eta}} = \theta \int \left(s z_a \left[\frac{\eta}{\tau w z_w} \right]^{\eta} \right)^{\frac{1}{1-\eta}} \frac{u'(c(\mathbf{z}, s, x))}{\int u'(c(\mathbf{z}, t, x))dG(t)} dG(s), \tag{A.5}$$

for

$$c(\mathbf{z}, s, x) = (1-\eta) \left(s z_a \left[\frac{\eta}{\tau a z_w} \right]^{\eta} \right)^{\frac{1}{1-\eta}} x^{\frac{\theta}{1-\eta}} + \tau w z_w - x \tag{A.6}$$

The cutoff $z_a^*(z_w)$ is defined by:

$$v^a(z_a^*(z_w), z_w; w) = v^w(z_w; w). \tag{A.7}$$

By the implicit function theorem:

$$\frac{dz_a^*(z_w)}{dw} = \frac{\frac{\partial v^w}{\partial w} - \frac{\partial v^a}{\partial w}}{\frac{\partial v^a}{\partial z_a^*(z_w)}} \tag{A.8}$$

Applying the Envelope Theorem to evaluate these derivatives implies:

$$\frac{d \log(z_a^*(z_w))}{d \log(w)} = \eta + w z_w \theta \frac{\frac{u'(w z_w)}{\int u'(c(\mathbf{z},s,x))dG(s)} - \tau}{x^{1-\frac{\theta}{1-\eta}}} \tag{A.9}$$

There are two cases from here. In the case of Lemma 1, if prudence is great enough, then the ratio in the numerator of the second term of the right hand side of equation (A.9) is less than τ, which implies that the second term is negative. Furthermore, for any (z_a, z_w), clearly x is decreasing in prudence, so that greater prudence implies that the denominator in the second term of equation (A.9) is decreasing in prudence, so that the second term is negative, strictly decreasing and unbounded. Therefore, for sufficiently high prudence, the right hand side of equation (A.9) is negative. This is the first result.

In the case of Lemma 2, prudence is low and the second term on the right hand side of equation (A.9) is positive. The first term is also positive, which implies the second result. ■

B Per-Period Effects and Aggregate Shocks

To what extent to the results hold year-by-year? We re-run the regressions as

$$\begin{aligned} y_{ivt} &= \alpha + \beta B_{vt} + \gamma B_{vt} + \eta_t + \delta_i + \varepsilon_{ivt} \qquad \text{for t = 2,3} \\ y_{ivt} &= \alpha + \beta B_{vt} + \gamma B_{vt} + \eta_t + \delta_i + \varepsilon_{ivt} \qquad \text{for t = 2,4.} \end{aligned}$$

Table 14 shows the main results for each period. All of the main results hold period-by-period. Total earnings from $t = 2$ to $t = 4$ is not statistically significant ($p = 0.232$), but the point estimate is still in line with the estimates at $t = 3$.

Table 14: Effects on Market Income, by Period

Panel A: t=3	Total Earnings			Farm Expenditures			Farm Outcomes			
	Earnings	Wages	Days	Intermediates	Fertilizer	Pesticide	Maize Harvest	Maize Yield	Bean Harvest	Bean Yield
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Build	339.86**	-23.02	1.13*	471.05***	253.39**	126.11***	1.57*	5.57	0.67**	2.65*
	(0.028)	(0.225)	(0.078)	(0.009)	(0.013)	(0.005)	(0.068)	(0.194)	(0.046)	(0.059)
No-Build Baseline Average	1025.73	275.77	3.52	612.50	405.60	176.45	1.58	9.03	0.98	3.94

Panel B: t=4	Total Earnings			Farm Expenditures			Farm Outcomes			
	Earnings	Wages	Days	Intermediate	Fertilizer	Pesticides	Maize Harvest	Maize Yield	Bean Harvest	Bean Yield
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Build	240.77	-28.44	0.93*	415.78*	240.71**	176.62**	0.37	8.91***	1.12***	3.04*
	(0.232)	(0.462)	(0.055)	(0.070)	(0.024)	(0.041)	(0.687)	(0.008)	(0.008)	(0.077)
No-Build Baseline Average	1025.73	275.77	3.52	612.5	405.60	176.45	1.58	9.03	0.98	3.94

Table notes: This table reproduces the main results from the paper, but reports them period-by-period instead of pooled. p-values in parentheses are clustered using the wild cluster bootstrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

C More Results and Robustness

C.1 Rainfall At Bridge Site and Floods

We use daily rainfall data from the Climate Hazards Group InfraRed Precipitation with Station data (CHIRPS) which covers the period 1981-2016.[19] CHIRPS provides rainfall estimates at the 5 degree resolution. We combine GPS coordinates for potential bridge locations in our study, our high frequency data, and CHIRPS data to assess the correlation between bridge site rainfall and flood realizations. To do so, we correlate rainfall realizations with flood realizations at each of the 15 cells that either received a bridge or were the best potential spot to build a bridge in control villages. We use two rainfall measures. The first is millimeters of rain in the two weeks covered by each two-week period in our sample. The second uses deviations from historical averages. We compute the time series of rainfall from 1981-2013 at two week period, and compute the z-score for each period in our data. The results are in Table 15. The results are positive but not overwhelmingly so, consistent with substantial uncertainty in flooding even conditional on local weather.

C.2 High Frequency Data Balance Checks

Table 16 shows the results from the regression

$$y_{iv} = \alpha + \beta Bridge_v + \gamma HF_{iv} + \eta(Bridge_{iv} \times HF_{iv}) + \varepsilon_{iv}.$$

Here, y_{iv} is some outcome at baseline for household i in village v, $Bridge_v = 1$ if village v will receive a bridge, while $HF_{iv} = 1$ if household i participates in the high frequency survey.

C.3 How high frequency survey response rates change during floods

Figure 3 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

[19] See Funk et al. (2015) for more information on the data and its construction.

Table 15: Correlation of Days Flooded and Rainfall Measures at Site

	mm Rain	z-score
Aguas Calientes	-0.1491	-0.3081*
El Pueblito	-0.0028	-0.1639
El Tamarindo	0.4606***	0.2154
El Terrero	0.3830**	0.1765
La Caldera	0.4872**	0.2278
La Calera	0.3028*	0.0943
La Cana	0.3424*	0.0997
Las Gabetas	0.1380	0.0892
Mata Palo	0.3955**	0.1546
Paneagua	0.5283***	0.3399*
Plan de Grama	0.0255	-0.0859
Rio Abajo	0.3523**	0.0809
Rio Grande	0.2391	-0.0432
San Jose de Sacal	0.2925	0.2177
San Juan de Limay	0.2361	0.0598
Mean	0.2688	0.0770
Median	0.3028	0.0943

Table Notes: An entry in the table is the correlation between the rainfall measure (either millimeters of rain or the z-score of rainfall deviations) at the bridge site or best potential bridge site in each village with the number of days flooded in the past two weeks. Statistically significant correlations are starred, where *, **, *** indicate significance at the ten, five, and one percent level.

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 17. 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 6 reproduces Figure 3 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

Table 16: Pre-Bridge Differences High Frequency Data

	Constant	Bridge	High-Frequency	Interaction
Household Composition				
HH head age	47.17***	4.82**	-5.14***	-4.65**
	(0.00)	(0.02)	(0.00)	(0.05)
HH head yrs. of education	2.63***	0.70	1.03***	-0.50
	(0.00)	(0.19)	(0.00)	(0.42)
No. of children	1.06***	0.00	0.32***	-0.04
	(0.00)	(1.00)	(0.00)	(0.82)
HH size	3.81***	0.13	0.49***	0.03
	(0.00)	(0.47)	(0.00)	(0.92)
Occupational Choice				
Agricultural production	0.47***	0.02	0.04	-0.01
	(0.00)	(0.72)	(0.14)	(0.82)
Off-farm work	0.59***	0.00	0.02	0.01
	(0.00)	(0.93)	(0.56)	(0.79)
Total wage earnings (C$)	1204.68***	57.63	354.33*	-77.55
	(0.00)	(0.84)	(0.06)	(0.82)
Farming				
Corn harvest	2.21***	-1.01*	-0.66*	1.03
	(0.00)	(0.10)	(0.09)	(0.15)
Bean harvest	0.72***	-0.08	-0.11	-0.26
	(0.00)	(0.76)	(0.52)	(0.40)
Plant corn?	0.16***	0.03	0.02	-0.03
	(0.00)	(0.50)	(0.59)	(0.62)
Plant beans?	0.17***	0.01	-0.01	-0.05
	(0.00)	(0.91)	(0.72)	(0.40)

Table notes: Flood intensity measures as measured from high frequency data and refer to the previous two weeks during rainy season only. p-values in parentheses. We do no clustering procedure here as to give the regression the greatest chance of finding a statistically significant difference between the two groups.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

values with zeros. This extreme assumption generates the lowest possible bound on the results driven by the unbalanced nature of the panel.

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

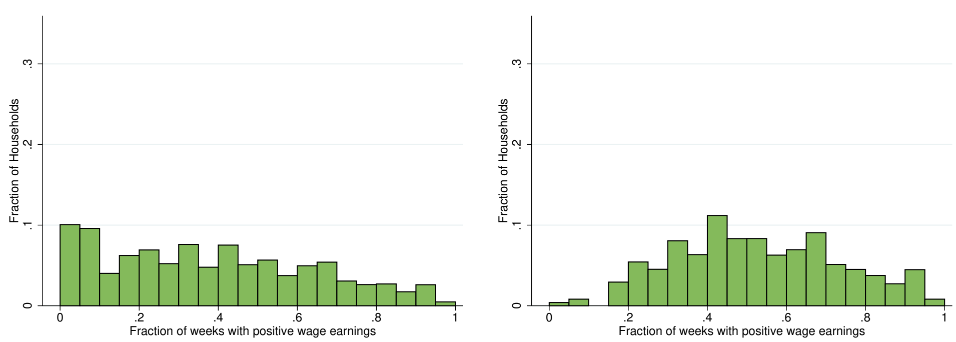
Table 17: Effect of flooding on survey response

	(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 bootstrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 6: Fraction of weeks with labor market income

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



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.

C.4 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.[20] The outcome variable here is an indicator equal to one if the crop is planted (not necessarily harvested), and the results are in Table 18.

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

Table 18: 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: p-values in parentheses are clustered using the wild cluster bootstrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

C.5 Using "current storage" as a direct measure of stored crops

Table 19 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.

Table 19: Direct Measure of Farm Savings

	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

Table notes: These results define savings as the response to the question "How much of crop X do you currently have stored?" p-values in parentheses are clustered using the wild cluster bootstrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$