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

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Abstract

We measure the impact of increasing integration between rural villages and outside labor markets. Seasonal flash floods cause exogenous and unpredictable loss of market access. We build bridges that eliminate this risk. Identification exploits variation in riverbank characteristics that preclude bridge construction in some villages, despite similar need. We collect detailed annual household surveys over three years, and weekly telephone followups to study contemporaneous effects of flooding. Floods decrease labor market income by 18 percent when no bridge is present. Bridges eliminate this effect. The indirect effects on labor market choice, farm investment and profit, and savings are quantitatively important and consistent with the predictions of a general equilibrium model in which farm investment is risky and the labor market can be used to smooth shocks. Improved rural labor market integration increases rural incomes not just through higher wages, but also through these quantitatively important indirect channels.

JEL Classification Codes: O12, O13, O18, J43

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1 Introduction

The majority of households in the developing world live in rural areas where labor markets are poorly integrated across space and productivity is particularly low (Gollin, Lagakos and Waugh, 2014). Increased integration has potentially large benefits in rural areas where household income is derived from both farming and labor markets (Foster and Rosenzweig, 2007). In addition to giving households access to higher wages outside their village (Bryan, Chowdhury and Mobarak, 2014; Bryan and Morten, 2015), it may also affect agricultural choices by reducing distortions in farm input choices (such as fertilizer expenditures) through relaxing credit constraints or allowing farmers to better manage risk.[1] The prevalence of both farming and wage income in rural areas implies that these potential spillover effects may be large, and thus critical for understanding the full effect of labor market integration.

In this paper, we directly study the impact of integrating rural Nicaraguan villages with outside labor markets and show that it has sizable effects on household wage earnings, farm investment decisions, and savings. We use seasonal flash floods as a source of variation in market access. This context is useful for studying market integration for several reasons. First, these flash floods are an unpredictable, exogenous, and observable change to market access. Second, it is both a common phenomenon in the developing world and widely cited as a major development hurdle.[2] Our context therefore allows for a straightforward interpretation of variation in market access while directly addressing a salient constraint to growth in the developing world.

We then build footbridges that connect villages to markets, eliminating the uncertainty of market access. We conduct household-level surveys the year before the bridges are constructed and for two years after. In addition, we collect 64 weeks of data from a subset of households during the same period to understand the contemporaneous impact of flooding on household outcomes. As emphasized in Foster and Rosenzweig (2007), rural households have many income streams with interrelated outcomes. This is true in our context as well, and we directly focus on how mul-

[1] See Rosenzweig and Binswanger (1993) for the depressing effect of weather risk on farm investment and Mobarak and Rosenzweig (2014) and Karlan et al. (2014), among others, for the changes generated by formal rainfall insurance. Jayachandran (2006) and Fink, Jack and Masiye (2017) show how missing credit markets affect agricultural employment and production.

[2] This is true both of 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.

tiple margins are affected by improved outside market access, such as labor market outcomes and agricultural production choices.

Our identification strategy is based on the fact that many villages need bridges, but construction is infeasible for some villages due to the characteristics of the riverbeds that they aim to cross. Because these rivers are typically distant from the houses and farmland of the village (the average village household is 1.5 kilometers from the potential bridge site), the failure to pass the engineering assessment is orthogonal to any relevant household or village characteristics. We verify this by showing that baseline characteristics are balanced across villages that do and do not fulfill the engineering requirements, which we detail in Section 2.

Our results imply that uncertain market access is an important constraint to both labor market access and agricultural productivity, and we find economically and statistically significant effects on both. We first use 64 weeks of high frequency data to study the contemporaneous impact of flooding on household income. In the absence of a bridge, floods depress labor market earnings by 18 percent and increase the probability of reporting no income. When a bridge is constructed, both of these effects disappear. Floods therefore generate uncertain access to labor markets, and a bridge eliminates this uncertainty.

We also find that labor market income increases in non-flood periods once a bridge is constructed. To understand this effect, we turn to our in-depth annual surveys to study the composition of wage earnings and provide details on the mechanisms generating this increase. Men shift their time from relatively low paying jobs in the village to higher paying jobs outside the village. This shift also increases the wage inside the village, causing the inside and outside wages to converge in the response to a bridge. Moreover, this implies that those remaining in the village for work benefit as well, through the general equilibrium impact of the bridge. This result is consistent with Mobarak and Rosenzweig (2014) and Akram, Chowdhury and Mobarak (2016), who find changes in wages in response to increased agricultural investment and rural emigration respectively.

The impact on women is entirely driven by changes in labor force participation. The average household increases its number of women in the labor market by over 60 percent (from 0.17 to 0.27). This is entirely driven by new entrants taking jobs outside the village. We do not find any change in female wages in or outside the village. Thus, the bridge impacts a number of different margins of labor market earnings and participation, but differs by gender.

Finally, we use our high frequency data to show that essentially all households, even those that primarily farm, work off-farm at least some of the time. Thus, labor market access potentially impacts agricultural decisions for a large number of households. This is the case empirically. We find that farmers spend nearly 60 percent more on intermediate inputs (fertilizer and pesticide) in response to a bridge, while farm profit increases by 75 percent.[3] Therefore, the impact of the bridge has substantial effects the agricultural sector as well.

One possible explanation for these results is that bridges makes it easier to directly purchase inputs or get crops to market for sales.[4] As we discuss in Section 2, this is not the case here for a number of reasons. Riverbeds are easily crossable during non-flood periods, and are dry in the absence of flood episodes. Anything that can be stored over a short period of time is therefore unlikely to be affected by the bridge, and it saves minimal travel time when not flooded. Second, the rainy season overlaps with the main crop cycle. Farm inputs are purchased in advance of the rainy season, while crops are harvested after. A bridge is therefore unlikely to directly affect these decisions. Indeed, we find no evidence of a change in crop prices in response to a bridge.

We build a model to investigate a different potential mechanism linking labor market access to agricultural outcomes. Our empirical results require us to take seriously the temporal nature of agricultural decision-making and the response of wages to access. Specifically, we consider how short-run high frequency changes in market access potentially impact longer-run seasonal decisions, and general equilibrium effects, as the village wage rises in response to a bridge. We do so in a small open economy model in which farmers make irreversible fertilizer investment decisions that take time to pay off and are subject to shocks that are unknown at the time of investment. This

[3] It is worth emphasizing that while the effects are large, the treatment is also very expensive ($40,000 each), so these effects are not as outsized as they may seem at first glance. We compute an average return on investment of 19 percent. Approximately two-thirds of this are from the benefits of labor market access, while the other third is the spillover effect into higher farm profit. So although the benefits are large, the returns are not implausibly high given the high costs of bridge construction. See Section 7 for more details.

[4] This idea underlies standard theories of internal trade barriers between urban and rural areas, such as Adamopoulos (2011), Gollin and Rogerson (2014), Sotelo (2016), and Van Leemput (2016).

assumption is also used to motivate formal rainfall insurance programs, such as Mobarak and Rosenzweig (2014) and Karlan et al. (2014). This implies that investment is lower than its efficient level because farmers internalize the impact of this *ex ante* decision on *ex post* consumption. We further include a farmer labor decision in the model, in which farmers can earn wage income by working off-farm or increase yield by remaining on-farm. However, households are subject to an aggregate shock that affects their ability to access the outside labor market, which formalizes the notion of a flood in the model.

We interpret a bridge as a reduction in both the mean and variance of this shock. That is, while the river may still flood, it not longer limits the ability to access the outside labor market. We show theoretically that uninterrupted market access allows households to smooth consumption using off-farm labor income. In the absence of formal insurance, infrastructure allows easier access to labor markets, which households use to smooth income across states of the world. This decreases consumption risk from higher *ex ante* investment, and thus incentivizes farmers to take on more investment, consistent with our empirical results.

However, this partial equilibrium result need not hold in general equilibrium, as the village wage also increases. The higher wage lowers demand for farm labor, which decreases the marginal product of fertilizer and puts downward pressure on fertilizer expenditures. The overall change in fertilizer expenditures thus depends on the relative magnitudes of these two equilibrium effects. The model therefore interprets the positive changes in fertilizer expenditures as the bridge providing a sufficiently large decrease in the distortionary effect of risk net of the wage increase.

We confirm this mechanism by testing a number of other implications predicted by the model. First, a critical implication of the model mechanism is that savings should decrease in response to a bridge, despite the fact that households are richer. In the absence of a bridge, farmers keep a large share of their harvest in storage as a buffer stock to smooth consumption in the face of negative shocks. With the bridge acting as a substitute consumption smoothing technology, households store less harvest and redirect those resources toward fertilizer investment. We test this theoretical result empirically and find support. Agricultural storage among farmers decreases from 90 percent of harvest to 80 percent of harvest in response to a bridge.

Second, we show that there is a strong negative correlation between changes in fertilizer expenditures and crop storage among treated households. That is, those increasing fertilizer expenditure are also the same households that are saving less in response to the bridge. Lastly, we investigate two sources of heterogeneity that the model predicts are important. First, we show that those households with higher consumption at baseline are less affected by the bridge. This is consistent with the model prediction that the rich are relatively less constrained than the poor, and should therefore benefit less. Second, we show that households farther from the bridge site benefit less. Since farther households face higher costs of accessing the market, even after the introduction of a bridge, they should benefit less. All of these results are all consistent with our interpretation of the bridge as a smoothing technology.

A major barrier to studying transportation infrastructure as an intervention is the high cost of construction, which typically limits the ability to identify the underlying mechanisms driving changes or the scope of outcomes considered (Van de Walle, 2008). In our context, each bridge costs $40,000 because of high engineering standards required to survive powerful floods. Because of the high cost, our study includes household-level data from only 15 villages. Our ability to detect statistically significant effects is a function of the intensity of the treatment and low intra-cluster correlations, which average 0.06 among our outcomes. We correct our inference for the small number of clusters by using the wild bootstrap cluster-t procedure (Cameron, Gelbach and Miller, 2008) throughout the paper. Moreover, we provide a number of robustness checks in Section 7. These include robustness to different inference using permutation tests and utilizing the panel dimension of our data to vary the regression specification to include household fixed effects. Lastly, our use of high frequency data can reduce the same size needed to detect a given treatment effect (McKenzie, 2012), which we utilize for our results on labor market income. We are therefore able to detect statistically and economically significant effects, and moreover, the results are robust in terms of both statistical significance and magnitudes.

1.1 Related Literature

Our work relates closely to work on the spatial distribution of labor, including Bryan, Chowdhury and Mobarak (2014), Bryan and Morten (2015), and Akram, Chowdhury and Mobarak (2016). Because off-farm work is an important component of rural income and potentially mitigates risk (Kochar, 1999; Foster and Rosenzweig, 2007), we contribute to this literature by assessing how this (mis-)allocation of labor affects local agricultural production, and how decreasing the cost of labor market access increases agricultural production. Other work has focused on the creation of formal rainfall insurance arrangements designed to decrease the risk inherent in making investment decisions before the realizations of these shocks, such as Cole, Giné and Vickery (2017), Mobarak and Rosenzweig (2014), and Karlan et al. (2014). Similarly, Jayachandran (2006) and Fink, Jack and Masiye (2017) show how labor can be used to move resources over time in the absence of other formal savings markets that make this possible in developed countries.

Our intervention is also related to a growing literature on the benefits of internal trade from better infrastructure investment. In addition to those cited in the introduction, Donaldson (2013), Allen and Arkolakis (2016), Asturias, Garcia-Santana and Ramos (2016), and Alder (2017) focus on the ability of infrastructure to allow easier movement of goods across space. Allen and Atkin (2016) show that the ability to adjust risk-production profiles in response to lower trade costs amplifies the gains from lower internal trade costs among Indian farmers. Our intervention specifically targets the ability to move people more easily across space, while minimizing the effect on the ability to move goods. Yet even in our context, we find economically important effects and provide another potential margin through which infrastructure benefits rural populations.

Lastly, Asher and Novosad (2016) and Shamdasani (2016) show that a large-scale Indian roads program hastens structural transformation, thus moving workers off farms. Dinkelman (2011) finds similar results from electrifying rural South Africa. These papers show the importance of labor movement in response to large-scale infrastructure development, and are complimentary to our work. Our involvement with planning and construction allows us to collect detailed micro data to investigate the underlying mechanisms in a way that is difficult with administrative data. The trade-off, of course, is that we must operate at a smaller scale than these projects. Our data allows us to provide more detailed evidence on the benefits of new infrastructure in the treated locations, but our results are almost certainly an incomplete accounting of the aggregate impact of scaling such an intervention.[5]

2 Background

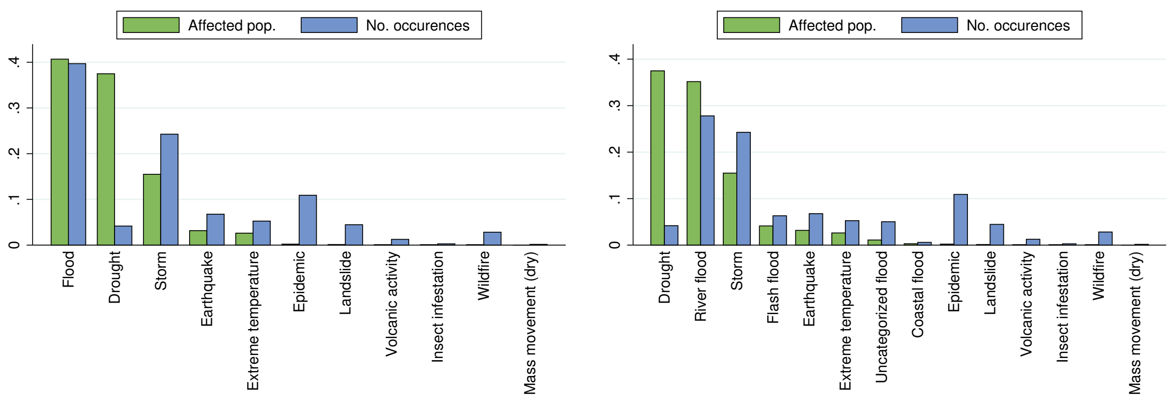
2.1 Flooding Risk

According to EM-DAT (2017), over 40 percent of people affected by disasters worldwide since 2000 are affected by flooding. Of that, nearly all are due to river floods, as shown in Figure 1.[6]

Figure 1: World Disasters (2000-2016)

(a) Different Disaster Types

(b) Finer Decomposition of *Flood*



In Nicaragua, both policy makers and residents cite flooding and the resulting isolation as a critical development constraint (World Bank, 2008*a*). The villages in our sample are located in mountainous areas that face annual seasonal flooding during the rainy season between May and November. This overlaps with the main cropping season as crops are planted in late May and harvested in November.

During the rainy season, floods cause stream and riverbeds that are usually passable on foot to rise rapidly and stay high for days or weeks. This flooding is unpredictable in its timing or intensity. Rainfall in the same location is not necessarily a good predictor of flooding, as rains at higher altitudes may be the cause of the flood-

[5]These villages are typically about 1 percent the size of the markets to which they are connected. In principal, connecting every village to a given market would have important effects in the receiving market.

[6]A disaster is included in the EM-DAT (2017) 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.

ing, a feature of flooding in other parts of the world as well (e.g. Guiteras, Jina and Mobarak, 2015, in Bangladesh). During the baseline rainy season, the average village is flooded for at least one day in 45 percent of the two-week periods we observe it. The average flood lasts for 5 days, but ranges from less than one to 9 days (the ninetieth percentile). On average, this implies that a village is flooded for 2.25 days every two weeks.

During these periods, villages are cut off from access to outside markets. However, it is important to emphasize a number of features of this flooding risk that are relevant for interpreting our results. First, floods are intense torrents of water from the mountains, not simply villages situated next to rivers. Thus, crossing the river by swimming, or any other method, entails substantial risk of injury or death.[7] These floods usually generate prohibitively dangerous crossing conditions or a long journey on foot to reach the market by another route. For our purposes, we interpret a flood as a substantial increase in the cost of reaching outside markets.

Second, these rivers are easily crossable when not flooded, and usually contain little to no standing water. Appendix E shows river crossing locations in a number of sites from the study. As can be seen from the photos, the riverbeds are generally dry when not flooded with clearly marked roads or paths out of the village. Moreover, these villages are not located on deep ravines that make crossing difficult during dry times. This is important for the interpretation of our results, and contrasts this context from standard issues around transportation infrastructure that is used to generate a constant reduction in transportation costs, as in recent work by Adamopoulos (2011), Gollin and Rogerson (2014), and Sotelo (2016).[8]

2.2 Economic Activity in Rural Nicaragua

Crop Cultivation Our study takes place in the provinces of Estelí and Matagalpa in northern Nicaragua. The main cropping season coincides with the rainy season, with planting occurring at the beginning of the rainy season and harvesting happening after it ends. In relation to the discussion above, flooding is therefore unlikely to physically prohibit farmers from access to fertilizer or taking harvest to market.

[7] We are aware of at least two people (one on horseback) in our sample that died trying to cross flooded rivers during the last survey wave.

[8] We find no evidence of effects on the prices of goods, which confirms that those channels are inoperative in our context.

At baseline, 51 percent of households farm some crop. Of those households, 47 percent grow beans and 41 percent grow maize. The next most prevalent crop is sorghum (8 percent). The key cash crops in the region are tobacco and coffee, as Northern Nicaragua climate and geography are well suited for both. However, tobacco and coffee are almost exclusively confined to large plantations. Only 3 percent of households in our sample grow coffee at baseline, while less than one percent grow tobacco. As we discuss below, coffee and tobacco jobs (picking, sorting, etc.) are an important source of off-farm wage work. The modal use of staple crop harvest is home consumption. Over 90 percent of maize and bean harvest is either consumed immediately or stored for future household consumption. The majority of those who sold crops either sell in the outside market (58 percent) or to middlemen who buy in the village and export to other markets (38 percent). Only 4 percent sell to local stores in the same village.

Fertilizer is used by 73 percent of all farming households. While for a developing country this is a relatively high prevalence of fertilizer, fertilizer expenditures are only 16 percent of total harvest value. This share is not quite as low as the poorest African countries, but substantially lower than developed countries (Restuccia, Yang and Zhu, 2008).

The Labor Market We use bi-weekly data collected from households in our sample to show that nearly all households receive labor market income at some point.[9] Figure 2 is a histogram counting the share of weeks each household receives positive labor market income. Despite the fact that 51 percent of households farm at baseline, most are also active in the labor market some of the time. 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.[10] Households are almost never entirely specialized in farming, suggesting potential for a relationship between the labor market and on-farm outcomes, which we study in later sections.

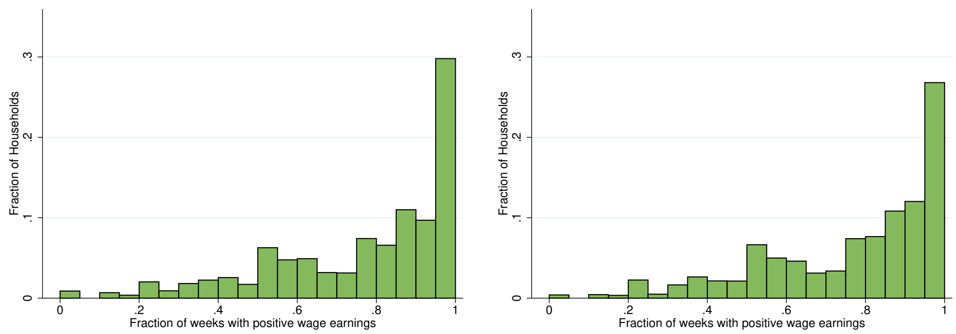
Jobs held by village members are made up of those inside the villages (62 percent)

[9] We discuss data collection in Section 3.

[10] This is a cell phone-based survey. Therefore, 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.2 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. Therefore, the results are robust to even the most conservative possible assumptions on response rates.

Figure 2: Fraction of weeks with labor market income

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



and those employed in the outside markets (38 percent). The latter are at risk of being inaccessible during a flood. Connected markets have between 10,000 and 20,000 people, compared to 150 to 400 people in the small villages we study, so these villages make up only a small fraction of the labor supplied outside the village. Outside-village jobs also pay more on average. There is a 30 percent daily wage premium for men outside the village and an even larger 70 percent daily wage premium for women, though women are employed at a much lower rate.

In both cases, jobs are primarily on short term contracts, operated in spot markets. At baseline, 80 percent of primary jobs held were on short-term (less than one week) contracts. This differs somewhat depending on job location. In the village labor market, 90 percent of all jobs held are short-term, while outside the village 64 percent of jobs are short-term. The majority of jobs in the village relate to farming. Ninety-two percent of jobs held within the village are short-term hired farmhands, while the rest are employed in various other occupations (e.g., clothes washers, teachers, brick makers, carpenters). The modal job outside the village is also farming-related, though typically on large farms producing cash crops. Thirty-five percent work in tobacco or coffee plantations. Workers in outside markets cross the riverbed to reach the market town where trucks pick up workers to bring them to work. Workers are then dropped off at the same location at the end of the day. Thus, the market towns are important staging points for this work. The remaining jobs that account for at least 5 percent of outside market workers are teachers (9 percent), carpenters (9 percent), manufacturing workers (7 percent), brick layers (7 percent), cigar rollers (6 percent), and maids (5 percent).

3 Intervention, Data Collection, and Identification Strategy

3.1 Intervention

The bridges we build traverse potentially flooded riverbeds, thus allowing village members consistent access to outside markets. We partner with Bridges to Prosperity (B2P), a non-governmental organization that specializes in building bridges in rural communities around the world. B2P provides engineering design, construction materials, and skilled labor to the village. Bridges are designed by a lab of civil engineers in the United States in consultation with local field coordinators, who are also engineers. Bridges cannot be crossed by cars, but can support horses, livestock, and motorcycles. A bridge that can survive multiple rainy season requires durable, expensive materials and a sufficiently sophisticated design to overcome issues of rising water levels, soil erosion, and other risks that face infrastructure.

B2P 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. This assessment is made based on the number of people that live in the village, the likelihood that the bridge would be used, proximity to outside markets and available alternatives.

If the village passes the needs assessment, the country manager conducts an engineering assessment. The purpose of this assessment is to determine if a bridge can be built at the proposed site that would be capable of withstanding a flash flood. To be considered feasible, the required bridge cannot exceed a maximum span of 100 meters, and the crests of the riverbed on each side must be of similar height (a differential not exceeding 3 meters). Moreover, evidence of soil erosion is used to estimate water height during a flood. The estimated high water mark must be at least two meters below the proposed bridge deck.[11]

We compare villages that passed both the feasibility and the needs assessments, and therefore received a bridge, to those that passed the needs assessment, but failed

[11] Note that the optimal place to build a bridge need not be the optimal place to cross on foot. See Appendix E for on-foot crossing locations.

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, the characteristics of the riverbed are unlikely to be correlated with any relevant village characteristics. We show that villages that do and do not receive bridges are balanced on their observable characteristics in Table 1.

Because of the bridges each cost \$40,000, the number of bridges that can be funded is limited.[12] 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.[13]

3.2 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 only to collect GPS coordinates from households and sign them up for the high frequency survey. The data used in our analysis comes from surveys conducted at the end of the main rainy season, in November 2014, November 2015, and November 2016. Bridges were constructed in early 2015. Therefore we have surveys from three years for all villages. For those that receive a bridge, we observe one survey without a bridge and two surveys with a bridge. We refer to these survey waves as $t = 0, 1, 2$.

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 all village households. The number of households identified in each village varied widely, from a maximum of 80 to a minimum of 24, with an average household size of 4.2. Participation in the first round of the survey was very high in general, with 97 percent 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 this would bias their answers,

[12]We discuss cost-effectiveness in Section 7. The internal rate of return to the bridge is 14 percent.

[13]The villages are far from one another, so there is no risk that the households in a control village could use the bridge in a treatment village.

or may make them feel they are compelled to answer the survey when they would not otherwise choose to participate.

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

The second component of our data is biweekly follow-up surveys conducted by phone with a subset of households. Because floods are high frequency and short term events, this data shows the contemporaneous effect that flooding has on households. We carried out these surveys for 64 weeks, covering the rainy season before construction, along with the first dry and rainy seasons after construction. Each household was called every two weeks and asked questions about the previous two weeks, 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.

3.3 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. Identification requires that the features required to pass the feasibility test are independent of any relevant household or village-level statistics. To test that these villages are comparable, we run the regression

$$y_{iv} = \alpha + \beta B_v + \varepsilon_{iv}$$

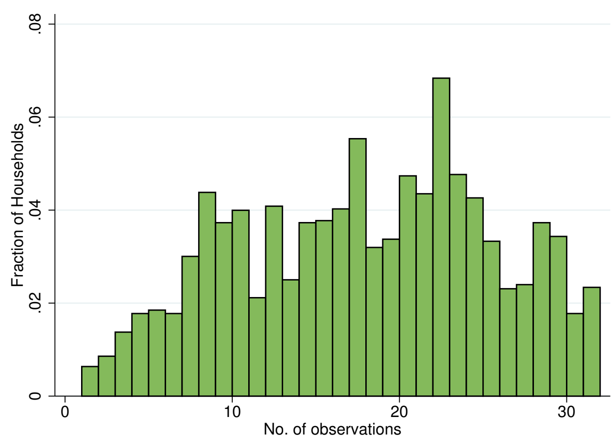
on the baseline data, where $B_v = 1$ if village v gets a bridge between $t = 0$ and $t = 1$. We consider a number of different outcomes, and show that households show no observable differences across the two groups. Table 1 produces the results, and we find no difference across households in build and no-build villages.

3.4 High Frequency Sample Selection

Because the high frequency data was collected by phone, two issues are worth highlighting before turning to the results. First, the high frequency data is not representative of the villages under study as not every individual has a cell phone. Table 12 in Appendix C.1 shows how high frequency respondents compare to the overall populations in the study. As one may suspect with a cell phone-based survey, household characteristics differ slightly between those who participate and those who do not, as respondents tend to be younger and slightly more educated. However, along dimensions such as as wage income and farming outcomes, both groups look similar. Importantly, within the high frequency sample, those in villages that receive a bridge and those that do not have similar characteristics.

The second issue is that the survey is an unbalanced panel as not everyone answered the phone each time. Figure 3 plots the histogram of the number of observations per household in the high frequency data. The minimum is 1, the maximum is 32, and the average is 12. The maximum possible is also 32, as each village is surveyed biweekly.

Figure 3: Number of Observations per Household



4 Empirical Results on Labor Market Earnings

We begin by showing that labor market earnings respond positively to the introduction of a bridge.

4.1 Labor Market Earnings and Floods

We first estimate the relationship between floods and labor market earnings. In the high frequency data, we observe how realized labor earnings depend on contemporaneous flooding in villages. We interact an indicator variable for a bridge being present with flooding to estimate how the relationship between income and flooding changes once the bridge is built. We include household and time fixed effects to control for constant characteristics of households, and for seasonal variation in earnings. Our empirical specification in the high frequency data is:

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

The variable $B_{vt} = 1$ if village v has a bridge in week t, while $NB_{vt} = 1 - B_{vt}$. 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. P-values are computed using the wild bootstrap cluster-t where clustering occurs at the village level. We use two measures of income in regression (4.1): earnings 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.

When bridges are absent, flooding has a strong effect on labor market outcomes. The decline in labor market earnings is $C\$143.6$ ($p = 0.034$), which is 18 percent of mean earnings.[14] Moreover, the propensity to earn no labor market income increases by 7 percent ($p = 0.040$) from a mean of 24.9 percent. However, when a bridge is built the effect on income disappears. In villages with a bridge, flooding is associated with an insignificant increase in income of $C\$5.1$ ($p = 0.874$), and the propensity to report no income actually decreases slightly by 3.8 percent ($p = 0.048$). 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.

Finally, it is notable that bridges increase income even in the absence of the flood. That is, during a non-flooded week, villagers with a bridge earn an average of C$159 ($p = 0.004$) more. Since the bridge is intended to connect the village to outside markets during floods, it is surprising that it has any effect outside of flooding periods. We explore the cause of this finding in depth using the detailed annual surveys in Section 4.3 and find that a bridge causes workers to switch to jobs outside the village. The income gains, therefore, extend beyond just flooding periods. The bridges both smooth income during flood shocks and increases the average income level of households.

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

Figure 4: Density of Income Realizations

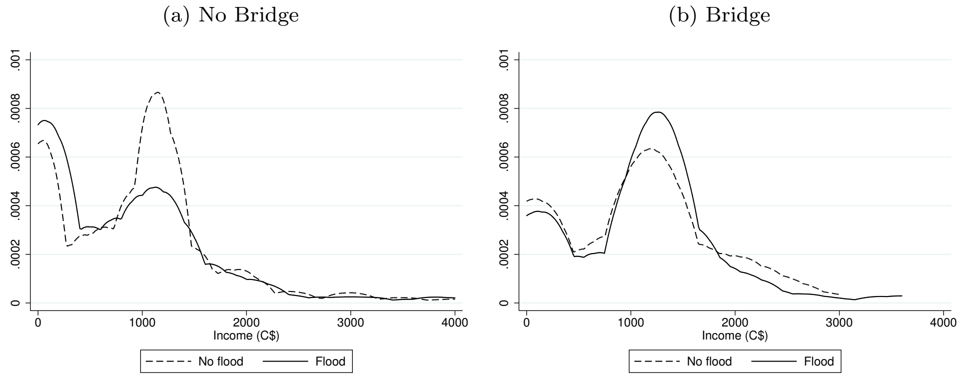


Figure notes: Figure 4a includes all village-weeks without a bridge, including those villages that eventually receive a bridge. Figure 4b includes all village-weeks post-construction.

4.2 Do households substitute intertemporally?

The results in Section 4.1 show that flooding is associated with decreases in earnings during a flood. If a household cannot access the labor market in a given week, they can potentially recoup their lost earnings by increasing earnings in the next (un-flooded) week. However, this need not be the case if on-farm labor productivity shocks are highly correlated with non-farm labor productivity shocks. This would imply that the marginal product of on-farm labor would be high at exactly the time at which control households would wish to increase off-farm labor, thus dampening any effect.[15] We can test for these responses in our high frequency data by including lags in the earnings regression. We therefore run a regression similar to (4.1), but include lags as well

$$y_{ivt} = \beta_0+\beta_1 B_{vt}+\beta_2 F_{vt}+\beta_3(B_{vt}\times F_{vt})+\beta_4 B_{v,t-2}+\beta_5 F_{v,t-2}+\beta_6(B_{v,t-2}\times F_{v,t-2})+\eta_t+\delta_i+\varepsilon_{ivt}. \tag{4.2}$$

$B_{vj} = 1$ if a village has a bridge at week $j \in \{t-2, t\}$ and F_{vj} is defined similarly for floods. The week and household fixed effects are η_t and δ_i. Results are in Table 3. Columns (1) and (3) reproduce the earlier results with no lags, and confirm them. Column (2) shows that the results are inconsistent with control villages responding to floods by increasing future earnings. A flood at time t decreases contemporaneous

[15] Anticipating the model, we allow endogenous responses of this sort. The theoretical results are therefore robust to such intertemporal re-optimization.

earnings by C\$117 ($p = 0.082$), as shown previously in Section 4.1. A flood two weeks in the past implies a statistically insignificant C\$17 decrease ($p = 0.718$), suggesting control households are not responding to past floods with increased current labor market earnings. Column (4) presents a similar result using an indicator for no income earned as the dependent variable. The returns among treatment villagers are consistent with the same theory. Households actually earn C\$126 less ($p = 0.178$) when they were flooded two weeks before, though it is not statistically significant. If anything, these results are consistent with the ability of the *treatment* villages to better adjust to shocks through utilization of the labor market.

4.3 Earnings from Annual Surveys

In the previous sections, we showed that bridges eliminate labor market income risk during floods and also provide a benefit in non-flood periods. We next use our annual surveys to better understand these results. These surveys were conducted at the end of the rainy season from 2014 to 2016 ($t = 0, 1, 2$). Our baseline regression specification is

$$y_{ivt} = \alpha + \beta B_{vt} + \eta_t + \delta_v + \varepsilon_{ivt} \tag{4.3}$$

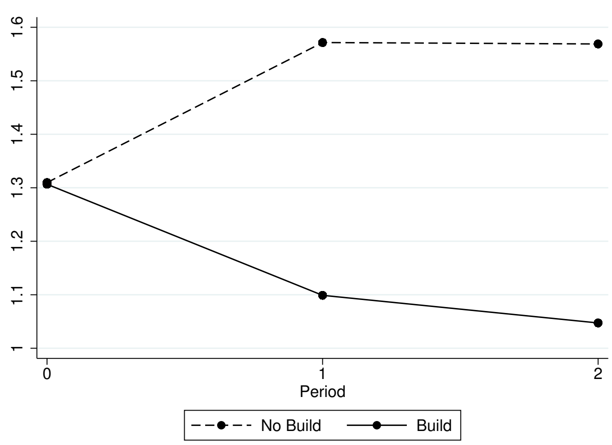
where $B_{vt} = 1$ if a bridge is built, η_t and δ_v are year and village fixed effects. Throughout, we use the wild bootstrap cluster-t at the village level.[16] The results are in Table 4, where we consider total earnings, and also break down the results by gender. Consistent with the previous results, labor market earnings increase by C\$380 ($p = 0.098$). This is almost entirely accounted for by the C\$306 increase in outside earnings ($p = 0.000$). Inside earnings decrease slightly (C\$27.70), but the change is statistically insignificant ($p = 0.828$). The same results hold when one distinguishes by gender. Columns 4 and 7 show that both men and women earn more, and these increases are entirely accounted for by earnings outside the village. For both genders, earnings inside the village decrease slightly, but both treatment effects are statistically insignificant.

We use the detailed employment information in the annual surveys to shed light on the mechanisms that generate these changes in earnings. Table 5 decomposes

[16] See Section 7 for further discussion of robustness. The results are robust to both the inclusion of household fixed effects and alternative clustering procedures.

earnings by the number of household members, daily wages, and days worked. Men shift employment from inside to outside labor market work. In the average household, the number of males working outside increases by 0.19 (p = 0.000), compared to a 0.12 person decrease (p = 0.128) inside the village. Combined they generate a statistically insignificant net change in the number of males employed. Next, we find that male daily wages inside the village increase by C$69 ($p$ = 0.092), consistent with general equilibrium effects resulting from the decreased labor supply induced by the bridge. The male wages outside the village do not change (-C$5.6, p = 0.816) because these villages account for a small fraction of labor market activity outside their village. The wage gap between inside-village and outside-village employment, therefore, converges for men. Figure 5 shows the ratio of the average daly wage net of year fixed effects in treatment and control and confirms this divergence. Lastly, despite men moving to work outside the village, the number of in-village male days worked in the average household changes by an insignificant amount (-0.30, p = 0.418). Thus, those who remain in the village work more intensely at the higher wage. This implies an important spillover effect: even those who do not directly take advantage of the bridge still receive benefits in terms of higher in-village wages.

Figure 5: Relative Male Wage Outside Village to Inside Village



Panel B of Table 5 shows the results for women. The change in total household days worked mirror those for men. Days worked outside the village increase by 0.59 (p = 0.006) while number of days worked in the village do not change (-0.07, p = 0.530). However, the underlying mechanisms for this change are different. Instead of shifting job locations, we see a substantial increase in labor force participation. The average household increases the number of women employed for wages by 0.11 people ($p = 0.006$) over the baseline average of 0.17. This result is entirely due to entry in the outside labor market. The number of employed women nearly doubles outside the village (from 0.12 to 0.22, $p = 0.000$) while there is no change in village employment. Consistent with this, we find no statistically significant changes in wages either inside or outside the village for women. Thus, while the bridge causes men to change where they work, it induces new women into labor market activity.

Both sets of results provide an explanation for the results of the high frequency surveys, namely, that bridges increase labor market earnings in non-flood weeks. As these more detailed results show, both men and women take up jobs outside the village. While the bridge increases access to the market during flood weeks, it also provides an opportunity to access jobs that pay more during non-flood weeks as well. We next build a model to guide our discussion of another type of spillover, the link between labor market access and agricultural decisions.

5 Model

Our goal is to develop a realistic model of agricultural decision-making that takes into account the empirical results of Section 4. This requires taking the temporal nature of agriculture seriously, to link short–run high-frequency changes in market access to longer-term seasonal outcomes on the farm. Moreover, Section 4 shows that we need to take general equilibrium effects into account in any theory proposed. We include both margins in the model we build here, and use it to motivate further empirical tests in the next section.

We model a village as a small open economy, in which consumption goods and farm inputs can be purchased from the larger outside economy. There is an outside labor market in which villagers can choose work at an exogenous wage, w^o. Villagers can work within the village on farms at an endogenous, market-clearing wage w^i.

Within a village, there is a continuum of infinitely-lived households that are endowed with a technology called a farm. Households can save at gross return R, which may be less than one, which we interpret as a low quality crop storage technology.

Throughout, we use the terms household and farmer interchangeably. Farmers are *ex ante* heterogeneous in their ability vector $\mathbf{Z} = (Z_A, Z_L, \varphi)$, which includes their farming ability Z_A, their absolute working ability Z_L, and their comparative advantage working outside the village φ. $\mathbf{Z}$ is constant within a season, but may vary across seasons. Farmers are also subject to aggregate shocks to outside market access τ and on-farm labor productivity ε.

Outside Labor Market Households can work in the outside labor market, which pays a wage w^o per efficiency unit of time. This wage is constant over time.[17]

On-Farm Production Each household owns a farm. These farms produce output using labor and an intermediate input. The timing works as follows. Every T periods, a new season begins. At the beginning of the season, each farmer makes an irreversible intermediate input investment in their farm (e.g., fertilizer or pesticide). Output is harvested at the end of the season, T periods later. The farm technology is given by

$$Y = Z_A X^{\alpha} N^{\gamma}, \tag{5.1}$$

where Z_A is idiosyncratic farmer ability, X is the intermediate input, N is the stock of labor services that have been accumulated, and $\alpha + \gamma < 1$. Each period, the farmer employs labor on their own farm by either hiring workers within the village or by employing their own labor on-farm. The total labor employed in their technology in period t is e_t. The stock of labor N depends on how much labor was employed in each of the T periods within a given season. We allow for the possibility that farm labor is not perfectly substitutable across time, such that the stock of labor services is

$$N = \left(\sum_{t=1}^{T} \varepsilon_t^{\frac{1}{\sigma}} e_t^{1-\frac{1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}}, \tag{5.2}$$

where ε_t is a village-level shock to on-farm labor productivity in a given period.

Labor Allocation Problem The last part of the problem is to decide how each farmer uses her time. She can work outside the village or inside the village. If she works

[17] To more directly close the model, one can assume the existence of a stand-in firm outside the village with production function $Y^o = AN^o$, where A is some fixed productivity term. The gross wage per efficiency unit is $w^o = A$ in any competitive equilibrium.

outside the village, she receives $(1-\tau_t)w^o Z_L \varphi$ while working inside the village generates $w_t^i Z_L$ income. The wage w_t^i is required to clear the within-village labor market. τ is an aggregate shock that controls access to the outside market. If $\tau = 1$, the the market is inaccessible, while $\tau = 0$ implies no cost associated with accessing the market. This implies her realized wage is $w_t = \max\{w_t^i, (1-\tau_t)w^o\varphi\}$, and total wage income is $w_t Z_L$.

5.1 Recursive Formulation

Given the model and timing described above, we can write the household problem recursively. This first requires some notation. Define $s_t = (\mathbf{Z}_1, \tau_1, \varepsilon_1, \ldots, \mathbf{Z}_t, \tau_t, \varepsilon_t) \in \mathcal{S}_t$ as one possible realization of shocks from 1,..., t, which occurs with probability $\pi(s_t)$. Since we assume $\mathbf{Z}$ is fixed between $t = 1$ and $t = T$, it is useful to define the subset of shocks that satisfy this requirement, $\widehat{\mathcal{S}}_t = \{s_t : \mathbf{Z}_{\tilde{t}} = \mathbf{Z}_1 \ \forall$ realizations of $\mathbf{Z}_1$ and all $\tilde{t} = 1, \ldots, t\} \subset \mathcal{S}_t$ and $\widehat{\mathcal{S}}_t(\mathbf{Z_1}) = \{s_t : \mathbf{Z}_{\tilde{t}} = \mathbf{Z}_1 \ \forall \tilde{t} = 1, \ldots, t\}$.[18] With that in hand, the value of beginning a season with asset holdings A and ability $\mathbf{Z}$ is

$$V(A, \mathbf{Z}) = \max_{\{\phi, c_t, e_t, S_t \geq 0\}} \sum_{t=1}^{T} \beta^t \sum_{s_t \in \widehat{\mathcal{S}}_t(\mathbf{Z})} \pi(s_t) u(c_t(s_t)) + \beta^T \sum_{s_T \in \widehat{\mathcal{S}}_T} \pi(s_T) V(A'(s_T), \mathbf{Z}') \quad (5.3)$$

subject to:

$$\begin{aligned}
&\phi \in [0,1] \\
&S_0(s_0) = (1-\phi)A \\
&w_t(s_t) = \max\{w_t^i(s_t), \varphi(1-\tau(s_t))w^o\} \\
&c_t(s_t) = RS_{t-1}(s_{t-1}) - S_t(s_t) + w_t(s_t)Z_L - w_t^i(s_t)e(s_t) \\
&N(s_T) = \left(\sum_{t=1}^{T} \varepsilon(s_t)^{1/\sigma} e(s_t)^{1-1/\sigma} \right)^{\frac{\sigma}{\sigma-1}} \\
&A'(s_T) = S_T(s_t) + Z(\phi A)^{\alpha} N(s_T)^{\gamma}.
\end{aligned}$$

Throughout we will assume that u is strictly increasing, strictly concave and has a

[18] Also, for notational simplicity, we suppress the dependence of the problem on the aggregate state $\mu(A, \mathbf{Z})$ and its transition function $\Lambda(\mu)$.

positive third derivative. The choice variables for consumption (c), savings (S), and labor (e) are measurable with respect to the history of shocks up to that period.[19]

This implies that farmers can adjust to shocks within the season along several different margins. For example, if a farmer receives a high τ realization, she can respond by drawing down her stock of savings, reducing consumption, and adjusting her sectoral labor decisions in whatever way maximizes her continuation utility. Importantly, savings cannot go negative at any point during the season. This creates a motive to maintain a buffer stock of storage to insure against a sequence of bad shock realizations.

Farmer labor market choices follow a cut-off rule

$$\varphi^*(s_t) = \frac{w_t^i(s_t)}{(1-\tau(s_t))w^o}$$

such that all households with $\varphi > \varphi^*$ choose to work outside the village at time t with history s_t. On the other hand, the choice of fertilizer investment $X = \phi A$ is irreversible. Thus, this margin is not directly available for farmers to adjust in response to shocks, consistent with the theoretical motivation on formal agricultural insurance programs (Mobarak and Rosenzweig, 2014; Karlan et al., 2014).

5.2 Equilibrium

The competitive equilibrium of this economy is defined by a distribution $\mu(A, \mathbf{Z})$, a value function V, decision rules o, ϕ, c, e, S, and prices w^i and w^o such that (1) the value function V solves the household's problem given by (5.3) and the constraint set, (2) the law of motion for μ, denoted $\Lambda(\mu)$, is consistent with the shock transitions and the decision rules, and (3) the village labor market clears for all $t = 1, \ldots, T$:

$$\int_{(A,\mathbf{Z}):\varphi \leq \varphi_t^*(s^t;A,\mathbf{Z})} Z_L d\mu(A, \mathbf{Z}) = \int_{(A,\mathbf{Z})} e(s_t; A, \mathbf{Z}) d\mu(A, \mathbf{Z}) \tag{5.4}$$

5.3 Discussion: Nature of the Exercise

Before characterizing the model, it is useful to highlight how our model and analysis map to the data. Our goal is to compare two different infrastructure regimes: one

[19] Note also that our formulation allows for arbitrary time series dependence within a season, but is i.i.d. across seasons. This assumption is not critical for the results, but simplifies exposition.

without a bridge and another that mimics the introduction of a bridge. We have in mind that bridges have two effects. First, they make access to the outside market more consistent, so that the variance in transportation costs should fall. Second, they make it weakly easier to access outside markets in every state of the world. We model introduction of a bridge as a change in the distribution of τ that both reduces the variance of τ and makes it so that $\tau(s_t)$ is weakly lower in every s_t. In particular, we have in mind that flooding events correspond to high τ realizations, and that the bridge dampens those increases in τ.

5.4 Farm Investment Decision

The critical decision that households make is how to divide their farm output between two types of savings: storage and productive investment. Storage 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 is realized, households may experience sharp declines in consumption.

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

$$R^T + \frac{\sum_t \sum_{s_t} R^t \eta(s_t)}{\sum_{s_T} \xi(s_T)} = \alpha Z_A (\phi A)^{\alpha-1} \bar{N}^\gamma \sum_{s_T} \left(\frac{N(s_T)}{\bar{N}} \right)^\gamma \frac{\xi(s_T)}{\sum_{s_T} \xi(s_T)} \tag{5.5}$$

where $\eta(s_t)$ is the Lagrange multiplier on non-negativity of savings, and $\xi(s_t)$ is

$$\xi(s_T) = \beta^T \pi(s_T) V_1(A'(s_t), \mathbf{Z}')$$

and

$$\bar{N} = \left(\sum_{s_T} \pi(s_T) N(s_T)^\gamma \right)^{\frac{1}{\gamma}}.$$

For comparison, the optimality condition of a farmer that maximizes expected profit, as would be the case with perfect credit and insurance markets, would be

$$R^T = \alpha Z_A (\phi A)^{\alpha - 1} \bar{N}^\gamma. \tag{5.6}$$

We refer to the level of ϕ that maximizes expected profit as the undistorted benchmark. In both (5.5) and (5.6), the left-hand side is the marginal value of savings over the course of the season. An additional unit of storage at period 0 is worth R^T at the end of the season. In addition, without credit markets to smooth consumption, the value of an additional unit of storage is that it makes the household less likely to reach its non-negativity constraint on storage, which would limit their ability to mitigate consumption losses from negative shocks. This is the additional term on the left-hand side of (5.5). The right hand side is the marginal value of a unit of productive investment. The difference here is the weights assigned to different shock sequences s_T. In (5.5), households weigh sequences of shock realizations by their impact on the marginal utility at the beginning of the subsequent season. That is, they internalize the fact that *ex ante* fertilizer investment exposes them to significant *ex post* losses if a series of negative shocks are realized. In the undistorted economy, sequences are weighted only by the likelihood they occur, $\pi(s_T)$. Because the value function is concave in A, this shifts weight toward low realizations of shock sequences relative to those assigned by exogenous probabilities π.

With some algebra on (5.5), we can summarize the totality of these distortionary effects as

$$\Delta(A, \mathbf{Z}, s_t) = \frac{\displaystyle\sum_{s_T : s_t \in s_T} \left(\frac{N(s_T)}{\bar{N}}\right)^{\gamma - 1 + 1/\sigma} \frac{\xi(s_T)}{\displaystyle\sum_{s_T : s_t \in s_T} \xi(s_T)}}{1 + \displaystyle\sum_{k \geq t} \sum_{s_k : s_t \in s_k} R^{k-T} \frac{\eta(s_k)}{\displaystyle\sum_{s_T : s_t \in s_T} \xi(s_T)}}. \tag{5.7}$$

This term, while complicated, summarizes how far ϕ is from the undistorted benchmark level. In particular, $1/\Delta \in [1, \infty)$ summarizes the total distortion in the economy. In the undistorted economy, where $\eta = 0$ and $\xi(s_t) = \pi(s_t)$, $\Delta(A, \mathbf{Z}, s_t) = 1$. When any of those terms are positive, $\Delta(A, \mathbf{Z}, s_t) < 1$ and in the limit, $\Delta = 0$.[20]

[20]The fact that $\Delta \leq 1$ follows from the fact that the numerator of (5.7) is always less than or equal to one, as the risk neutral weights ξ shift weight toward outcomes with low N, and the denominator is at least one because η and $\xi \geq 0$, with equality only in the undistorted case. That $\Delta \geq 0$ follows from the fact that all terms in the equation are positive.

Therefore, $\Delta \in (0,1]$ and lower Δ implies a more distorted economy relative to the undistorted benchmark in (5.6).

We can then rearrange (5.5) and use the definition of Δ to derive the share of resources devoted to fertilizer,

$$\phi(A,\mathbf{Z})^{1-\frac{\alpha}{1-\gamma}} = \Omega(A,\mathbf{Z})\Delta(A,\mathbf{Z},s_0)\left(\sum_{s_T}\pi(s_T)\left(\sum_t \varepsilon(s_t)\left[\frac{\Delta(A,\mathbf{Z},s_t)}{w^i(s_t)}\right]^{\sigma-1}\right)^{\frac{\sigma\gamma}{\sigma-1}}\right)^{\frac{1}{\sigma(1-\gamma)}} \tag{5.8}$$

where Ω depends only on the individual state $(A,\mathbf{Z})$ and parameters. Equation (5.5) shows that the share of resources devoted to fertilizer depends on two endogenous pieces of this model – the distortion Δ and the village wage w^i. We first show that a bridge increases Δ. That is, the introduction of a bridge moves farmers closer to the savings and fertilizer choices they would make with access to formal credit and insurance markets.

Proposition 1. *Consider two processes $\tau_0(s_t)$ and $\tau_1(s_t)$ such that $\forall s_t$, $\tau_0(s_t) \geq \tau_1(s_t)$ and that $\forall s_{t-1}$, $Var(\tau_0(s_t)|s_{t-1}) \geq Var(\tau_1(s_t)|s_{t-1})$. Then for every state $(A,\mathbf{Z})$, $\Delta_0(A,\mathbf{Z}) \leq \Delta_1(A,\mathbf{Z})$*

Proof. See Appendix A. ■

The intuition for this result can be seen in equation (5.5). First, the non-negativity constraint on storage implies that households must maintain a buffer stock of savings to insure across negative shock realizations. When the household's income process becomes safer, they are less concerned about the non-negativity constraint binding, as the bridge provides a secondary smoothing technology. This frees resources to be used in investment that were formerly used as a buffer stock. Second, households are risk averse and prudent, so that the reduction in income risk that they face increases their willingness to substitute from safe storage to risky fertilizer investment. Both forces allow the bridge to decrease the distortionary impact of consumption risk on fertilizer expenditures.

Note that an immediate corollary of Proposition 1 is that if this model was in partial equilibrium (that is, $w_0^i(s_t) = w_1^i(s_t)$ for all s_t), then fertilizer expenditures increase and liquid saving decreases. However, the equilibrium wage response breaks the link between the sign of the distortion and the sign of the investment change, as we show in Proposition 2.

Proposition 2. *For the same change in the process for τ considered in Proposition 1, the village wage increases. That is, for all s_t, $w_0^i(s_t) \leq w_1^i(s_t)$.*

Proof. See Appendix A. ■

The intuition for this result is straightforward, as the wage increases in response to a decrease in costs of leaving the village labor market. All else equal, this decreases the labor supply as villagers leave to take advantage of higher paying jobs outside the village, and the village wage therefore responds upward.

Proposition 2 leaves open the theoretical possibility that fertilizer investment actually falls, despite the fact that the distortion is getting smaller in response to a bridge. One can see this in equation (5.8). While Proposition 1 shows that Δ increases, Proposition 2 shows that w^i increases as well. Thus, these two forces push ϕ in opposite directions. If the change in distortion is sufficient small relative to the increase in wage, fertilizer investment will decrease. A particularly extreme example of this is the undistorted economy, where $\Delta = 1$. As can be seen from (5.8), Proposition 2 then immediately implies that fertilizer expenditures decrease in response to a bridge. With no changes in the distortionary effect from a bridge, investment always decreases.

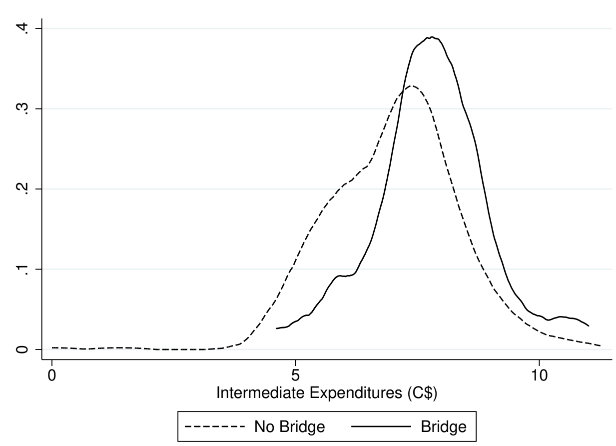
Combining the theoretical results shows that the equilibrium response of fertilizer investment in our model requires balancing these two competing effects. The bridge allows for better consumption smoothing by giving villagers access to a new labor market. This decreases the distortionary effect on investment relative to the first best world and, all else equal, increases fertilizer investment and decreases liquid savings. However, the increase in the wage puts downward pressure on investment by decreasing complimentary labor services. Which of these two effects dominate in equilibrium is therefore an empirical question. If we observe an increase in fertilizer investment, then through the lens of the model this implies not only that farmers are distorted relative to profit-maximizing farms at baseline, but also that a bridge provides a sufficiently large change in the ability to smooth consumption relative to the increase in wage. In the next section, we study the effects of the bridges on agricultural choices and outcomes empirically, and test the predictions generated by our model.

6 Empirical Impact on Agricultural Outcomes and Storage

6.1 Agricultural Input Expenditures and Profit

We now examine the implications of the model empirically by estimating the effect of bridge construction on agricultural decisions of households. The results on agricultural outcomes using regression (4.3) are presented in Table 6. We first consider intermediate input (fertilizer plus pesticide) expenditures, and also the two components individually. These are columns 1-3 in Table 6. First, we see a substantial increase in intermediate expenditures. Intermediate expenditures increase by C\$659.97 ($p = 0.048$) on a baseline of C\$890. The changes are primarily accounted for by fertilizer investment, which increases by C\$383 ($p = 0.026$) compared to a statistically insignificant C\$167 ($p = 0.260$) for pesticide.[21] Figure 6 plots the density of log intermediate expenditures in villages with and without a bridge. Not only does the mean increase, but variance across households falls from 1.33 to 1.21 among those using positive amounts of fertilizer and pesticide.

Figure 6: Density of Log Intermediate Expenditures (C\$)



Columns 4–7 then consider how this increased input use translates into yields on staple crops. We look at changes in harvest for maize and beans, measured in total

[21] These results are for the average household, not the average household engaged in farm work, so the the total amount of intermediate inputs increases substantially in response to a bridge.

quintales (100 pounds) harvested.[22] Here, we find positive but mostly statistically insignificant results, consistent with the fact that farm outcomes are subject to substantial shocks after investment is made. We do find that maize yield increases by 11.90 quintales per acre ($p = 0.004$).

Panel B decomposes these results into differences between households that do and do not farm at baseline. The average effects are entirely driven by continuing farmers. That is, giving baseline farmers easier access to the labor market increases their agricultural investment, consistent with the model developed in Section 5.

Finally, we measure changes in farm profit. This is the value of output produced net of fertilizer and pesticide expenditures and payments to farm labor. Since not all crops are sold at market, we value harvest quantities at the median sale price during the year of production. We compute this value of harvest first only maize and beans, as 90 percent of harvesting households harvest at least one of these two crops. No other crop is planted by more than 10 percent of households, so sale prices are limited outside these main staples. As a robustness check we also include the next two most prevalent crops, sorghum and coffee, as they are planted by 9 and 4 percent of farming households during this period. The results are in Table 7. Farm profit increases by 76 percent. This is consistent with our theory of bridges decreasing distortions. Despite the increase in input costs, profit increases as well.

6.2 Savings Response

The counterpart to increased investment in the model is lower savings. We therefore next consider crop storage, the key liquid savings vehicle in rural Nicaragua. Storage is defined as quantity harvested net of sales, debt payments, gifts, and land payments.[23] Any household with no crop production is given a value of zero in this regression. Table 8 shows how bridges affect savings behavior. Regressions 1 and 3 show the average effect. Farmers save about 9 percentage points less of both their maize harvest ($p = 0.014$) and their bean harvest ($p = 0.052$). Columns (2) and (4) again show that

[22] In Appendix D.1, we show that there is no shift into cash crops in response to a bridge, hence our focus on staple crops here. In Nicaragua, most coffee is grown on large plantations, so this type of shift is *a priori* unlikely. Moreover, newly planted coffee trees do not produce coffee for several years.

[23] In Appendix D.2 we present the results when we define storage as the amount of each crop currently held in the household, which we ask directly. The results are similar. However, "amount currently stored" is net of any already-consumed harvest and is therefore not the total measure of harvest stored. For this reason, we prefer the in-text measure of storage.

the decrease in storage is concentrated among continuing farmers, the same subgroup as those who increase investment. Among continuing farmers, we find decreases of 13 percentage points for maize ($p = 0.016$) and 17 percentage points for beans ($p = 0.056$). Among those who did not farm at baseline, we see small and statistically insignificant changes in storage rates across build and no-build villages.

While changes in the average are consistent with our model, it further predicts that fertilizer expenditures and savings should co-move at the individual level. We therefore correlate changes from baseline intermediate expenditures with changes for baseline storage among treatment households. The correlation is -0.28 when using corn storage and -0.34 for bean storage. Both are statistically significant at the one percent level. Again consistent with the model, those who are increasing fertilizer the most are also those decreasing their savings the most.

6.3 Heterogeneous Effects

We lastly investigate two sources of heterogeneity that the model predicts are important, based on both endogenous and exogenous variation across households.

First, we consider physical distance. While we reduce the cost of crossing a flooded river, this is only one aspect of the total cost of market access. Our intervention does not allow distant households to more easily reach the bridge site. Households vary substantially in their distance from the bridge. The average household at baseline is 1.5 kilometers from the bridge site, with a ninetieth and tenth percentile of 2.9 and 0.2 kilometers. To the extent that this distance increases the cost of accessing the bridge site, the estimated magnitudes may vary within villages. We use household and bridge GPS locations to construct the distance in kilometers to the bridge site for each household, normalized by the distance of the median village household.[24] The second dimension of heterogeneity that matters here is initial consumption. Because of the assumed curvature in the utility function, the benefits to the poor should be larger than those for the rich. We therefore interact the treatment with baseline consumption

[24]In interpreting these results, it is worth noting that we did not find any households that relocated within their village at any point in the survey period. Nicaragua has weak land title rights, and most households report that they have lived in the same place since the Sandinista land reforms of the 1980s. As such, the location of households is unlikely to change in response to a bridge.

expenditures. Using these two sources of heterogeneity, we run the regression

$$X_{ivt} = \alpha + \beta B_{vt} + \gamma D_{iv0} + \delta C_{iv0} + \zeta(B_{vt} \times D_{iv0}) + \theta(B_{vt} \times C_{iv0}) + \eta_t + \delta_v + \varepsilon_{ivt}. \quad (6.1)$$

where X, B, D, and C are intermediate expenditures, bridge, distance, and baseline consumption respectively. Intermediates and consumption are measured as inverse hyperbolic sines (to allow for zeros in intermediate expenditures). The results are in Table 9. The interaction terms, ζ and θ, are both negative as the theory predicts. Increasing baseline consumption by one percent implies a -0.88 percent ($p = 0.002$) decrease in treatment impact. That is, richer individuals are already sufficiently unconstrained that they gain less from the consumption smoothing provided by the bridge. The interaction term for distance has a negative coefficient as well, -0.614 ($p = 0.084$). Both sources of heterogeneity are consistent with detailed predictions of our theory.

7 Further Discussion and Robustness

Before concluding, we discuss a number of potential alternative explanations and show that our results are robust to alternative methods of statistical inference. We discuss statistical significance and treatment effect sizes in more detail given our relatively small number of clusters. Lastly, we compute a return on investment for the intervention.

7.1 Alternative Explanations for Empirical Results

We provide a number of other results to investigate other potential channels. These results are available in Appendix D, but we briefly discus them here.

7.1.1 Prices Change in Agriculture

An alternative explanation for these agricultural results is that prices change. This would occur if bridge construction decreases trade costs and causes prices to converge in equilibrium, as in standard trade theories. Since the prices of staple crops are lower in farming communities than in the broader economy, this causes maize and bean prices to rise. Therefore, farmers increase agricultural inputs and yields rise. This would occur, for instance, if the village is flooded at harvest time so that the farmer is forced to sell their harvest at low prices within the village.

Our survey collects data on the realized prices of sold crops. Table 17 tests whether sale prices change for maize and beans. Prices increase by about 9 percent for maize and beans, but neither is statistically significant. The treatment effect for maize is C$18 ($p = 0.834$) from a baseline control mean of C$189 and the effect on bean price is C$78 ($p = 0.646$) from a baseline control mean of C$871.

To explain this result, recall that the floods under consideration in this context last for days or weeks, but not for a period of time such that these staple crops would experience significant spoilage. As such, although it is true that transportation costs are very high during a flood, farmers can wait for the flood to subside without significant cost and realize the outside market price for their goods.

7.1.2 Land Consolidation

One alternative theory would be that the bridge allows for land to be reallocated more productively.[25] While land transactions are rare in these villages, there do exist informal rental arrangements among households by which the amount of land that they farm can increase or decrease. This could also imply increased agricultural investment and yield, and thus be consistent with our main results. Table 18 tests whether total cropland or rentals (formal and informal) change in response to the bridge, and we find no evidence of such changes. We also test if there is any change in the total share of the population that farms. Consistent with the land use results in regression 1-3, we see only slight, statistically insignificant changes in the propensity to farm.

7.2 Robustness and Discussion of Small Sample

Even though we had only 15 villages in the study, we obtained statistically significant effects. This is less surprising in the high frequency results, as repeated measurement requires less sample size to detect effects. The low frequency data does not benefit from the same design. Here, there are two reasons why we find statistically significant effects. First, the treatment effects are large. Second, the intra-cluster correlations are relatively low. In the main empirical results, the intra-cluster correlations range

[25] For example, this would occur if relatively low skilled farmers move to work in the urban areas and informally rent their land to high skilled farmers.

from 0.002 to 0.108 with both a mean and median of 0.057. This implies that for our median dependent variable, the minimal detectable effect is roughly 69 percent higher than if the randomization were done at the household level.[26] Combined with the large average treatment effects, we are able to detect statistically significant results. However, given the small number of clusters, it is instructive to show that our results are robust. First, we consider a different clustering procedure. Second, we vary the regression specification by including household fixed effects. They are each discussed briefly here, with detailed results provided in Appendix B.

Clustering Procedure We first make statistical inferences using randomized inference. We re-run the main regressions using randomized inference based on Fisher (1935) instead of the wild bootstrap cluster-t procedure of Cameron, Gelbach and Miller (2008). Roughly, while the bootstrap procedure fixes the treatment assignment and selects random samples of the data, randomized inference fixes the data sample but randomly varies the treatment. To compute these "exact p-values," we run our main results for each of the ${}_{15}C_6 = 5005$ possible treatment realizations across villages. Defining $\mathbf{T}_j \in \mathbf{T}$ as the vector of treatment assignments across villages for assignment $j \in \{1, \ldots, 5005\}$, and $\widehat{\beta}_j(y) \in \{\widehat{\beta}_1(y), \ldots, \widehat{\beta}_{5005}(y)\}$ as the estimated treatment effect for outcome y under assignment $\mathbf{T}_j$, we compute the exact p-value for outcome y as

$$p(y) = \frac{\sum_{j=1}^{5005} \mathbb{1}\left[|\widehat{\beta}_j(y)| \geq |\widehat{\beta}_{obs}(y)|\right]}{5005}$$

where $\widehat{\beta}_{obs}(y)$ is the estimated bridge effect for the actual treatment assignment. These are in Appendix B, and we note that the results are quite similar to the results with the wild bootstrap.

In the main body of the paper we prefer the wild bootstrap because, unlike exact p-values, it does not require that villages are i.i.d. between treatment and control. Since this is not the case here, the wild bootstrap cluster-t is the econometrically correct clustering procedure.[27] However, given the use of permutation tests in other small-sample work (Cohen and Dupas, 2010; Bloom et al., 2013), it is still instructive

[26] This calculation assumes all clusters have an average of 33.5 households per village, to simplify the exposition.

[27] Hagemann (2017) uses Monte Carlo simulations to compare a number of placebo methodologies with small clusters. He finds that with a similar number of clusters to what we have here, the cluster-robust version of the wild bootstrap in Cameron, Gelbach and Miller (2008) outperforms alternatives.

to show that our results are robust.

Regression Specification As a robustness check to the regression specifications used here, we utilize the fact that we have three years of data, including observations before and after bridge construction, and estimate the main regressions with household fixed effects instead of village fixed effects. We find similar magnitudes and statistical significance for our estimates. If anything, using household fixed effects make most of the results stronger. As such, the empirical specification used in the main text seems to be a conservative choice relative to other reasonable alternatives.

7.3 Return on Investment of the Bridge

Lastly, we compute the cost-effectiveness of a bridge. Each bridge costs approximately 40,000 USD, or C$1,100,000 at an exchange rate of 0.036 USD = 1 córdoba. We first compute the annualized benefit in terms of increased labor market earnings per household, which is derived from our high frequency data using changes in flooding and average time flooded. In particular, it is computed as

$$\begin{aligned}\text{Annual Effect on Earnings} &= 26 \times \Big(\%\text{ with flood } \times \text{Wage effect in flood weeks} \\ &\qquad + \%\text{ with no flood } \times \text{ Wage effect in no flood}\Big) \\ &= 26 \times \Big(0.095 \times 308.12 + 0.905 \times 159.42\Big) \\ &= C\$4512.21.\end{aligned}$$

The annual effect on the farm is derived directly from the treatment effect on farm profit, C$1957.61, and together imply a total annual effect of C$6489.82. On average, there are 33.5 households per village, which implies a total village benefit of C$216,739. The internal rate of return can be computed as the solution to

$$1,100,000 = \sum_{t=1}^{T} \frac{216,739}{(1+r)^t}$$

where T is the useful life of the bridge in years. Bridges to Prosperity designs bridges to last 40 years.[28] This implies that the internal rate of return is 19.69 percent. If one

[28]This estimate is based on internal tower corrosion rates of 25 microns per year. After 40 years, this is 1 millimeter, which no longer satisfies the design criteria for safety.

were to compute the same rate of return using only the impact on wage earnings, the return would be 13.66 percent. Therefore, not considering the spillovers from labor market access to farm profit would underestimate the annual return of labor market access for rural communities by 31 percent.

8 Conclusion

We study the impact of integrating rural villages with more urban markets. We build footbridges that eliminate the risk of unpredictable seasonal flooding. These bridges have a substantial impact on the rural economy. Bridges eliminate the decrease in contemporaneous income realizations during floods, while allowing individuals to move into better jobs. This increases income during non-flood periods as well. Second, agricultural investment in fertilizer and yields on staple crops both increase. Third, crop storage decreases. These results imply that (1) lack of consistent outside market access can have a substantial impact on long-term agricultural decisions in rural economies and (2) the benefits of infrastructure extend beyond the ability to move goods more easily across space.

We then build a model that links these results together, in which bridges facilitate consumption smoothing through more consistent labor market access, and show that it is consistent with the data. Linking these on- and off-farm channels is important for policy, given the variety of income-generating activities in rural areas (Foster and Rosenzweig, 2007; World Bank, 2008*b*). While we find no evidence of goods price changes, other work focused on larger projects (e.g. Asher and Novosad, 2016) find important implications for structural transformation and off-farm migration. An important avenue for future work is to link results like ours with larger scale projects to better discipline and understand the interaction of trade, structural transformation, and infrastructure. One possible reason for this difference in results is that while much of the literature at the intersection of trade and development has focused on new transportation infrastructure as a constant reduction in the cost of moving between locations, our results suggest that the second moment of trade cost shocks also matters. That is, uncertainty about the ability to access outside markets affects *ex ante* decisions. This possibility has received little attention in the context of developing countries, where this issue is likely to be the most salient.

Lastly, the annual return on investment for these bridges is 19.7 percent over the useful life of the bridge, and over 30 percent of that value comes from the impact on agricultural profit. Despite the high cost ($40,000 per bridge), this type of infrastructure is cost-effective. Understanding how to better target locations with the largest potential benefit would further increase cost-effectiveness, as there is growing acknowledgement of location-based heterogeneity in infrastructure improvements (Allen and Arkolakis, 2016).

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Main Tables for Text

Table 1: Pre-Bridge Differences

	Constant	Bridge
Flooding Intensity		
Days flooded	2.40***	-0.45
	(0.00)	(0.46)
Flood likelihood	0.47***	-0.06
	(0.00)	(0.54)
Flood length (days)	5.10***	-0.36
	(0.00)	(0.84)
Household Characteristics		
Distance to bridge site (km)	1.52***	-0.09
	(0.00)	(0.33)
HH head age	45.05***	-0.08
	(0.00)	(0.95)
HH head yrs. of education	3.43***	0.26
	(0.00)	(0.36)
No. of children	1.28***	0.04
	(0.00)	(0.68)
HH size	4.15***	0.07
	(0.00)	(0.62)
Occupational Choice		
Agricultural production	0.49***	0.05
	(0.00)	(0.26)
Off-farm work	0.57***	-0.03
	(0.00)	(0.47)
Total wage earnings (C$)	1063.80***	1.11
	(0.00)	(1.00)
Farming		
Corn harvest	2.49***	1.00
	(0.00)	(0.21)
Bean harvest	1.50***	0.26
	(0.00)	(0.26)
Plant staples (maize or beans)?	0.34***	0.03
	(0.00)	(0.45)
Fertilizer + pesticide expenditures	899.56***	99.50
	(0.00)	(0.59)
Joint F-test (linear), p-value	0.357	
Chi-squared test (probit), p-value	0.287	

Table notes: Flood intensity measures are from high frequency data and refer to the previous two weeks in the pre-construction rainy season. An observation in these three regressions is a community-week, while the rest are done at the household level. The F and Chi-squared tests are conducted excluding the flood intensity measures. p-values in parentheses. We do not cluster the standard errors here, as to give the regression the greatest chance of finding a statistically significant difference between the two groups.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Effects of Flooding on Income

	Household Income	No Income Earned
Flood × No Bridge	-143.627**	0.070**
	(0.034)	(0.040)
Flood × Bridge	5.071	-0.038**
	(0.874)	(0.048)
Bridge	159.424***	0.061
	(0.004)	(0.110)
Control mean	783.563	0.249
Observations	6443	6756
Individual F.E.	Y	Y
Week F.E.	Y	Y
Intra-cluster correlation	0.080	0.027

Table notes: p-values computed using the wild cluster bootstrap-t with 1000 simulations are in parentheses, clustered at the village level. 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 3: Effects of Flooding on Income with Lags

	Household Income (1)	Household Income (2)	No Income Earned (3)	No Income Earned (4)
Bridge_t	159.424**	84.029	0.061*	0.054
	(0.004)	(0.344)	(0.071)	(0.293)
Flood_t	-143.627**	-117.205*	0.070**	0.044
	(0.012)	(0.082)	(0.034)	(0.123)
$\text{Bridge}_t \times \text{Flood}_t$	148.699**	153.747***	-0.107***	-0.138***
	(0.038)	(0.000)	(0.003)	(0.000)
Bridge_{t-2}		77.443		0.023
		(0.366)		(0.728)
Flood_{t-2}		-17.401		0.005
		(0.718)		(0.854)
$\text{Bridge}_{t-2} \times \text{Flood}_{t-2}$		-125.768		0.035
		(0.178)		(0.345)
Control mean	783.563	783.563	0.249	0.249
Observations	6,443	4,295	6,756	4,589
Individual F.E.	Y	Y	Y	Y
Week F.E.	Y	Y	Y	Y
Intra-cluster correlation	0.080	0.080	0.027	0.027

Table notes: p-values computed using the wild cluster bootstrap-t with 1000 simulations are in parentheses, clustered at the village level. 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 4: Effects on Market Earnings

	All			Men			Women		
	Total	Outside	Inside	Total	Outside	Inside	Total	Outside	Inside
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Build	380.39*	306.10***	-27.70	267.09*	189.34***	-64.37	80.65*	79.21***	-7.53
	(0.098)	(0.000)	(0.828)	(0.062)	(0.006)	(0.282)	(0.082)	(0.000)	(0.790)
Control Mean, $t = 0$	1063.80	357.18	616.27	473.54	210.19	170.43	113.51	62.60	18.23
Observations	1,494	1,493	1,491	1,494	1,492	1,491	1,494	1,491	1,494
Time F.E.	Y	Y	Y	Y	Y	Y	Y	Y	Y
Village F.E.	Y	Y	Y	Y	Y	Y	Y	Y	Y
Intra-cluster correlation	0.073	0.050	0.050	0.049	0.023	0.015	0.027	0.018	0.005

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

Table 5: Decomposing Earnings Changes

Panel A: Men	No. of HH Members			Daily Wage		Days	
	Total	Outside	Inside	Outside	Inside	Outside	Inside
	(1)	(2)	(3)	(5)	(6)	(8)	(9)
Build	0.048	0.192***	-0.120	-5.63	68.57*	0.866***	-0.303
	(0.466)	(0.000)	(0.128)	(0.816)	(0.092)	(0.000)	(0.418)
Control Mean, $t = 0$	0.543	0.294	0.251	182.025	138.980	1.401	1.299
Observations	1,507	1,507	1,507	306	349	1,494	1,497
Time F.E.	Y	Y	Y	Y	Y	Y	Y
Village F.E.	Y	Y	Y	Y	Y	Y	Y
Intra-cluster correlation	0.048	0.041	0.020	0.105	0.000	0.042	0.032
Panel B: Women	No. of HH Members			Daily Wage		Days	
	Total	Outside	Inside	Outside	Inside	Outside	Inside
	(1)	(2)	(3)	(5)	(6)	(8)	(9)
Build	0.109***	0.107***	0.013	44.99	4.45	0.589***	-0.072
	(0.006)	(0.000)	(0.568)	(0.348)	(0.918)	(0.006)	(0.530)
Control Mean, $t = 0$	0.171	0.118	0.055	206.754	121.894	0.538	0.183
Observations	1,507	1,507	1,507	147	107	1,493	1,498
Time F.E.	Y	Y	Y	Y	Y	Y	Y
Village F.E.	Y	Y	Y	Y	Y	Y	Y
Intra-cluster correlation	0.043	0.021	0.019	0.035	0.061	0.019	0.003

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

Table 6: On-Farm Impact

	Input Expenditures			Maize		Beans	
Panel A: Average Farm Outcomes	Intermediates	Fertilizer	Pesticide	Harvest Quantity	Yield	Harvest Quantity	Yield
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Build	659.97**	383.31**	166.52	1.81	11.90***	1.02	2.19
	(0.048)	(0.026)	(0.260)	(0.202)	(0.004)	(0.172)	(0.306)
	Input Expenditures			**Maize**		**Beans**	
Panel B: Intensive and Extensive Margins	Intermediates	Fertilizer	Pesticide	Harvest Quantity	Yield	Harvest Quantity	Yield
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Build × Farm at $t = 0$	1231.48**	702.07**	315.48	4.13*	12.84***	1.58	2.23
	(0.026)	(0.022)	(0.244)	(0.080)	(0.006)	(0.124)	(0.312)
Build × No farm at $t = 0$	-7.16	11.60	-7.96	-0.94	9.22***	0.35	2.07
	(0.958)	(0.932)	(0.918)	(0.264)	(0.008)	(0.634)	(0.342)
Control mean, $t = 0$	889.56	607.43	303.48	2.49	12.29	1.50	4.59
Observations	1,492	1,493	1,492	1,492	359	1,499	356
Time F.E.	Y	Y	Y	Y	Y	Y	Y
Village F.E.	Y	Y	Y	Y	Y	Y	Y
Intra-cluster correlation	0.068	0.051	0.071	0.073	0.097	0.108	0.059

Table notes: Farm at $t = 0 = 1$ if the household is engaged in any crop production at baseline ($t = 0$), where *No Farm = 1 - Farm.* p-values in parentheses are clustered at the village level using the wild cluster bootstrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Farm Profit

	Farm Profit (1)	Farm Profit (2)	Farm Profit (3)	Farm Profit (4)
Build	2223.43***		1957.61**	
	(0.000)		(0.020)	
Build × Farm at $t = 0$		3211.46***		2990.37**
		(0.000)		(0.000)
Build × No farm at $t = 0$		1086.28		768.56
		(0.100)		(0.380)
Control mean, $t = 0$	2351.69	2351.69	2559.20	2559.20
Observations	1,492	1,492	1,493	1,493
Time F.E.	Y	Y	Y	Y
Village F.E.	Y	Y	Y	Y
Intra-cluster correlation	0.077	0.077	0.088	0.088

Table notes: Farm at $t = 0 = 1$ if the household is engaged in any crop production at baseline ($t = 0$), where *No Farm = 1 - Farm.* Regressions 1 and 2 measures harvest value of maize and beans, while regressions 3 and 4 includes sorghum and coffee also. p-values in parentheses are clustered at the village level using the wild cluster bootstrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Farm Savings Choices

	Maize		Beans	
	(1)	(2)	(3)	(4)
Build	-0.085**		-0.091*	
	(0.014)		(0.052)	
Build × Farm at $t = 0$		-0.130		-0.172
		(0.016)**		(0.056)*
Build × No farm at $t = 0$		-0.032		0.007
		(0.286)		(0.824)
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
Village F.E.	Y	Y	Y	Y
Intra-cluster correlation	0.036	0.036	0.048	0.048

Table notes: Farm = 1 if the household is engaged in any crop production at baseline, where *No Farm = 1 - Farm.* p-values in parentheses are clustered at the village level using the wild cluster bootstrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Heterogeneous Impact on Farm Expenditures

	Intermediates (1)	Intermediates (2)	Intermediates (3)	Intermediates (4)
Build	1.045*	9.616***	1.767**	10.845***
	(0.056)	(0.000)	(0.020)	(0.000)
Consumption		0.652**		0.657**
		(0.024)		(0.020)
Build × Consumption		-0.831***		-0.877***
		(0.002)		(0.002)
Distance			0.144	0.166
			(0.644)	(0.552)
Build × Distance			-0.586	-0.614*
			(0.124)	(0.086)
Control mean, $t = 0$	3.458	3.458	3.458	3.458
Observations	1,507	1,507	1,483	1,483
Time F.E.	Y	Y	Y	Y
Village F.E.	Y	Y	Y	Y
Intra-cluster correlation	0.075	0.075	0.075	0.075

Table notes: The dependent variable is the inverse hyperbolic sine of intermediate input expenditures. *Distance* is kilometers from house to bridge site normalized by median distance in the village. *Consumption* is the inverse hyperbolic sine of baseline consumption expenditures. p-values in parentheses are clustered at the village level using the wild cluster bootstrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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A Proofs

A.1 Proof of Proposition 1

Proof. Following the definition of $\Delta(A, \mathbf{Z}, s_t)$ in equation (5.7), we first note that V' is decreasing and convex, so when variation in $A'(s_T)$ decreases, the fall in $\xi(s_T)$ in low $A'(s_T)$ states is greater than in high $A'(s_T)$ states. Therefore, since $A'(s_T) = S(s_T) + Z_A(\phi A)^\alpha N(s_T)^\gamma$, low $N(s_T)$ states also have low $A'(s_T)$. Therefore, if we define a probability measure with mass in a given state given by $\xi(s_T)/\sum \xi(s_T)$, then when τ changes, this probability measure shifts mass from relatively low $N(s_T)$ states to relatively high $N(s_T)$ states. Therefore, the numerator increases. The denominator is obviously decreasing, as less risk implies smaller likelihood and severity of reaching the borrowing constraint. Therefore, $\Delta(A, \mathbf{Z}, s_t)$ unambiguously increases. This proves Proposition 1. ■

A.2 Proof of Proposition 2

Proof. For Proposition 2, first suppose that the inside wage $w^i(s_t)$ was unchanged when the process on τ changed. We guess and verify that no change in wages implies that $\forall s_t, e(s_t)$ increases. Note that labor demand is given by:

$$e(s_t) = \varepsilon(s_t)\left(\frac{\gamma Z_A(\phi A)^\alpha \bar{N}^{\gamma-1+\frac{1}{\sigma}}}{w_t^i(s_t)}\Delta(A, \mathbf{Z}, s_t)\right)^\sigma \tag{A.1}$$

By Proposition 1, $\Delta(A, \mathbf{Z}, s_t)$ is weakly higher. By our guess, $\bar{N}$ is weakly higher. If $w_t^i(s_t)$ is fixed, then the greater $\Delta(A, \mathbf{z}, s_t)$ implies that ϕ is also higher. This verifies the guess that $\forall s_t, e(s_t)$ is weakly higher when the inside wage $w^i(s_t)$ is fixed.

Next, notice that if the inside wage is fixed, then the total labor supplied in inside village labor markets is weakly lower. The total inside village labor supply is given by equation (5.4). Then if $\tau(s_t)$ is weakly lower in every state, then $\varphi^*(s_t)$ is weakly lower in every state, which means that weakly less labor is being employed in local labor markets.

Therefore, when the process on τ changes, in every state s_t both labor demand is greater for every $w^i(s_t)$ and labor supply is smaller for every $w^i(s_t)$. Therefore, the only way that the inside-village labor market can clear is for $w^i(s_t)$ to increase. This proves Proposition 2.

$\blacksquare$

B Robustness of Main Results

Section B.1 recomputes the main results using randomized inference instead of the wild bootstrap cluster-t, while Section B.2 recomputes the main results using household fixed effects instead of village fixed effects.

B.1 Using Randomized Inference

Table 10 recomputes the main results using the randomized inference procedure detailed in Section 7.2. The p-values derived from this procedure are in brackets, while the wild bootstrap cluster-t p-values are included in parentheses for ease of comparison.

Table 10: Main Results with Randomized Inference

	Earnings			Farm Expenditures			Farm Outcomes					Storage	
	Total Earnings	Outside Earnings	Inside Earnings	Intermediates	Fertilizer	Pesticide	Maize Harvest	Maize Yield	Bean Harvest	Bean Yield	Farm Profit	Maize	Beans
	(1)	(2)	(3)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Build	380.39	306.10	-27.70	659.97	383.31	166.52	1.81	11.90	1.02	2.19	1957.61	-0.085	-0.091
	(0.090)*	(0.000)***	(0.828)	(0.048)**	(0.026)**	(0.260)	(0.202)	(0.004)***	(0.172)	(0.306)	(0.020)**	(0.014)**	(0.052)*
	[0.042]††	[0.000]†††	[0.822]	[0.057]†	[0.085]†	[0.252]	[0.163]	[0.006]†††	[0.246]	[0.322]	[0.096]†	[0.032]††	[0.010]††
Control Mean	1025.73	357.18	616.27	889.56	607.43	303.48	2.49	12.29	1.50	4.59	2559.20	0.942	0.928
Observations	1,494	1,493	1,491	1,492	1,493	1,492	1,492	359	1,499	356	1,493	1,507	1,507
Time F.E.	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Village F.E.	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Intra-cluster correlation	0.073	0.073	0.050	0.061	0.040	0.081	0.054	0.082	0.122	0.032	0.088	0.036	0.048

Table notes: This table reports the main results using randomized inference to compute p-values. Those p-values are in brackets. For comparison, the original p-values using the wild bootstrap cluster-t are included as well, in parenthesis. p-values for the wild bootstrap cluster-t are denoted * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, while those using randomized inference are denoted †, ††, and † † †.

B.2 Using Household Fixed Effects

As a robustness check, we compare the following two regression specifications

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

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

The first specification includes village fixed effects (δ_v) and is the main specification in the text. As a robustness test of the specification, we re-compute the main results using household fixed effects (δ_i) instead. The results are in Table 11. We also include the main estimates and p-values from the text for ease of comparison.

Table 11: Main Results with Household Fixed Effects

	Earnings			Farm Expenditures			Farm Outcomes					Storage	
	Total Earnings	Outside Earnings	Inside Earnings	Intermediates	Fertilizer	Pesticide	Maize Harvest	Maize Yield	Bean Harvest	Bean Yield	Farm Profit	Maize	Beans
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Village FE	380.39	306.10	-27.70	659.97	383.31	166.52	1.81	11.90	1.02	2.19	1957.61	-0.085	-0.091
	(0.090)*	(0.000)***	(0.828)	(0.048)**	(0.026)**	(0.260)	(0.202)	(0.004)***	(0.172)	(0.306)	(0.020)**	(0.014)**	(0.052)
Household FE	307.59	295.24	-41.76	646.48	437.81	152.94	1.65	14.76	1.16	3.15	1532.57	-0.085	-0.088
	(0.156)	(0.000)***	(0.726)	(0.012)**	(0.000)***	(0.286)	(0.238)	(0.006)***	(0.032)**	(0.012)**	(0.048)**	(0.022)**	(0.050)
Control Mean, $t = 0$	1025.73	357.18	616.27	889.56	607.43	303.48	2.49	12.29	1.50	4.59	2559.20	0.942	0.928
Observations	1,494	1,493	1,491	1,492	1,493	1,492	1,492	359	1,499	356	1,493	1,507	1,507
Time F.E.	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Intra-cluster correlation	0.073	0.073	0.050	0.061	0.040	0.081	0.054	0.082	0.122	0.032	0.088	0.036	0.048

Table notes: This table reports the main regression specification using household and village fixed effects. Note that these are two separate regressions. 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 High Frequency Details

This appendix covers additional results and details that are useful to understand the results of the paper. Section C.1 and C.2 cover the high frequency survey. The former discusses selection into the survey and balance, while the latter shows (1) response rates are uncorrelated with the likelihood of flooding and (2) even the most extreme assumption on missing values does not invalidate the fact that most individuals work in the labor market sometimes.

C.1 High Frequency Data Balance Checks

Table 12 shows the results from the regression

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

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 $HFNum_{iv}$ is the number of responses for household i in the high frequency survey.

Table 12: Pre-Bridge Differences High Frequency Data

	Constant	Bridge	High-Frequency Responses	Interaction
Household Composition				
Distance to bridge site (km)	1.67***	-0.15	-0.01**	0.00
	(0.00)	(0.29)	(0.02)	(0.83)
HH head age	47.04***	1.26	-0.19**	-0.21
	(0.00)	(0.53)	(0.04)	(0.18)
HH head yrs. of education	3.02***	0.30	0.04**	0.00
	(0.00)	(0.48)	(0.03)	(0.96)
No. of children	1.16***	-0.02	0.01*	0.01
	(0.00)	(0.91)	(0.08)	(0.36)
HH size	3.81***	-0.01	0.03***	0.02
	(0.00)	(0.97)	(0.00)	(0.26)
Occupational Choice				
Agricultural production	0.44***	0.10*	0.004*	-0.005
	(0.00)	(0.09)	(0.098)	(0.273)
Off-farm work	0.53***	-0.02	0.003	-0.00
	(0.00)	(0.77)	(0.26)	(0.91)
Total wage earnings (C$)	1037.68***	182.72	2.45	-21.10
	(0.00)	(0.49)	(0.84)	(0.31)
Farming				
Maize harvest	2.52***	0.23	-0.00	0.09
	(0.00)	(0.85)	(0.0.95)	(0.31)
Bean harvest	1.34***	0.72	0.01	-0.05
	(0.00)	(0.27)	(0.63)	(0.32)
Plant staples (maize or beans)?	0.34***	0.06	0.00	-0.00
	(0.00)	(0.33)	(0.89)	(0.51)
Fertilizer + pesticide expenditures	934.26***	187.54	-3.22	-11.45
	(0.00)	(0.49)	(0.80)	(0.59)

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

We redo the same exercise except with an indicator for whether a household takes part in the high frequency survey.

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

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. The results are in Table 13.

Table 13: Pre-Bridge Differences High Frequency Data

	Constant	Bridge	High-Frequency	Interaction
Household Composition				
Distance to bridge site (km)	1.78***	-0.34*	-0.34**	0.32
	(0.00)	(0.06)	(0.01)	(0.13)
HH head age	51.37***	-1.21	-8.31***	0.83
	(0.00)	(0.64)	(0.00)	(0.78)
HH head yrs. of education	2.48***	0.76	1.25***	-0.65
	(0.00)	(0.18)	(0.00)	(0.32)
No. of children	0.96***	0.11	0.42***	-0.05
	(0.00)	(0.56)	(0.00)	(0.80)
HH size	3.66***	0.00	0.64***	0.16
	(0.00)	(0.99)	(0.00)	(0.62)
Occupational Choice				
Agricultural production	0.45***	0.08	0.06	-0.04
	(0.00)	(0.31)	(0.37)	(0.64)
Off-farm work	0.53***	-0.03	0.05	0.00
	(0.00)	(0.70)	(0.45)	(0.96)
Total wage earnings (C$)	906.02***	297.20	207.95	-407.73
	(0.00)	(0.39)	(0.44)	(0.32)
Farming				
Maize harvest	2.78***	-1.28	-0.39	3.26*
	(0.00)	(0.39)	(0.74)	(0.07)
Bean harvest	1.49***	0.78	0.01	-0.76
	(0.01)	(0.36)	(0.99)	(0.45)
Plant staples (maize or beans)?	0.35***	0.05	-0.01	-0.03
	(0.00)	(0.49)	(0.91)	(0.71)
Fertilizer + pesticide expenditures	962.20***	124.82	-82.31	-44.42
	(0.00)	(0.73)	(0.77)	(0.92)

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

C.2 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 14. 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.

Table 14: 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

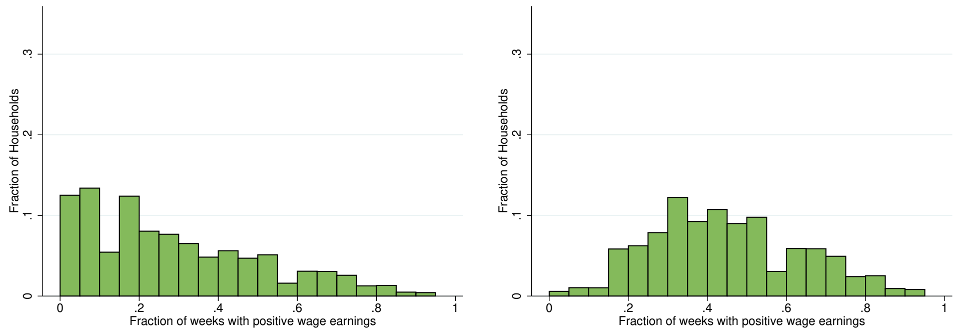
Table notes: p-values in parentheses are clustered using the wild cluster bootstrap-t with 1000 simulations. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

To further emphasize this point, Figure 7 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.

Naturally, this shifts the distribution toward zero. However, even when considering all households, the fifth percentile household still receives labor market income in 3

Figure 7: Fraction of weeks with labor market income

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



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.

D More Results

This appendix covers additional results and details that are useful to understand the results of the paper. Sections D.1 shows no change in crop planting decisions. Section D.2 shows that using the response to the question "How much do you currently have stored in your home?" provides similar results to the storage results in the main text. Sections D.3 and D.4 show that there are no changes in farm output prices and land consolidation. Lastly, Section D.5 provides period-by-period results to show that our results are not driven by a single year.

D.1 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. We considered other cash crops as well, and find similar results to coffee. The outcome variable here is an indicator equal to one if the crop is planted (not necessarily harvested), and the results are in Table 15.

Table 15: Planting Decisions

	Maize	Beans	Coffee
	(1)	(2)	(3)
Build	0.007	0.080	0.004
	(0.912)	(0.164)	(0.780)
Observations	1,507	1,507	1,507
Time F.E.	Y	Y	Y
Village F.E.	Y	Y	Y
Intra-cluster correlation	0.072	0.111	0.071

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

D.2 Using "current storage" as a direct measure of stored crops

Table 16 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 does not measure the total amount of harvest stored, as some was presumably consumed prior to the survey. Nevertheless, the results are similar to those in the main text. The average effect for maize storage becomes insignificant, though the magnitude (-0.113, p = 0.124) is still similar to that in the main text (-0.085, p = 0.014). Importantly, however, the same result emerges that farming households at baseline see the majority of the effect. This is consistent with both the theory and the empirical results in the text.

Table 16: Direct Measure of Farm Savings

	Maize		Beans	
	(1)	(2)	(3)	(4)
Build	-0.113		-0.084*	
	(0.124)		(0.068)	
Build × Farm		-0.210*		-0.163*
		(0.058)		(0.088)
Build × No Farm		0.005		0.011
		(0.884)		(0.728)
Observations	1,507	1,507	1,507	1,507
Time F.E.	Y	Y	Y	Y
Village F.E.	Y	Y	Y	Y
Intra-cluster correlation	0.082	0.082	0.061	0.061

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

D.3 Output Prices for Sold Crops

Table 17: Output Prices

	Maize Price	Bean Price
	(1)	(2)
Build	18.183	78.012
	(0.834)	(0.646)
Control mean, $t = 0$	189.333	871.429
Observations	176	184
Time F.E.	Y	Y
Village F.E.	Y	Y
Intra-cluster correlation	0.129	0.016

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

D.4 Land Use and Farming

Table 18: Land Use and Farm Size

	Total Land Owned	Total Land Cropped	Rent out any land?	Any farming?
	(1)	(2)	(3)	(4)
Build	-0.333	-0.092	-0.018	-0.051
	(0.468)	(0.532)	(0.496)	(0.444)
Control mean, $t = 0$	2.636	1.074	0.067	0.488
Observations	1,601	1,601	1,601	1,601
Time F.E.	Y	Y	Y	Y
Village F.E.	Y	Y	Y	Y
Intra-cluster correlation	0.088	0.112	0.021	0.051

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

D.5 Per-Period Effects

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

$$\begin{aligned} y_{ivt} &= \alpha + \beta B_{vt} + \delta_v + \varepsilon_{ivt} && \text{for t = 0,1} \\ y_{ivt} &= \alpha + \beta B_{vt} + \delta_v + \varepsilon_{ivt} && \text{for t = 0,2.} \end{aligned}$$

Table 19 shows the main results for each period. All of the main results hold period-by-period. Total earnings from $t = 0$ to $t = 2$ is not statistically significant ($p = 0.188$), but the point estimate is still in line with the estimates at $t = 1$.

Table 19: Main Empirical Results by Period

Panel A: t=1	Earnings			Farm Expenditures			Farm Outcomes				Storage	
	Total Earnings	Outside Earnings	Inside Earnings	Intermediates	Fertilizer	Pesticide	Maize Harvest	Maize Yield	Bean Harvest	Bean Yield	Maize	Beans
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Build	404.14**	308.95***	-63.17	659.96*	378.69*	220.58	2.03	11.59***	0.56	1.01	-0.090***	-0.127**
	(0.032)	(0.000)	(0.700)	(0.094)	(0.052)	(0.266)	(0.248)	(0.008)	(0.478)	(0.702)	(0.004)	(0.012)
Control mean, $t = 0$	1025.73	357.18	616.27	612.50	405.60	176.45	1.58	9.03	0.98	3.94	0.936	0.937
Time F.E.	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Village F.E.	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Panel B: t=2	Earnings			Farm Expenditures			Farm Outcomes				Storage	
	Total Earnings	Outside Earnings	Inside Earnings	Intermediates	Fertilizer	Pesticide	Maize Harvest	Maize Yield	Bean Harvest	Bean Yield	Maize	Beans
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Build	362.62	305.08***	18.11	682.59*	415.84**	98.28	1.72	11.22**	1.62**	3.24	-0.082	-0.048
	(0.188)	(0.00)	(0.916)	(0.054)	(0.038)	(0.494)	(0.304)	(0.016)	(0.028)	(0.120)	(0.136)	(0.306)
Control mean, $t = 0$	1025.73	357.18	616.27	612.5	405.60	176.45	1.58	9.03	0.98	3.94	0.937	0.937
Time F.E.	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Village F.E.	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

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

E Dry Riverbed Crossings (Photos)

Figure 8: Riverbeds During Dry Times

(a)

(b)

(c)

(d)