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A Framed Field Experiment

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Abstract

Improving rural credit markets requires a good understanding of the root causes of market failures and taking necessary steps to address them. This paper investigates the role of productivity shocks in borrowers' repayment choices. Using a framed field experiment that simulated a repeated interaction in an input credit market, the analysis finds strong evidence that negative productivity shocks lead to higher default, even when they do not induce negative returns. This relationship is robust to the presence of an information exchange system enforcing dynamic incentives. The findings suggest that recurrent agricultural production shocks resulting from the negative effects of climate change could exacerbate failures in rural credit markets, undermining hard-won progress toward rural financial inclusion.

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Productivity Shocks and Repayment Behavior in Rural Credit Markets: A Framed Field Experiment

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1. INTRODUCTION

Access to credit remains limited for many rural households in developing countries. Credit markets in rural areas of developing countries are generally characterized by market failures associated with imperfect information in the presence of risks (Dorward et al., 1998; Poulton et al., 1998; Sadoulet, 2005; Tedeschi, 2006; Conning and Udry, 2007). These failures persist partly because institutions for contract enforcement are weak, increasing the potential for high default rates. In these conditions, credit providers are more conservative, leading to less than optimal credit supply, or second-best credit arrangements with high interest rates, in rural credit markets (Conning and Udry, 2007). Credit providers will often require assets of significant values as collateral, thereby excluding the poorer households who desperately need capital, regardless of their potential to generate high returns from the loan. This is a situation of market failure in that both credit providers and potentially credit-worthy rural households often lose the potential gain from trade by not completing the transaction.

Limited financial access is often cited amongst the demand-side reasons for the low inputs use and investments by households in rural areas of developing countries (Morris, 2007). Even when farmers know about an agricultural technology or input (e.g: fertilizer, improved seeds), believe in its profitability and can obtain it locally at low transaction costs [1], adoption could be hindered by limited financial means or access to credit.

[1] This set of conditions is rarely met in most developing country contexts

For the reasons cited above, there is a general agreement that innovations that improve financial access for poor rural households with limited collateral should be part of the comprehensive set of actions needed for addressing low rural investments (Kelly et al., 2003). This is critical to promote growth, especially in the agricultural sector. The path to such innovation requires a better understanding of the causes of market failures in rural financial markets, and taking explicit steps to address them. This paper contributes directly to this debate, focusing specifically on opportunistic credit default as a cause of credit market failure.

The available literature on rural finance cites hidden information (adverse selection) and hidden action (moral hazard) as crucial determinants of credit default and thereby market failures (Conning and Udry, 2007). Moral hazard in credit markets can be further decomposed into ex ante and ex post moral hazard. Ex-ante moral hazard often relates to borrowers failing to follow up on their promised investment and production plans, but rather investing in more risky enterprises or investing lower levels of effort than agreed upon with the loan provider (Stiglitz, 1990). Ex-post moral hazard refers to borrowers' decision to simply renege on their commitment, and not repay the loan even if their investment has yielded positive returns (Ghosh and Ray, 2016; Ghosh et al., 1999). This latter situation, called strategic or opportunistic default, is the focus of this study. Unlike ex-ante moral hazard and adverse selection issues which are primarily driven by information asymmetries, opportunistic default stems primarily from the lack of credible and effective sanctions sys-

tems to enforce contracts.

The micro-finance literature has documented the use of asset collateralization, group lending or peer monitoring, and dynamic incentives as ways to overcome information asymmetry issues and opportunistic default in rural markets. The role of collateral is limited by the fact that the poorest often lack valuable assets to put up as a guarantee for the loan. Group lending relies on joint liability as a mechanism to overcome those issues. This approach requires borrowers to sort themselves in groups. Loans could be made to individuals, but the group as a whole is held jointly liable in case of default. The mechanism effectively transfers screening, monitoring, and enforcement costs from the lender to borrowers, providing a cost-effective way for banks to reduce adverse selection, moral hazard and enforcement problems. However, the success of group lending becomes limited when we care about the poorest (Armendáriz de Aghion and Morduch, 2000), or when the group is either non-existent or too large to have the necessary information to ensure repayment (Tedeschi, 2006). Also, as noted by Stiglitz (1990) group lending transfers risks to the borrowers who are perhaps less equipped to absorb them than the lender. Fischer (2013) also shows how joint liability can lead to inefficiencies and limited growth of microfinance-funded enterprises. Therefore it has become a subject of interest to find mechanisms through which individual non-collateralized lending to the poorest can be sustained.

There is a relatively large literature, with an early contribution from Besley (1995) , which has discussed dynamic mechanisms through repeated

interaction and reputation mechanisms as alternative ways to overcome strategic default without relying on group lending based on joint liability. The fundamental idea is that when a borrower depends on successive loans to keep his business functional, the threat of being denied future loans can provide incentives to avoid default in the current period (Hulme and Mosley, 1996; Armendáriz de Aghion and Morduch, 2000; Tedeschi, 2006). Dynamic incentives imply, as noted by Conning and Udry (2007), that it may be possible to generate incentives for good repayment behavior, by conditioning future loan offers on current behavior, provided that the threat of no further loan activity is credible and sufficiently punishing. However, the power of dynamic incentives is reduced when there are multiple lenders competing and when the lender and borrower are not in an exclusive relationship, unless there is a collective punishment mechanism supported by an information exchange system amongst the lenders (Greif, 1993; Greif et al., 1994; Conning and Udry, 2007).

However, the effectiveness of the above-described mechanisms for limiting default risks can be hampered in the presence of productivity shocks affecting the returns to the investments made by the borrower. Productivity shocks can affect repayment behavior through an economic channel by genuinely affecting the borrowers' repayment ability, for example after a net loss. Tedeschi (2006) notes that in dynamic incentives, punishment should instead only be sufficiently long to prevent a borrower from strategic default, but not so long as to unduly punish the borrower that experiences a negative economic shock. Productivity shocks could also affect default risks through

a behavioral channel. As long as the borrower has a concave utility function, repaying after a low return to investments is more costly than repaying after a high return, even when productivity shocks are not important enough to fully impair the ability to repay. Thus increased productivity risk as a results of climate change adds another dimension to the default risks in rural credit markets which could reinforce failures in the market.

This paper contributes to both the micro-finance and the climate change literature. On the micro-finance side, this is the first paper (the authors are aware of) to shed light on a relatively nuanced and overlooked source of strategic default in rural credit markets: productivity shocks (not necessarily leading to negative returns). On the climate change side, this is also the first paper (the authors are aware of) to point to a behavioral mechanism through which climate change may affect rural credit markets, stressing the need for promoting innovative financial products that incorporate such climate concerns. In the paper we developed a simple stylized theoretical model focusing on the impact of productivity shocks on repayment decisions, in the hypothetical settings where farmers were offered access to input credit and had to make repayment decisions after harvest realization. We then implemented an incentivized framed field experiment to test the model predictions. We test this under two experimental conditions: one in which the repayment history of each farmer is made available to all the credit suppliers, thereby facilitating enforcement through dynamic incentives, and one in which repayment history was not made public.

The following section presents the theoretical framework for the key hypotheses underlying the experimental design. Section 3 presents the experimental design and the data generated through its implementation. Section 4 presents the results and discussions from the data analysis. Section 5 concludes with a reflection on how to improve rural credit markets in the face of increasing climate risks affecting the agricultural sector.

2. THEORETICAL MODEL AND PREDICTIONS

A model of input on credit

We develop a simple stylized model of repeated input credit iterations between borrowers and lenders in the context of farm input credit. This model builds largely on the theoretical discussion of rural financial markets in Conning and Udry (2007).

Consider a repeated matching game between a set of agro dealers or brokers $n_s = \{1, , N_s\}$ and a set of farmers $n_b = \{1, , N_b\}$. The farmers need inputs for agricultural production but do not have the capital to pay upfront and thus must rely on credit from the agro dealers. Suppose the agro dealers consider selling on credit in order to maximize the volume of sales. The problem is that to do so, they will have to offer uncollateralized input loans to farmers because farmers do not own valuable assets to provide as collateral. At each stage of the game, each agro dealer plays a 2-player sequential stage game with each farmer. First, at the beginning of the agricultural season, the agro dealer decides whether or not to make an offer of input on credit to the farmer. After harvest, conditional on receiving the input on credit, the

farmer decides whether to repay or not. We assume the use of the agricultural input is always profitable (i.e., that agricultural output is always higher with the use of the input than without). Output without using the input is denoted R_{none} but there is a random productivity shock $\eta = \{Good, Bad\}$ that is realized after the input has been acquired and used. The return to the use of the input, $R = \{R_{good}, R_{bad}\}$, is assumed to be lower when the realized productivity shock is *bad* and higher when it is *good*. But in either case, the net return from using the input is higher than not using the input and is positive (i.e., $R_{good} > R_{bad} > R_{none}$). This assumption does not necessarily reflect the reality for all inputs but allows us to separate farmers' behavioral response to productivity shocks from the economic response due to their inability to repay after a net loss.

The payoff structure is assumed to be as follows. For each transaction initiated with a farmer, the agro dealer gets a payoff of $P - c > 0$ if the farmer does not renege, and $c < 0$ if the farmer does renege. P is the price at which the input is being sold to the farmer, and c is the cost of the input to the agro dealer. The agro dealer reservation payoff in case of no transaction with a farmer is 0. We assume that the agro dealer payoff function in the stage game is additively separable over all the transactions made with farmers in that stage. As for the farmers, they receive a reservation payoff R_{none} if they do not receive an offer in that stage and thus are not able to use inputs. If a farmer receives an offer, their payoff function is described by a mapping $g : \{R_{good}, R_{bad}\} \times \{Renege, Notrenege\} \rightarrow R$. Their payoff depends on their repayment decision and the realization of the productivity shock. We define

$u_{h\eta}$, $u_{c\eta}$, and $\underline{u}$ to be the farmers' state contingent utilities from not reneging, reneging, and not using the agricultural input, respectively.

Farmers' repayment decision function

In a one shot game, we know that the Nash Equilibrium of the sequential game between each agro dealer and any given farmer is the absence of transaction, characterizing the market failure issue presented in the introductory section. However, in a repeated game, it is possible to sustain trade between the parties under some conditions that we characterize below.

In any period t, the present value of the lifetime expected utility to the farmer from never defaulting (V_h) given the realization of the productivity shock $\eta = \{Good, Bad\}$ is:

$$V_{h\eta} = u_{h\eta} + \frac{\delta}{(1-\delta)} \times [E_\eta u_h] \tag{1}$$

where δ and u_h are, respectively, the discount factor and payoff from not reneging as defined earlier. $E_\eta u_h$ is the expected utility of the farmer for periods when he does not renege.

Meanwhile, the present value of the lifetime expected utility from a one-time default is:

$$V_{c\eta} = u_{c\eta} + \frac{\delta}{1-\delta} \times [\theta E_\eta \underline{u} + (1-\theta) E_\eta u_h] \tag{2}$$

where $\eta = \{Good, Bad\}$ and $E_\eta \underline{u}$ is the expected utility of the farmer for periods when he does not receive input credit and, consequently, does not

use inputs. θ is the probability that a defaulting farmer gets punished. θ depends on the strength of the multilateral system in place, and the efficiency with which information about defaulters flows between agro dealers so that defaulters are indeed identified, reported, and duly ostracized.

According to the Nash Folk Theorem (Fudenberg and Tirole, 1991; Mas-Colell et al., 1995), cooperation between farmers and input suppliers can be achieved under the assumptions described above, as long as farmers are patient enough (δ is high enough).

The sustainability condition requires that:

$$V_{h\eta} \geq V_{c\eta} \tag{3}$$

$$u_{h\eta} + \frac{\delta}{(1-\delta)} E_\eta u_h \geq u_{c\eta} + \frac{\delta}{(1-\delta)} [\theta E_\eta \underline{u} + (1-\theta) E_\eta u_h] \tag{4}$$

This is equivalent to:

$$\delta \geq \frac{1}{1 + \theta \frac{E_\eta u_h - E_\eta \underline{u}}{u_{c\eta} - u_{h\eta}}} = \delta_\eta^* \tag{5}$$

Equations 5 demonstrates that in any period, only farmers with a discount factor greater than δ_η^* will not default and trade is sustainable only with those farmers. Assuming that the productivity shock is independently and identically determined in each round, the per-period forgone benefit from continuing to get inputs on credit $E_\eta u_h - E_\eta \underline{u}$ is fixed in each future period.

Therefore, the minimum discount rate required to sustain trade depends mostly on how big the farmers immediate gain from defaulting $u_{c\eta} - u_{h\eta}$ is in the current period. In particular, $u_{c\eta} - u_{h\eta}$ can be interpreted as the opportunity cost of repaying for the input received on credit in the current period, and is a function of the realization of the productivity shock in that period. For risk averse farmers, $u_{c,Good} - u_{h,Good} > u_{c,Bad} - u_{h,Bad}$ and therefore, in the good state of the nature, δ^*_η is lower than in the bad state of nature, ceteris paribus..

Equation 5 also reveals the importance of θ. That is the probability of being detected as a defaulter by other agro dealers. As θ increases the minimum discount rate necessary for the farmer not to default decreases. This probability is related to the credibility and sufficiency of the punishment threat, and is determined by many factors such as the number of input suppliers and the degree of communication between them.

We derive our main empirical hypotheses from equation 5.
Hypothesis 1: Equation 5 indicates that as θ increases, δ^*_η decreases for all η. This leads to the following hypothesis, largely proven in the existing micro-finance literature (Luoto et al., 2007; Tedeschi, 2006; De Janvry et al., 2010): as information about borrowers' credit history is made available to all lenders, the probability of a defaulter being punished increases, and therefore, the probability of default by borrowers decreases.

Hypothesis 2: Equation 5 also indicates that as $u_{c\eta} - u_{h\eta}$ increases, δ^*_η

also increases for all η. That is, as the opportunity cost of repaying increases, the minimum discount rate required for the farmer not to default increases. This leads to the second testable hypothesis, which is the core focus of this paper: in the event of a negative productivity shock, due to the realization of a bad state of nature, the probability of default by farmers receiving inputs on credit increases.

We design a lab-in-the-field experiment (also called framed field experiment) and test primarily the extent to which productivity shocks affect repayment behavior (hypothesis 2), under two credit market conditions: (i) one with public information about borrowers repayment history, and (ii) one with no information available on borrower repayment history. The use of a framed field experiment provides us with some control advantage over the decision making conditions, hence allowing us to test their effects on key outcomes of interests, given that reliable observational data to test our hypotheses are quasi-inexistant. See Harrison and List (2007) for a fuller discussion on field experiments.

3. EXPERIMENTAL DESIGN AND DATA

The experiment was implemented in 10 small villages in Kwara State, Nigeria (see Table 1). Each experimental session (one per village) involved 20 participants selected randomly from a sample of farmers listed beforehand. However, the participants in our experiment are not necessarily representative of the whole population of Kwara State farmers. The villages were selected purposefully to limit overlap with some other villages in which our

reserach team was conducting a separate study. The participants in our experiment were all rice farmers of Haussa and Yoruba ethnicities, mostly male (91 percent), with an average age of 46 years old.

During each round of the experiment, participants were randomly assigned to be either a farmer (who might receive inputs on credit), or a paid broker of an agro dealer (henceforth, agro broker). Each session had 4 agro brokers and 16 farmers and participants remained in the same role for the entire experiment.

Each experimental session consisted of 10 or 11 rounds. After the 9th round in each village, a coin was flipped at the end of each round to determine whether to continue an additional round of the game or not. This is to establish a random stopping point of the game and reduce farmers incentive to behave strategically in the last rounds. During the experiment, each round represented an agricultural season and the decisions made by participants were based on simulating the important aspects of actual input credit markets. As such, each round consisted of two periods a pre-planting period and a post-harvest period. In the pre-planting period, the agro brokers offered inputs on credit to the farmers and the farmers decided which (if any) agro broker's offer to accept. In the post-harvest period, farmers harvest returns were determined (based on weather and input use) and farmers would choose whether to repay the agro broker for the input or not. The possible decisions and their payoff implications for agro brokers and farmers are described in the following sections.

Table 1: Experiment Villages in Kwara State, Nigeria

Local Government (LGA)	Village Name	Communication	Number of rounds
PATIGI	AGBOORO	Yes	10
PATIGI	CHAKYAGI	No	10
EDU	CHEWURU	Yes	11
EDU	CHIKANGI	No	10
EDU	CHIKANGI TIFIN	Yes	11
EDU	EFFAGI	No	10
EDU	GBARIGI	Yes	11
EDU	KPANGULU	No	10
PATIGI	KUSOGI GANA TSWALU	Yes	10
PATIGI	SHESHI TASHA	No	10

The experiment is designed to simulate a multiple-round market for inputs-on-credit and test the role of productivity shock on farmers repayment behavior. This effectively tests hypothesis 2 derived from our theoretical framework. To do so, a round-level treatment variable is generated. Specifically, in each round of the game, we allow the weather to take on one of two states: good weather or bad weather. Good weather implies that productivity and profitability of farmers, when using inputs, are highest. In case of bad weather, productivity while using inputs is lower than in good weather state, but still better than not using inputs at all. Following hypothesis 2, we expect lower levels of farmer default in rounds with good weather than in rounds with bad weather. In each round, the weather state is determined by

the flip of a coin after credit decisions were made by the agro brokers, but before repayment decisions are made by the farmers.

In addition, five out of the 10 study villages were randomly assigned to receive an information or communication treatment. In those five villages the repayment history of all the farmers was made public to all the brokers before they made credit offers. This availability of information is expected to reinforce dynamic incentives (hypothesis 1), and allow us to explore how robust farmers' repayment responses to productivity shock is in the presence of dynamic incentives.

Decisions and Payoffs for Agro Brokers

Each of the four agro brokers in each village began each round with 300 kg of fertilizer to potentially be sold on credit to farmers. In the pre-planting period, the broker decided, for each farmer, whether to offer input on credit or not. To simplify the decisions, we assumed that the input comes in bags of fixed quantities (say 100kg) and each farmer only needs 100kg.[2] Therefore, an offer made to a farmer implied 100kg of input offered to the farmer by the broker. This means that the broker could make offers to at most 3 farmers in each round. Once offered, each farmer could accept or decline the offer. In the post-harvest period, agro brokers received payments from the farmers

[2]This assumption is for pure simplification and does not particularly influence the outcome of the experiment. Conning and Udry (2007) made a similar assumption when modeling moral hazard in rural financial markets.

to whom they made input loans. The value of the input loaned was set to N100 per kg[3]. Thus a farmer who borrowed 100kg of fertilizer from an agro broker would be expected to repay N10,000. However, the actual amount received and the agro brokers commission/penalty depends on the farmers repayment decision. The farmers had the option to: not repay at all (0% of amount owed), partially repay (50% of amount owed), or repay in full (100% of amount owed). The possible outcomes for an agro broker, from any given farmer who received inputs on credit, are summarized in the Table 2.

Table 2: Brokers commission/Penalty Schedule

	Description	Amount/Value	
		No offer made	Offer made
	Amount of fertilizer loaned	0	100kg/N10000
Repay in full	Amount collected	0	N10000
	Brokers commission/penalty	0	N2000
50% repayment	Amount collected	0	N5000
	Brokers commission/penalty	0	-N1500
0% repayment	Amount collected	0	N0
	Brokers commission/penalty	0	-N3000

Overall the agro brokers earnings from input sales during a round consisted of two parts. (i) first, a base salary of N3000 paid if at least one

[3] 100 NGN=0.6200 USD at the time of the experiment (https://www.poundsterlinglive.com/best-exchange-rates/us-dollar-to-nigerian-naira-exchange-rate-on-2014-07-14).

farmer accepted an offer from the broker in question[4]; (ii) second, the commissions/penalties from the repayment of loans made to farmers (3 or less per broker). As shown in Table 2, the broker receives a N2000 commission for every sale where repayment is complete but a penalty is imposed every time he offers inputs to farmers who do not repay fully. If a repayment is partial the agro broker has to pay a penalty of N1500 to the input dealer. Similarly, if the farmer repays nothing, the agro broker has to pay a penalty of N3000 to the input dealer. Note that, given the penalties, it is possible for the agro broker to lose money in a round. For example, assume that an agro broker makes offers to 3 different farmers and they all accept. The broker thus gets the base salary of N3,000. If all the farmers decide to fully default, the broker loses N3,000 per farmer or N9,000 total. Overall, the broker has a net loss of N6,000. In order to avoid the possibility that the broker owed us money at the end of the experimental session, every broker was promised N50,000, to be paid at the end of the session, provided that they had made at least one loan in any round. Net payments to agro dealers per round could vary from a loss of N6,000 as illustrated above to a net gain of N9,000 if three offers are accepted and fully repaid.

Decisions and Payoffs for Farmers

As described above, in each round farmers received offers from the agro brokers in the pre-planting period and decided to accept or reject the loan offer. Note that, to simplify the game, farmers could only accept fertilizer

[4]This base salary was designed simply to incentivize agro brokers in the game to make offers.

on credit from one agro broker in a specific round (100kg). Furthermore, fertilizer was assumed to always be advantageous for farmers. This means that fertilizer always increased yields and farmers' payoffs. There was also no alternative mechanism for farmers to get fertilizer. They are, by design, cash-constrained and fully dependent upon the loans for access to fertilizer. This was done to ensure that all the farmers had the same resources available to them at the beginning of a round/season. In the post-harvest period, the weather for the season was determined via a coin-flip (a single coin flip applied to all farmers and individual farmers were invited to flip the coin) and this, along with whether they received fertilizer, determined harvest yields.

As shown in Table 4, harvest yields were represented in terms of monetary returns to investment. Specifically, if the farmers used fertilizer and the weather turned out bad, they earned N13,000, while if the weather turned out good they earned N16,000. If they did not use fertilizer, the returns were much lower (N1,000) and were independent of the weather. After learning about the weather and resulting earnings, farmers who had received fertilizer, in the pre planting period, then chose a level of repayment (0%, 50%, or 100%). Given that the fertilizer on credit cost N10,000 (N100/kg), the possible per-round earnings for a farmer are shown in Table 3.

Information Treatment Variation and General Implementation

The communication treatment sessions differed from the non-communication sessions in that the agro brokers were given complete information about all farmers past repayment behavior in the game. This was done through a record kept publicly on a board in front of all the participants. The repay-

ment record board was updated after each round, thus showing each farmer's repayment decision in previous rounds. This implies that when a farmer does not repay the credit taken from a specific broker in a specific round, all other brokers will know about it before they make credit offers in the subsequent rounds. Farmers in these sessions were informed prior to the start of the game that their repayment behavior would be made public.

The experiment was paper-based in that agro brokers and farmers made decisions using decision sheets, but the data were recorded and payment amounts calculated using a computer. A team of six facilitators ran each experimental session. Once all participants were present, the instructions were first explained to them and any clarification questions answered. Participants were then separated into farmer and agro broker groups and received the appropriate decision sheets (broker sheet and farmer sheet). To give participants a chance to see the game in action and to ask questions a trial (non incentivized) practice round was first performed. During the actual experiment, all decisions were anonymous in that brokers and farmers were assigned unique numbers and all decisions were entered on paper and communicated to other relevant participants via collection and transcription of decision sheets by the facilitators. Each experimental session lasted about 3 hours including the introductory and Q&A sessions preceding the actual rounds of the game.

Table 3: Farmers payoff structure

Description		Amount/Value	
		No offer received	Offer received
Amount of fertilizer received (kg)		0	100kg
Low Return to investment (Bad Weather state)		**N1000**	**N13000**
If full repayment	Amount paid	0	N10000
	Farmer's net payoff	N1000	N3000
If partial (50%) repayment	Amount paid	0	N5000
	Farmer's net payoff	N1000	N8000
If no repayment	Amount paid	0	N0
	Farmer's net payoff	N1000	N13000
High Return to investment (Good Weather state)		**N1000**	**N16000**
If full repayment	Amount paid	0	N10000
	Farmer's net payoff	N1000	N6000
If partial (50%) repayment	Amount paid	0	N5000
	Farmer's net payoff	N1000	N11000
If no repayment	Amount paid	0	N0
	Farmer's net payoff	N1000	N16000

4. RESULTS

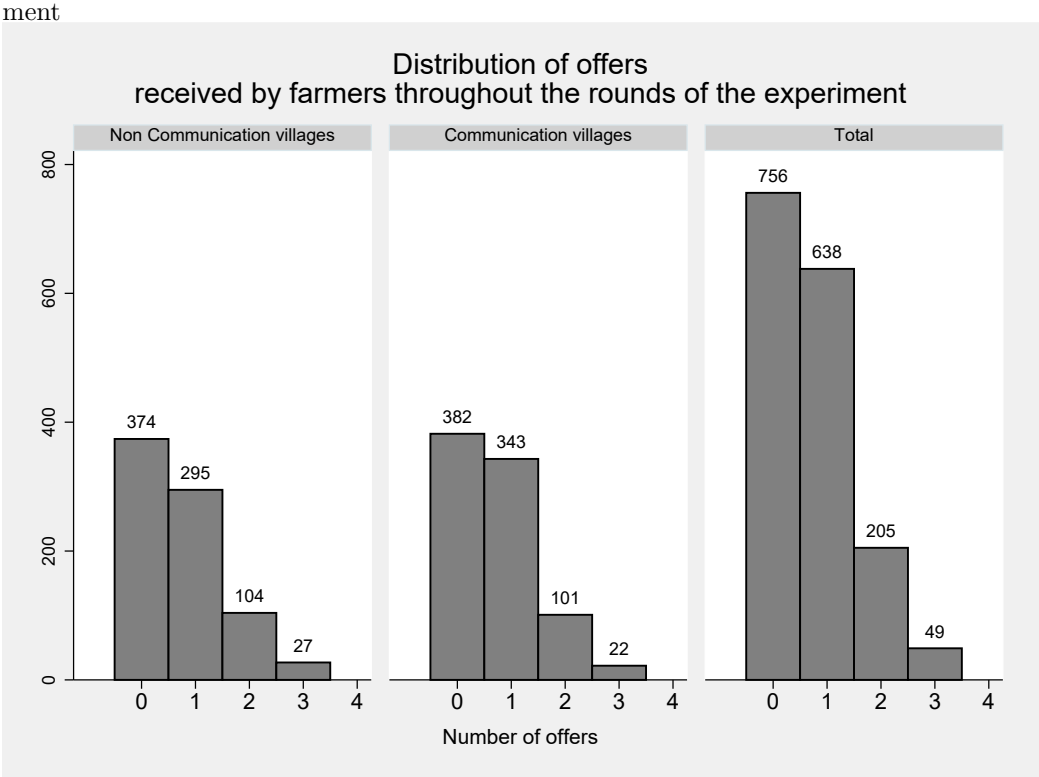
4.1. DESCRIPTIVE ANALYSIS

Data

The experiment generated overall 1,648 observations of farmer-round level variables across all the rounds (10 rounds in 7 villages and 11 rounds in 3 villages), with 16 farmers in each village. These observations are almost evenly distributed across "communication" and "no-communication" villages. The total number of actual or successful "transactions" from which we observe repayment behavior is 892, representing about 54 percent of all farmer-round observations. Of these 892 successful transactions, 426 are from "no-communication" villages and 466 from "communication" villages. In each round of the experiment, at most 12 out of 16 farmers per villages are able to receive an offer. This is because each of the 4 brokers has a maximum of 3 offers to make per round of the game. Given that the experiment did not include coordinated offers by the brokers, farmers could receive more than one offer in a typical round (See Figure 1), but could accept only one offer.

The coin flip which determined the round-level weather state or productivity shock treatment led to about 47 percent of (farmer-round) observations being assigned to the "good weather state". However it appears that a significantly higher proportion of observations were assigned to the "good weather state" in "no-communication" villages (56 percent) compared to "communication" villages (39 percent).

Figure 1: Distribution of offers received by farmers throughout the rounds of the experiment



Notes: The bars in each of these histograms represent the number of observations (farmer-round) associated with each potential number of offers (0-4). From left to right, the graph shows the distribution in Non Communicatin Villages, Communication Villages, and Overall.

Repayment rates by treatment status

The summary statistics on repayment rates across treatment status are presented in Table 4 and Figure 2. An analysis of those descriptive statistics indicates the following.

First, the participants in our experiment exhibit a generally high propensity to default. Column (3) of Table 4 indicates that, overall, across all treatment groups, only about 40 percent of credit transactions received were repaid fully while 13 percent recorded a complete default. While the rate of complete default is relatively low, it seems that more than 50 percent of the time when farmers in the game chose to not default completely, they chose to only repay partially rather than repaying fully. Such high propensity to default mirrors the reality of rural credit markets in developing countries, and transcribes well the role of opportunistic default as source of market failure in rural credit markets in developing countries.

Consistent with our hypotheses, we observe that a large proportion of the default recorded is driven by the bad weather state. Columns (1) and (2) of table 4 summarize the distribution of repayment choices under the two weather states and they appear significantly different. In case of bad weather, only about 27 percent of credit transactions are fully repaid, compared to 55 percent in case of good weather. This 28 percentage point difference in the proportion of full repayment is statistically significant at 1%. When we consider complete default specifically, we also notice that it is considerably higher in bad weather state (17 percent) compared to good weather state (8

percent) and this difference is statistically significant at 1%.

Exploring whether this patterns of repayment and weather state is consistent accross communication and non communication villages, we find, as displayed in Figure 2, that the histograms of repayment decisions follow similar patterns in communication versus non communication villages. However the difference is striking when we compare good weather versus bad weather histograms. On the left panel of the figure, are the bad weather state histograms, and the right panel is occupied by the good weather state histograms. Consistent with Table 4, the bad weather histogram appears similar to an *olympic podium*, with the highest proportion of transactions being partially repaid. In the good weather state the histogram appears as an *ascending stair*, with the highest proportion of transactions being fully repaid, and the lowest proportion going to complete default.

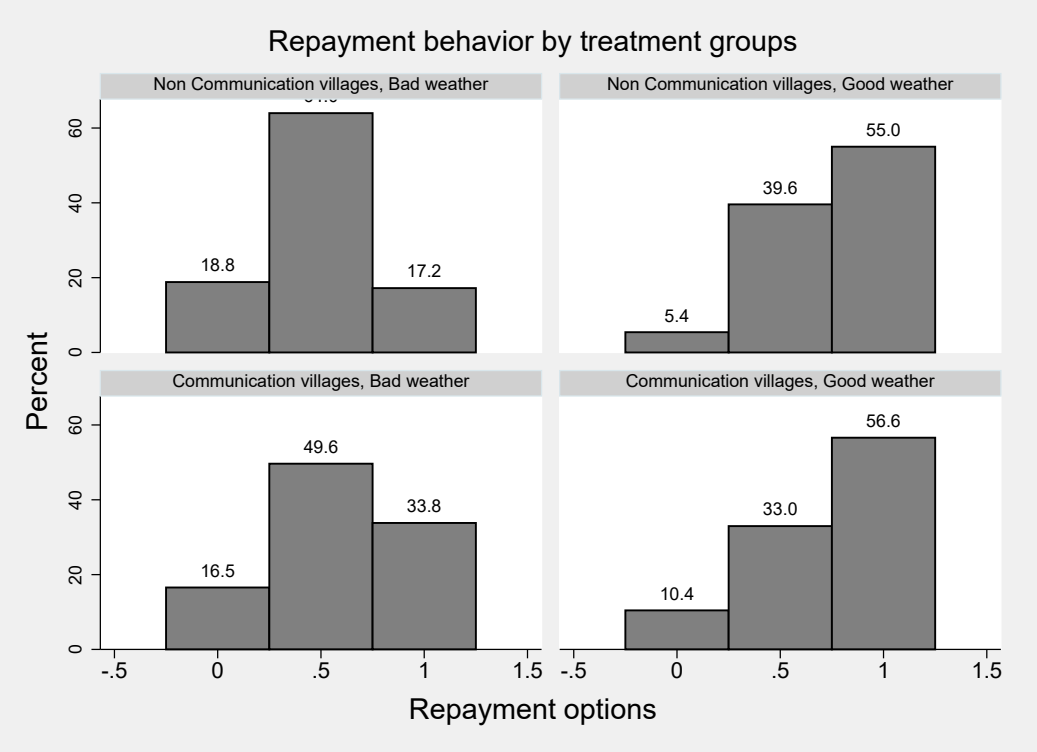
We support this descriptive evidence with a set of econometric analyses presented in the section below.

Table 4: Descriptive Analysis

	(1)		(2)		(3)		T-test
	Bad weather		Good weather		Total		Difference
Variable	N	Mean/SE	N	Mean/SE	N	Mean/SE	(1)-(2)
Complete Default	470	0.174	422	0.076	892	0.128	0.099***
		(0.018)		(0.013)		(0.011)	
Partial Default	470	0.553	422	0.367	892	0.465	0.186***
		(0.023)		(0.023)		(0.017)	
Full Repayment	470	0.272	422	0.557	892	0.407	-0.285***
		(0.021)		(0.024)		(0.016)	

Notes: The table presents the mean, standard error, and number of observations of the main outcome variables under Bad weather [1], Good weather [2], and overall [3]. The last column presents the t-test of significance of the difference in means between the bad weather and Good weather values. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Figure 2: Repayment Histogram by Treatment Groups



Notes: The histograms depict the distribution of the repayment choices made by farmers during the game by Communication and Weather treatment status.

4.2. ECONOMETRIC ANALYSIS

Empirical Model

The randomization strategy used to assign each of the two treatments ensures that the simple difference in means by treatment status is unbiased for the average treatment effects (Athey and Imbens, 2017) [5] We implement such strategy in a regression framework using the following linear model:

$$Y_i t = \beta_0 + \beta_1 T1_{it} + \beta_2 T2_{it} + \beta_3 (T1_{it} * T2_{it}) + \sum_{t=2}^{11} \delta_t * Round_t + \epsilon_{it} \quad (6)$$

where Y_{it} represents the observed repayment decision made by farmer i in round t. We estimate equation 6 not only using the 3-category repayment decision, but also with its binary transformation capturing full repayment, taking value 1 if repayment choice is 100 percent (full repayment), and zero otherwise. $T1$ is the binary *communication* treatment variable taking value 1 if a farmer reside in a communication village and 0 otherwise. Meanwhile, $T2$ is the binary *weather state* variable taking value 1 when the weather is good and 0 otherwise. The model in equation 6 includes an interaction term capturing how communication and weather state variables influence each others' effects on farmers' repayment behavior. Finally dummy variables for round fixed effects are included. The βs are the parameters to be estimated, while ϵ is the vector of random error term. We estimate this equation over-

[5] Ideally, we would use some covariates to verify that the randomization process yielded well balanced covariates across treatment groups, if we had access to such covariates. But no other variable was collected during the experiment precluding such test. We are nevertheless confident that the randomization process was carried out carefully enough to yield such balancedness.

all in our sample, but also in the communication villages and then in the non-communication villages only, to explore how these sub group analyses influence our results.

Equation 6 is consistent with a Linear Probability Model (LPM) and we estimate it using Ordinary Least Square (OLS) approach. The standard errors are clustered at the village-round level to account for the fact that the weather shock treatment is assigned at the level of the round in each village[6]. The choice of a Linear Probability Model (LPM) is justified by its ease of estimation and interpretation, and its robustness to distributional assumptions. The coefficients from the LPM give direct estimates of the average effects of the explanatory variables on the response probability, and allow for straighforward integration of interactions or quadratic terms. In addition, the average partial effects produced from non linear models (which would have allowed us to parsimonously allow the partial effect to vary with the explanatory variables), capturing the non linearity of the outcome variable, rely mainly on the assumption of normality of the error term, and can lead to biaised estimates in case such assumption is not satisfied (Wooldridge, 2010). Our primary interest being in average partial effects, we maintain the LPM estimated with OLS as our prefered model as this generally yields good estimates of average effects. We also run the non linear models estimated using Maximum Likelihood approach, to ensure that our results do no differ

[6]Ideally, we would have clustered the standard errors at the village level because the communication treatment is assigned at the village level. But given the small number of village clusters, the asymptotic properties justifying the clustering are no longer valid.

qualitatively[7]. We find very similar results, which supports our use of the LPM as our primary approach.

Regression Results

Tables 5 and 6 present the results of the OLS regressions of the repayment decisions of farmers on the treatment variables of interest.

The regressions in Table 5 use, as a dependent variable, the binary repayment outcome taking value 1 if repayment choice is 100 percent (full repayment), and zero otherwise. The table contains three main result panels. Panel 1 reports the overall results, while panel 2 and 3 report the results in communication and non communication villages respectively. The results in the table are consistent with the descriptive analysis. Farmers in communication villages seem more likely to repay fully than those in non communication villages, confirming the findings from previous studies that credit information systems such as credit bureaus, have an important role to play in improving outcomes in rural credit markets where institutions for contract enforcement are weak or inexistent (Luoto et al., 2007; McIntosh and Wydick, 2009).

Moreover, and also consistent with our descriptive analysis and research hypothesis, the good weather state indicator variable has a positive and significant effect on repayment behavior. Farmers are almost 38 percentage points more likely to repay fully when the weather is good than when the weather is bad. This is a relatively important effect given that climate-

[7]The results from the non linear models are available from the authors upon request

induced weather shocks are increasingly affecting agricultural outcomes (Lipper et al., 2017). We find the strong effect of the weather shock to be consistent across communication and non communication villages, even though the magnitude of the effect appears smaller in communication villages (21 percent) compared to non communication villages (40 percent). The coefficient of the interaction term between the weather and the communication variables in panel 1 indicates that the weather effect is reduced in presence of a mechanism such as communication exchange amongst credit providers, which reinforces dynamic incentives as a mitigating mechanism. The coefficient on the said interaction term is significant and negative, indicating that the effect of a good weather state on full repayment decision is 16 percentage points lower in communication villages compared to non communication villages.

The regressions in Table 6 use, as a dependent variable, the full 3-category repayment decision, taking values 0, 0.5, or 1 depending of the share of loan repaid by the farmers. Overall the results do not appear fundamentally different. The signs and significance levels of all the coefficients are maintained, even though some expected differences in magnitude of the coefficients are noticeable.

Table 5: Regression results: paying fully binary variable

	(1)	(2)	(3)
	Overall	Communication Villages	Non Communication Villages
Communication villages	0.162***		
	[0.007]		
Good weather	0.377***	0.213***	0.398***
	[0.000]	[0.002]	[0.000]
Communication villages X Good weather	-0.164+		
	[0.108]		
Constant	0.218**	0.507***	0.0454
	[0.050]	[0.003]	[0.728]
Observations	892	466	426
R^2	0.105	0.106	0.179
Adjusted R^2	0.092	0.084	0.159
Round fixed-effects	Yes	Yes	Yes
Village fixed-effects	No	No	No
Interaction term included	Yes	No	No
Sub-group analysis	Overall	Communication Villages	Non Communication Villages

Notes: P value in bracket. $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Regression results: 3-category repayment decision

	(1)	(2)	(3)
	Overall	Communication Villages	Non Communication Villages
Communication villages	0.0931**		
	[0.049]		
Good weather	0.259***	0.126**	0.275***
	[0.000]	[0.018]	[0.000]
Communication villages X Good weather	-0.128*		
	[0.095]		
Constant	0.550***	0.720***	0.447***
	[0.000]	[0.000]	[0.000]
Observations	892	466	426
R^2	0.099	0.095	0.168
Adjusted R^2	0.086	0.073	0.148
Round fixed-effects	Yes	Yes	Yes
Village fixed-effects	No	No	No
Interaction term included	Yes	No	No
Sub-group analysis	Overall	Communication Villages	Non Communication Villages

Notes: P value in bracket. $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.3. Robustness Checks

4.3.1. Redefining default

The main results presented in table 5 focus on the probability of repaying fully. One may wonder if the same conclusions hold for the decision to make at least a partial repayment versus completely defaulting. We implement this test using the same regression model as in equation 6, but using a dependent variable taking value 1 in case of complete default, and 0 otherwise. The results presented in table 7 remain qualitatively consistent with the findings decribed in section 4.2 above, though generally weaker. The good weather state indicator reduces the probability of complete default by 15 percentage points, significant at 1 percent, in the non communication villages, but does not seem to have any effect in the communication village.

Table 7: Regression results: paying nothing binary variable

	(1)	(2)	(3)
	Overall	Communication Villages	Non Communication Villages
Communication villages	-0.0240		
	[0.600]		
Good weather	-0.141***	-0.0382	-0.152***
	[0.002]	[0.429]	[0.001]
Communication villages X Good weather	0.0927		
	[0.175]		
Constant	0.118***	0.0662+	0.151***
	[0.008]	[0.123]	[0.003]
Observations	892	466	426
R^2	0.043	0.046	0.057
Adjusted R^2	0.029	0.023	0.034
Round fixed-effects	Yes	Yes	Yes
Village fixed-effects	No	No	No
Interaction term included	Yes	No	No
Sub-group analysis	Overall	Communication Villages	Non Communication Villages

Notes: P value in bracket. $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.3.2. Randomized Inference Estimates

Regression approaches for estimating treatment effects after randomized experiments are fairly common in the development literature. However, recent developments in the econometrics of randomized experiment seem to caution against the use of regression approaches to analyze data from randomized experiments. Athey and Imbens (2017), in their chapter in the Handbook of Economic Field Experiments claim that "*it is easy for the researcher using regression methods to go beyond analyses that are justified by randomization, and end up with analyses that rely on a difficult-to-assess mix of randomization assumptions, modeling assumptions, and large sample approximations. This is particularly true once one uses nonlinear methods*". They rather recommend randomization-based inference approaches, including Fisher and Neyman methods for calculating p-values. Almost a decade earlier, Imbens and Wooldridge (2009) had suggested that such methods "*deserve wider usage in social sciences*".

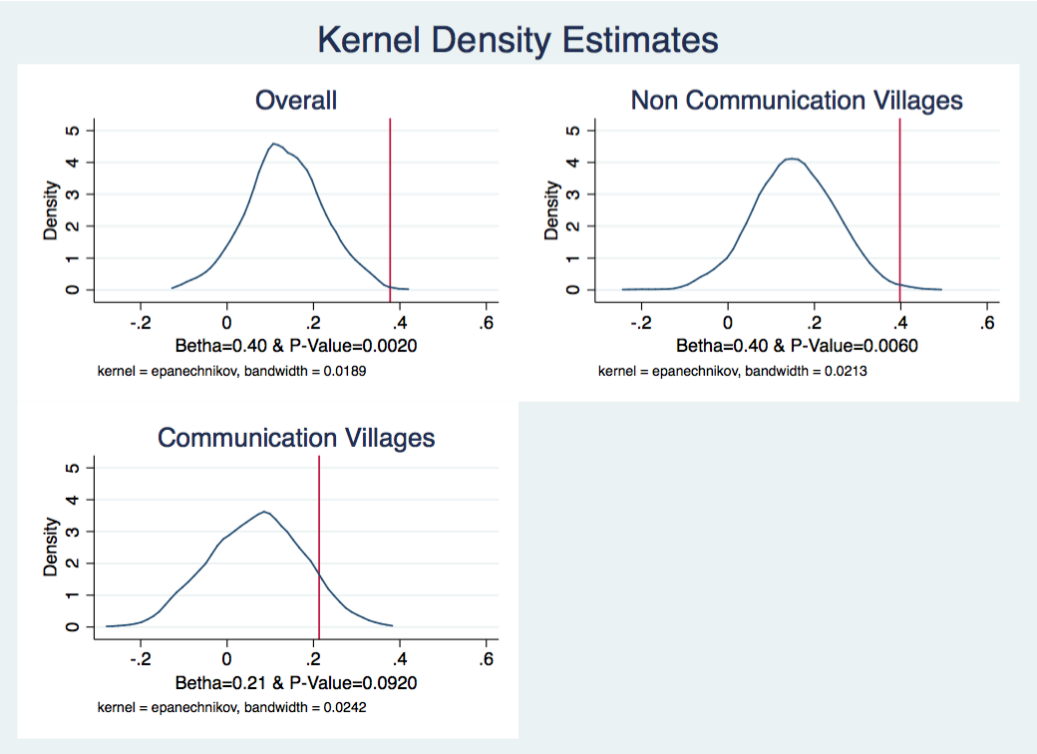
We apply Neyman's approach to our inference problem and test the effect of weather shocks on credit repayment decisions in the context of our lab-in-the-field experiment. The Neyman's estimator is simply the difference in average outcomes between treatment and control groups. The inference approach consists of deriving the distribution of the treatment effect under repeated draws of treatment assignment from the sets of all possible assignment vectors, keeping the observed outcomes fixed. The exact p-value is the proportion of draws for which the observed (fake) treatment effect is as large as the estimated effect (Athey and Imbens, 2017).

We implemented this approach to test the significance of the effects of weather shocks on repayment decisions overall, and separately in the communication villages and then in the non communication villages. We do this considering both definitions of default discussed above. All these results are summarized in Figures 3 and 4.

Overall the randomized inference results are qualitatively consistent with the results from the regression model estimations. As indicated in figure 3, the overall effects of good weather is positive and largely significant on the probability of repaying fully. This is consistent in both communication and non communication villages, though the effects are a lot stronger in non communication villages. As for the probability of complete default, the randomized inference results show significantly weaker effects of weather shocks compared to the regression analysis. Even in the non communication villages, we fail to reject the null hypothesis that the good weather indicator has no effect on the outcome variable. However, the sign of the estimated coefficient is as expected.

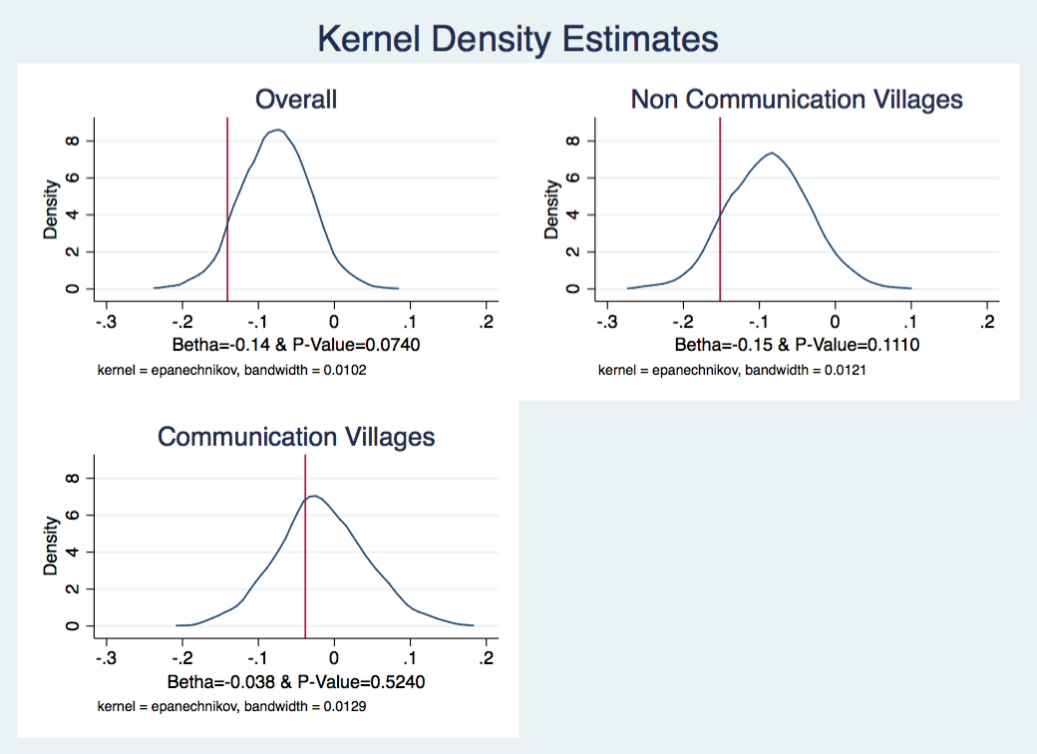
The fact that the weather shock treatment appears to significantly affect the decision to repay fully, but has only a weak effect on the decision to default completely only confims how important the potential of increased weather shocks from climate change may be for agricultural credit markets. They affect repayment decisions mostly at the top of the repayment distribution. Bad weather shocks would significantly reduce the "good behav-

Figure 3: Effect of good weather on full repayment decision: Randomized Inference Results



Notes: This graph shows the randomized inference results Overall [top-left], and by communication treatment [top-right] and [bottom-left]. The dependent variable is the binary indicator of full repayment. The standard error is estimated from 1000 replications, clustering at the round level in each village.

Figure 4: Effect of good weather on complete default decision: Randomized Inference Results



Notes: This graph shows the randomized inference results Overall [top-left], and by communication treatment [top-right] and [bottom-left]. The dependent variable is the binary indicator of complete default. The standard error is estimated from 1000 replications, clustering at the round level in each village.

ior" (full repayment) amongst borrowers likely to repay fully, while a good weather shock would not reduce the "bad behavior" as much. This suggests a behavioral argument implying that people are more responsive to negative shocks than to positive shocks. This findings aligns well with the S-shaped value function proposed in Kahneman and Tversky (1984) and Tversky and Kahneman (1991) to illustrate loss aversion, and which suggests that people's preferences are considerably steeper for losses than for gains, implying that "a loss of $X is more aversive than a gain of $X is attractive".

5. CONCLUSION

This paper poses the following question: can recurrent productivity shocks exacerbate failures in rural credit markets? We develop a stylized theoretical model that suggests a positive answer to this question, and the predictions of which, are also confirmed with data from a framed field experiment simulating a repeated input credit market. We find consistent evidence from descriptive, econometric, and non parametric analysis that productivity shocks such as weather-induced rainfall shocks can affect the repayment behavior of borrowers in rural credit markets, thereby increasing the risks of default and leading to further failures in markets. The findings are fairly robust to the existence of credit information exchange mechanisms (such as credit bureaus) increasingly promoted as a pathway towards broader financial inclusion. The implications of these findings are important as climate change and its negative effects are increasingly undermining agricultural production, especially in poor communities with low resilience. Specifically, our results suggest that climate change could also significantly undermine efforts to develop inclusive financial solutions such as credit markets for poor farmers. The findings offer additional arguments for the importance of insurance products tailored to the needs and conditions of the rural poor.

Though this experiment was conducted in a very specific context in Nigeria, the results suggest a relevant consideration for researchers and development practitioners focused on inclusive financial mechanisms to reach smallholder farmers. Further studies using larger and more representative samples, in various other contexts, to better understand the linkages between produc-

tivity shocks and rural credit markets, would be helpful to confirm these results.

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