The Role of Experience in Learning for Index Insurance Products: Evidence from Rural Kenya

This version: August 29, 2022.
Click here for the latest version.

Aranya Chakraborty*

Abstract

This paper investigates the role of experience in learning about index insurance products. I study the effect of payout and disaster experience in shaping the demand and knowledge for index insurance products. I develop a theoretical model where households learn about the covariate risk they face and the mapping of this covariate risk to the index insurance product that insures against it. The model predicts the impact of payout experience to depend on the households' optimism regarding the product design and a positive effect of disaster experience on the demand and knowledge for the product. I test these predictions using data from Index-Based Livestock Insurance (IBLI), Kenya. My results show that receiving a payout decreases demand at the extensive and intensive margin. I also find evidence that the effect of payout on the outcome variables can be explained by optimistic households updating their beliefs about the product design downward following a payout. Additional analysis suggests that policy interventions on their own may not be able to help overcome information frictions. Yet, these interventions can enhance households' learning from experience.

JEL Codes: D14, D81, D83, G52, Q18

Keywords: Index Insurance, Learning, Agriculture, Kenya.

1 Introduction

In recent decades, researchers and policy-makers advocated index-based insurance as a potential solution to the problem of missing insurance markets in subsistence agriculture (Carter et al., 2014). The idea is to condition the insurance payoff on some objectively observed index correlated with the individual-specific outcomes that individual actions cannot influence.[1] As

*Department of Economics, McGill University; Canada. E-mail: aranya.chakraborty@mail.mcgill.ca.
I am grateful to Sonia Laszlo for her continuous guidance and support throughout the project. I also thank Francesco Amodio, Matthieu Chemin, Markus Poschke, and Yongxu Yao for their helpful comments. I would also like to extend my gratitude to Munenobu Ikegami for helping me understand different aspects of the Index Based Livestock Insurance (IBLI) datasets. I am indebted to International Livestock Research Institute (ILRI) for access to the dataset used in this study. All views expressed in this work are my own.

[1] See Miranda and Farrin (2012) for some examples.

a result, index-based insurance sidesteps the asymmetric information problems of indemnity insurance schemes and helps provide affordable insurance to households living in high weather risk environments (Barnett et al., 2008). However, despite the low cost of such insurance and the purportedly high associated benefits, the take-up and renewal of these insurance policies remain surprisingly low (Platteau et al., 2017).[2] Existing literature identifies several possible reasons behind the low demand for index insurance products, including but not limited to product design (Clarke, 2016; Hill et al., 2016; Jensen et al., 2016; Janzen et al., 2020a), lack of financial knowledge (Patt et al., 2010; Cai et al., 2020), lack of trust in the insurer (Cole et al., 2013; Stern, 2019), and several behavioral factors (Elabed and Carter, 2015; Serfilippi et al., 2015; Belissa et al., 2020).[3] The literature also advocates learning from experience as a potential solution to the problem. However, the role of experience in learning for index insurance products has been under-explored in the literature.

This paper studies how experience shapes learning for index insurance products. In particular, I study the effect of payout and disaster experience in shaping the demand and knowledge for an index insurance product in rural Kenya. The objective is not only to understand the impact but also the mechanism of learning. In addition, I explore policy directions on using interventions to improve learning from experience.

I develop a theoretical model that formalizes the scenario where households are learning about the covariate risk they face, as well as the mapping of this covariate risk to the index insurance product that insures against it. I argue that experiencing disasters helps households learn about the covariate risk they face without affecting their perception of the product design. On the other hand, having a payout experience leads them only to update their beliefs regarding the product design. The product design is the design regarding the mapping of households' covariate risk to the index insurance product. The model predicts ambiguous impacts of receiving a payout on the demand and knowledge for the product. In particular, I find the effects to depend on the households' level of optimism regarding the product design. Additionally, the model predicts positive impacts of disaster experiences on demand and interest for the product, ceteris paribus.

To test the predictions of my theoretical model, I use data from Index-Based Livestock Insurance (IBLI), Kenya. I use a differences-in-differences identification strategy to identify the effects of receiving a payout. I also attempt to understand the mechanism behind such effects using a triple-differences specification. For additional empirical analysis, I exploit the randomized interventions in the data to understand policy directions for enhancing learning from

[2]We know these insurance schemes to be helping vulnerable rural population out of poverty trap (Janzen et al., 2020b; Noritomo and Takahashi, 2020), improving ex-ante risk-management decisions (Karlan et al., 2014; Elabed and Carter, 2014; Cai et al., 2015a; Cole et al., 2017; Gebrekidan et al., 2019; Matsuda et al., 2019), as well as ex-post risk-coping strategies (Bertram-Huemmer and Kraehnert, 2017; Janzen and Carter, 2018; Hill et al., 2019). Additionally, according to Jensen et al. (2017), index-based insurance is more cost-effective than direct cash transfers.

[3]Product design, in particular basis risk, has been argued to be the main reason behind the low take-up of index insurance products. Basis risk represents the difference between the realized individual loss and the loss predicted by the objectively observed index that determines the payouts.

experience.

Empirical analysis shows that receiving a payout decreases the extensive and intensive margin of demand for the group that received the payout, with the same group performing worse in answering knowledge questions regarding the product after the payout. I find that the effect of payout on the outcome variables, at least in part, can be explained by optimistic households updating their beliefs about the product design downwards following a payout. In terms of policy interventions, my analysis suggests that receiving a discount intervention mechanically increases demand (due to lower cost). However, it also increases households' chances of receiving a payout, leading them to optimally lower their demand. Similarly, while receiving a knowledge intervention mechanically increase households' knowledge regarding how the product works, it also helps them update their interest in the product downwards once a payout is received.

My study makes three contributions to the existing literature. First, I identify the causal effect of payout experience on the demand and knowledge for an index insurance product, along with the mechanism of such effect. The existing literature recognizes the role played by payout experience in shaping the demand for index insurance products. There is evidence both in favor of payouts increasing demand (Karlan et al., 2014; Stein, 2016), as well as decreasing demand (Timu et al., 2018). Payouts also increase demand for others in the social network (Karlan et al., 2014; Cai et al., 2020). In this literature, I contribute by being the first to explore the causal mechanism through which payout experience directly affects the demand and knowledge for index insurance products. In particular, I focus on the role played by households' perceptions regarding product design. Additionally, this study is also among the first that uses a differences-in-differences identification strategy to identify the causal effect of payout experience.

Second, I provide a theoretical framework that formalizes learning from experience for index insurance products and rationalizes my empirical findings. In their seminal papers, Besley and Case (1993; 1994) and Foster and Rosenzweig (1995) argue in favor of *learning by doing and learning from others* about optimal input use in agricultural technology adoption. These early studies argue in favor of learning by doing from experience. Their argument for expecting a learning-by-doing effect applies to index insurance products since insurance is an experience good. In addition, the added complexity of an index insurance contract vis-a-vis traditional insurance schemes, together with the low financial literacy level of farmers in the developing and underdeveloped regions of the world, makes the case in favor of a learning-by-doing effect even stronger. This is already recognized in the existing literature, that focuses on demonstrating learning-by-doing (Cole et al., 2014; Takahashi et al., 2020), or learning from others (Giné et al., 2013; Dercon et al., 2014; Cai et al., 2015b; Takahashi et al., 2020), or both (Santeramo, 2018; Cai et al., 2020). However, in the existing literature, less attention has been paid to understanding the mechanism of such learning. The current study aspires to address that through the channel of households' subjective perceptions and expectations.

Finally, I provide policy directions for using interventions to improve learning from experience for index insurance products. The role of discount and knowledge interventions in improving demand for index insurance products is well recognized.[4] Discount interventions are heavily used to increase initial adoption. The demand for index insurance products is highly price-sensitive (Jensen and Barrett, 2016). Knowledge interventions are supplementary tools for overcoming information frictions (Carter et al., 2014). However, in the existing literature, relatively less attention is given to providing policy directions on how to use these interventions in improving learning from experience. To the best of my knowledge, Cai et al. (2020) is the only other study exploring the role of interventions in channeling learning from payout experience. This study provides additional evidence in this regard.

The rest of this article is organized as follows. In Section 2, I present my theoretical framework and highlight the main hypotheses for this study. Section 3 discusses the data and presents descriptive statistics. Section 4 discusses identification strategies for my empirical analysis and presents associated results. Finally, in Section 5, I summarize the findings and make concluding remarks.

2 Theoretical Framework

In this section, I first present a theoretical model of index insurance following Janzen et al. (2020b). After introducing their framework, I discuss relaxing some simplifying assumptions of the model to add the possibility of learning.

2.1 Index Insurance without Learning

Consider household i from index-area j to have a asset holding A_{ijt} at period t. The household decides between how much to consume at this period (c_{ijt}) and how much to save as assets for the next period (A_{ijt+1}). The household is credit constrained such that $c_{ijt} \leq A_{ijt} + f(A_{ijt})$ with $A_{ijt+1} \geq 0$, where $f(\cdot)$ is a fixed production function that does not change over time.[5] The household face two types of shocks: a covariate shock θ_{jt} that is common to all other households living in the same index-area as them, and an idiosyncratic shock ϵ_{ijt} that is household-specific. In terms of the dataset used here, $1 \geq \theta_{jt} \geq 0$ can be interpreted as being the actual area-average livestock mortality, with $1 \geq \epsilon_{ijt} \geq 0$ being the individual level deviation from it. Consequently, $\mu_{ijt} := (\theta_{jt} + \epsilon_{ijt}) \in [0, 1]$ denotes the livestock mortality at the household level.

So, at any period t, the household first chooses their consumption (c_{ijt}). After that they realize the composite shock $\mu_{ijt+1} := (\theta_{jt+1} + \epsilon_{ijt+1})$, which determines their next period's asset holding $A_{ijt+1} = (A_{ijt} + f(A_{ijt}) - c_{ijt})(1 - \mu_{ijt+1})$.[6] Thus, the household's optimization

[4]Examples can be found in Giné et al. (2013), Takahashi et al. (2016), Ahmed et al. (2020), and Cai et al. (2020)

[5]In Janzen et al. (2020b), $f(\cdot)$ can be either a high or low return technology as their model focuses on the role of index insurance in escaping poverty trap. Here, simplification has been made for my purpose.

[6]Here, similar to Janzen et al. (2020b), I assume that the households can only observe negative shocks. This is because the main reason for purchasing an index-insurance product is to insure against adverse shocks. Thus, the

problem is:

$$max_{c_{ijt}} E_{\mu} \sum_{t=0}^{\infty} \beta^t u(c_{ijt}) \tag{1}$$
$$subject\ to:$$
$$c_{ijt} \leq A_{ijt} + f(A_{ijt})$$
$$A_{ijt+1} = (A_{ijt} + f(A_{ijt}) - c_{ijt})(1 - \mu_{ijt+1})$$
$$c_{ijt}, A_{ijt+1} \geq 0$$

where $u(\cdot)$ represents the household's period-specific utility function and β the discount factor.

Now, suppose that there exists an index insurance product that insures the household against the covariate shock θ_{jt} but not the idiosyncratic shock ϵ_{ijt}. The index insurance product makes the payout based on some objectively observed index $i(\theta_{jt})$ that represents the covariate shock. Payout $\delta(\theta_{jt})$ is positive if and only if $i(\theta_{jt})$ is higher than some strike point $s \geq 0$, i.e. $\delta(\theta_{jt}) = \max\{i(\theta_{jt}) - s, 0\}$.

With the index insurance product available, the household now decides how much to consume (c_{ijt}) and how much to insure (I_{ijt}) at each period t. The per unit price of the index insurance product is p. For their purpose Janzen et al. (2020b) assume $i(\theta_{jt}) = \theta_{jt}$ and that it is a common knowledge. This assumption has three implications. First, the index perfectly observes the covariate risk without any error. In terms of the terminology used in Elabed et al. (2013), this means that there is no *design risk* associated with the product.[7] Second, the consumers also believe the index represents the covariate risk perfectly. More specifically, there is no deviation between the objective value of $i(\theta_{jt})$ and its subjective perception for the consumer. Finally, the basis risk associated with the product, for both the insurer and insurees, is represented by the household-specific idiosyncratic risk ϵ_{ijt}. Under these assumptions, the household has perfect information regarding the basis risk associated with the product and makes their decisions accordingly. The household's optimization problem becomes:

$$max_{c_{ijt}, 0 \leq I_{ijt} \leq A_{ijt}} E_{\theta,\epsilon} \sum_{t=0}^{\infty} \beta^t u(c_{ijt}) \tag{2}$$
$$subject\ to:$$
$$c_{ijt} + pI_{ijt} \leq A_{ijt} + f(A_{ijt})$$
$$A_{ijt+1} = (A_{ijt} + f(A_{ijt}) - c_{ijt} - pI_{ijt})(1 - \mu_{ijt+1}) + \delta_{jt+1} I_{ijt}$$
$$\delta_{jt+1} = \delta(\theta_{jt+1}) = \max\{(i(\theta_{jt+1}) - s), 0\}$$
$$i(\theta_{jt+1}) = \theta_{jt+1}$$
$$c_{ijt}, A_{ijt+1} \geq 0$$

possibility of a positive shock is not so important from the perspective of a household if the household is risk-averse. However, such a possibility can be important for the insurer, which is beyond the scope of this paper.

[7] *Design risk* is the prediction error of the index in capturing the covariate risk.

In the following subsection, I relax the assumption that the index perfectly observes the covariate risk. In doing so, I introduce the possibility of design risk in the product. Additionally, I consider the scenario where the households are not fully informed about the correlation between the index and the covariate risk, thus the need for learning on their behalf. As a result, the households need to make decisions based on their beliefs regarding the correlation and can potentially learn about it over time. Thus, the subjective perception of the basis risk will be different for the households than its objective counterpart, absent complete learning.

2.2 Index Insurance with Learning

Consider the index to be represented by $\iota(\theta_{jt})$ instead of $i(\theta_{jt})$, where $\iota(\theta_{jt}) = \gamma^*\theta_{jt} + \nu_{jt}$, relaxing the assumption that the index perfectly observes the covariate risk. The parameter $\gamma^* \in [0,1]$ helps in mapping households' covariate risk θ_{jt} to the index $\iota(\theta_{jt})$, with ν_{jt} being the zero mean random error in mapping. The insurer does not observe θ_{jt}, so makes the payout contingent on $\iota(\theta_{jt})$. Similar to the last sub-section, for $I_{ijt} > 0$ per-unit return δ'_{jt} depends on the index $\iota(\theta_{jt})$ following the non-linear function:

$$\delta'_{jt} = \delta'(\theta_{jt}) = \begin{cases} \iota(\theta_{jt}) - s & if\ \iota(\theta_{jt}) \geq s \\ 0 & otherwise, \end{cases} \tag{3}$$

where s is the pre-determined strike point, which is common knowledge to everyone.

Under this modified scenario, the household's problem becomes:

$$\begin{aligned} & max_{c_{ijt}, 0 \leq I_{ijt} \leq A_{ijt}} E_{\theta,\epsilon} \sum_{t=0}^{\infty} \beta^t u(c_{ijt}) \\ & subject\ to: \\ & c_{ijt} + pI_{ijt} \leq A_{ijt} + f(A_{ijt}) \\ & A_{ijt+1} = (A_{ijt} + f(A_{ijt}) - c_{ijt} - pI_{ijt})(1 - \mu_{ijt+1}) + \delta'_{ijt+1} I_{ijt} \\ & \delta'_{ijt+1} = \delta'_{ijt}(\theta_{jt+1}) = \max\{(\iota_{ijt}(\theta_{jt+1}) - s), 0\} \\ & \iota_{ijt}(\theta_{jt+1}) = \gamma_{ijt}\theta_{jt+1} \\ & c_{ijt}, A_{ijt+1} \geq 0, \end{aligned} \tag{4}$$

where $\gamma_{ijt} \in [0,1]$ denote the household's belief for γ^* at period t. Similarly, $\iota_{ijt}(\cdot)$ is the subjective counterpart of $\iota(\cdot)$ and $\delta'_{ijt}(\cdot)$ is the subjective counterpart of $\delta'(\cdot)$.

It is worth noting that, under this scenario, households are able to observe the shocks perfectly. However, the error term ν_{jt} in $\iota(\theta_{jt})$ stops them from learning the true value of γ^* right away. This is similar to the learning-by-doing models of Foster and Rosenzweig (1995) and Jovanovic and Nyarko (1996). In what follows, I argue that the household's belief about γ^* and their expectation on θ_{jt+1} determine their demand for period $(t+1)$ through two different channels.

2.2.1 Payout Experience

Let me first consider the consequences of receiving (or not receiving) payouts. The objective probability of receiving a payout in period $t+1$ upon purchasing the product is:

$$\begin{aligned} Prob(\iota(\theta_{jt+1}) \geq s) &= Prob(\gamma^* \theta_{jt+1} + \nu_{jt+1} \geq s) \\ &= Prob(\gamma^* \geq \frac{s}{\theta_{jt+1}} - \frac{\nu_{jt+1}}{\theta_{jt+1}}) \approx Prob(\gamma^* \geq \frac{s}{\theta_{jt+1}}). \end{aligned}$$

If $\gamma_{ijt} \neq \gamma^*$, not purchasing the product helps the household learn nothing new about it absent knowledge spillovers. Thus, they will not update their beliefs regarding γ^*. In what follows, I argue that even the households purchasing the product may not update their beliefs of γ^* if they do not receive any payouts. To see this, consider two possible scenarios where the subjective belief of γ^* (i.e., γ_{ijt}) can differ from its objective counterpart:[8]

Case 1: $\gamma_{ijt} < \gamma^*$. Since γ^* is under-estimated, the demand will be lower than optimal. In such a scenario, not receiving a payout helps the household learn nothing new about γ^*. This is because if $\gamma^* < \frac{s}{\theta_{jt+1}}$, then $\gamma_{ijt} < \frac{s}{\theta_{jt+1}}$ and the household learn nothing new about the product. As a consequence, the demand should remain lower than optimal. However, if the household receives a payout, they observe the per-unit return of $(\gamma^* \theta_{jt+1} + \nu_{jt+1} - s)$. As they have already observed the θ_{jt+1} and know s, this helps them update their beliefs for γ^* upwards. Which will help bring the demand closer to the optimal. However, as mentioned above, this needs to happen for a few more periods before the households can cancel out the noise ν and realize the true value of γ^*.

Case 2: $\gamma_{ijt} > \gamma^*$. Since γ^* is over-estimated, the demand will be higher than optimal. In such a scenario, not receiving a payout helps the household update their beliefs for γ^* downwards if $\gamma_{ijt} > \frac{s}{\theta_{jt+1}}$. Similarly, receiving a payout helps them update their beliefs for γ^* downwards, bringing the demand closer to the optimal for both scenarios.

Thus, receiving a payout improves the information set in both cases. However, for case 1 (i.e., $\gamma_{ijt} < \gamma^*$) it increases the demand, while for case 2 (i.e., $\gamma_{ijt} > \gamma^*$) it decreases the demand. Therefore the average effect of such payout experience on demand depends on the average belief of γ^* in a population. The household's knowledge regarding how the product works, weakly indicative of their interest in the product, should be affected similarly. Thus, the effects need to be understood empirically for a given population. The upper half of Figure 1 presents this channel. Here payout experience leads to improved information about the product (in terms of the model, this translates to improved knowledge about γ^*). The effect of this increase in information on the demand and knowledge for the product is ambiguous. The effect particularly depends on the proportions of the households over and under-estimating γ^* before the payout experience. This result leads to my first hypothesis:

[8]If $\gamma_{ijt} = \gamma^*$, the households have the perfect information regarding γ^*, and thus, do not need to learn.

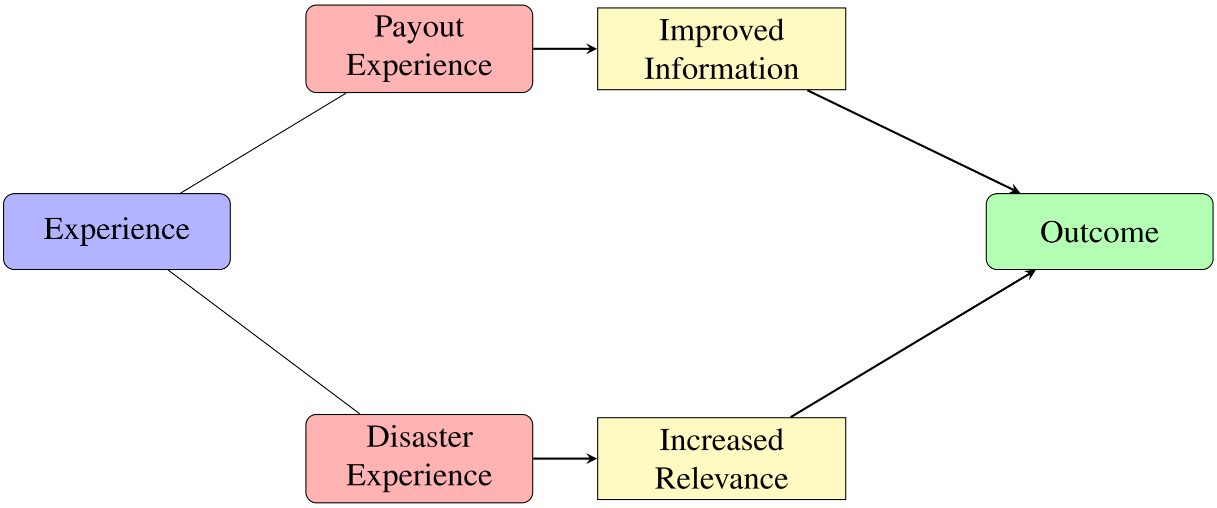

Figure 1: Effect of different experiences on final outcome

Hypothesis 1: *Ceteris paribus, receiving a payout improves the information regarding the index-insurance product. The effect of this on the demand and knowledge for the product is ambiguous:*

1. *If people are, on average, too optimistic about the product design (i.e., $\gamma_{ijt} > \gamma^*$), receiving a payout leads to a decrease in demand and knowledge for the product.*

2. *If the average population is too pessimistic about the product design (i.e., $\gamma_{ijt} < \gamma^*$), receiving a payout will lead to an increase in demand and knowledge for the product.*

2.2.2 Disaster Experience

Let me now concentrate on the consequences of experiencing a disaster. In particular, I consider the disaster that the index-insurance product insures against. In terms of the theory, this would mean observing high values of θ. As described earlier, households experience θ_{jt+1} only after purchasing insurance for the period $t+1$. Thus, they make the insurance decisions based on expectations regarding θ_{jt+1}. The realization of θ_{jt} matters for this purpose.

To understand this more concretely, consider the possibility that $\theta_{jt} \in \{\theta^H, \theta^L\}$. Here θ^H denotes a high value of covariate shock, and θ^L denotes a low value of covariate shock. Additionally, assume household i's subjective belief of θ_{jt+1} to be a Markov 1 process $\pi_{ijt}(\theta_{jt+1}|\theta_{jt})$. Then, I expect $\pi_{ijt}(\theta_{jt+1} = \theta^H|\theta_{jt} = \theta^H) > \pi_{ijt}(\theta_{jt+1} = \theta^H|\theta_{jt} = \theta^L)$. Similarly, $\pi_{ijt}(\theta_{jt+1} = \theta^L|\theta_{jt} = \theta^L) > \pi_{ijt}(\theta_{jt+1} = \theta^L|\theta_{jt} = \theta^H)$. In other words, the household i from index-area j believes that θ_{jt+1} is more likely to be θ^H if in the last period θ_{jt} was θ^H. On the other hand, the same household believes $\theta_{jt+1} = \theta^L$ to be more likely, if in the last period θ_{jt} was θ^L. The belief would impact the choice of I_{ijt} in the optimization (4) by affecting the calculation of the expected utility.

More specifically, high θ_{jt} will make the households perceive high θ_{jt+1} to be more likely, increasing their demand and interest for the insurance product. Similarly, low θ_{jt} will make the households perceive low θ_{jt+1} to be more likely, decreasing their demand and interest for the product. This result is in line with the empirical findings of Cai and Song (2017), Bjerge and Trifkovic (2018), and Mogge and Kraehnert (2022). It is important to note that this assumes household asset levels to remain the same over periods t and $t+1$. If the households lose their assets due to high θ_{jt}, their demand for the product in period t (i.e., I_{ijt}) will be mechanically lower as there are fewer assets to insure. Thus, empirical analysis needs to control for this possibility. The lower half of Figure 1 presents this channel. Here disaster experience leads to increased relevance for the product, which leads to higher demand and knowledge for the product (ceteris paribus, through the expectation of θ in the model). This result leads to my second hypothesis:

Hypothesis 2: *Ceteris paribus, experiencing a disaster in the last period increases the demand and knowledge for the product in this period.*

For the empirical analysis, I focus on testing Hypothesis 1. I do not test Hypothesis 2 in this paper. There is already a body of evidence supporting Hypothesis 2. Thus, one can interpret my theoretical framework as providing the rationale behind existing empirical findings. Additionally, I focus on assessing the impact of exogenous interventions in enhancing the influence of both payout and disaster experience.

3 Data and Descriptives

The objective of this section is to describe the dataset I use in this study. For that purpose, I start by providing the background for Index-Based Livestock Insurance and subsequently move to the discussion of the survey and interventions associated with the data collection. The final subsection focuses on describing baseline characteristics and trends in the data.

3.1 Background

The pilot phase of Index-Based Livestock Insurance (IBLI) started in the Marsabit District of Northern Kenya in 2010, subsequently extending to the Borena Zone of Southern Ethiopia in 2012.[9] IBLI uses Normalized Differenced Vegetation Index (NDVI) as the objectively observable measure of the greenness of a region to insure pastoralist households against drought-related livestock mortality.[10] I focus on the Kenyan pilot because the payouts were more widespread in the Kenyan pilot compared to its Ethiopian counterpart. Additionally, the Borena pilot makes ex gratia payments to complement the payouts, which makes things more complicated.

[9] Details regarding the project is in `https://ibli.ilri.org/index/`.

[10] Chantarat et al. (2012) discusses in detail the construction of the index insurance product.

In Kenya, the International Livestock Research Institute (ILRI), Cornell University, the BASIS Research Program at the University of California, Davis, and Syracuse University conducted the survey and implementation with their implementing partners Equity Bank, UAP Insurance Company, APA Insurance Company, and Takaful Insurance of Africa. The researchers divided the Marsabit district into five index regions for IBLI distribution.[11] The insurance was available to all households in these regions, who could self-select themselves into getting a contract. The district has a bi-modal rain pattern. Accordingly, the researchers designed the insurance product to be offered twice yearly before each rainy season, with each insurance contract being valid for a whole year. This design generated the possibility of overlapping payouts for some seasons. The intention was to reduce the credit and liquidity constraints of the households (Chantarat et al., 2012). Figure 2, which combines the information from Table 1 and Figure 1 from Ikegami and Sheahan (2014), demonstrates the bi-modal rain pattern observed in the region, IBLI sales periods, coverage periods, and the possibility of overlapping payout. In practice, however, the overlapping structure of contracts was not possible every year. As a result, some years had two sales periods as intended, while some had only one.

The NDVI was the primary input for calculating the area-average livestock mortality rate for each index region.[12] If the calculated area-average livestock mortality in an index region was higher than a certain threshold, payouts were made to all households covered by the insurance in that region.[13] The total payout to a household was contingent on the household-specific coverage bought and the index-area specific difference of the calculated livestock mortality from the threshold.

3.2 Survey and Interventions

Although IBLI was introduced to all five index regions of Marsabit, the survey only covered four of them. The primary geographic region of the survey was "sub-locations". Each index area contained multiple sub-locations, with the 4 index regions surveyed containing a total of 16 sub-locations. From each of these 16 sub-locations, a sample size of around 11% was set to be drawn proportional to the 1999 Kenya Population and Housing Census. Then, a minimum size of 30 and a maximum of 100 households were set per sub-location to decide the final sample size.[14] This resulted in a final sample of 924 households.

Figure 2 outlines the timing of household survey rounds. The baseline survey took place in 2009, with annual follow-up rounds after the introduction of the product in 2010-2013 and a fifth follow-up round in 2016 (not shown in the figure). For this study, I focus on the first five rounds of the household survey (the baseline and the first four follow-up rounds). The reason

[11] Premium rates and NDVI readings vary at the index area level.

[12] Details regarding the calculation can be found in Chantarat et al. (2012), and Jensen et al. (2018).

[13] This threshold was 15% for the first five sales periods. After that, consumers opted between 10% and 15% threshold levels, with different associated premium rates.

[14] Details can be found in Ikegami and Sheahan (2014).

behind this is threefold. First, the exogenous discount intervention was effective until the 6th IBLI sales period, i.e., it got discontinued following household survey round 5. As the product is highly price-sensitive (as shown in the results), this led to a massive drop in the associated demand. If included, this can bias my results. Second, the reference period hugely differs for survey round six compared to the past survey rounds, which can be problematic for my analysis. Finally, in 2015 some ex gratia payments complemented the payouts, which can complicate identification for my purpose. The researchers originally intended to repeat the sample size of 924 households in each survey round to construct a panel of these households. However, they could not trace down some of them in later periods and thus, added replacement households. For this study, I focus on the balanced panel of 820 households successfully surveyed in all five rounds.[15]

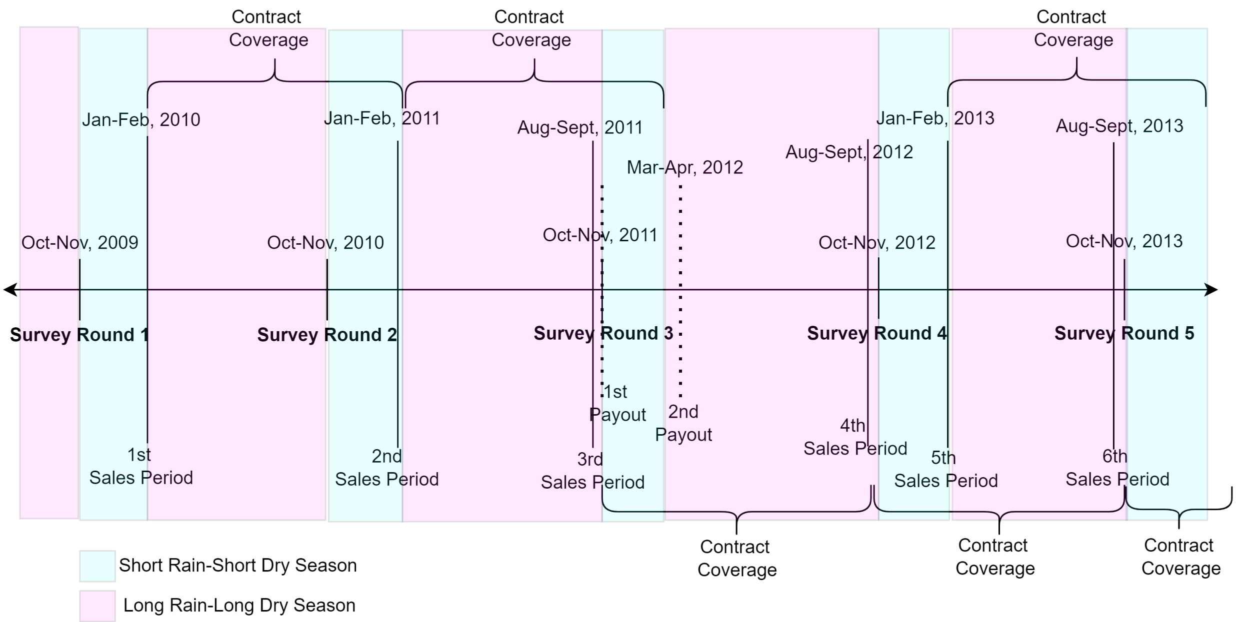

Figure 2: Timeline of IBLI Marsabit

The IBLI product was available to all households in the Marsabit District. However, the researchers distributed exogenous discount and knowledge instruments in the surveyed regions for impact evaluation purposes. The knowledge intervention was in the form of an IBLI knowledge game that was randomized and implemented only once before the first sales period. The discount interventions were in the form of discount coupons that the researchers randomly distributed independently in each sales period. These coupons were only valid for the sales period when they got distributed. Thus, for each sales period, we can identify the households as being part of one of the four different treatment groups:

1. **Control Group:** The households that were not given the one-time knowledge treatment and also did not receive the sales period-specific discount coupon.

[15]An analysis of related attrition is in Jensen et al. (2018).

2. **Only Discount:** The households that received the sales period-specific discount coupon but did not receive the one-time knowledge treatment.

3. **Only Knowledge:** The households that received the one-time knowledge treatment but did not receive the sales period-specific discount coupon.

4. **Discount and Knowledge:** The households that received the one-time knowledge treatment, as well as the sales period-specific discount coupon.

Table 1: Composition of Treatment Groups over Sales Periods

	Sales Period						
Treatment	1	2	3	4	5	6	Total
Control	249	236	221	230	222	115	1273
Only Discount	325	338	353	344	352	459	2171
Only Knowledge	83	101	95	87	98	46	510
Discount and Knowledge	163	145	151	159	148	200	966
Total	820	820	820	820	820	820	4,920

Notes: Control households in a sales period are the ones that have never received the knowledge treatment and do not receive that period-specific discount coupon. *Only Discount* households in a sales period are the ones that receive that period-specific discount coupon, but have never received the knowledge treatment. *Only Knowledge* households in a sales period are the ones that received the knowledge treatment, but do not receive that period-specific discount coupon. *Discount and Knowledge* households in a sales period are the ones that received the knowledge treatment, as well as that period-specific discount coupon.

Table 1 describes the composition of these treatment groups across six sales periods for the balanced panel of 820 households. The indemnity payouts, as shown in Figure 2, occurred two times during the first five survey rounds. These were in October-November, 2011, and March-April, 2012.

3.3 Descriptive Statistics

Table 2 describes the baseline summary statistics by different treatment groups.[16] The survey collected information on households' insurance purchase decisions, including their decision to buy the insurance and the type and number of animals insured. The first row of table 2 uses the dummy variable that records households' baseline binary decision to buy an insurance policy. At the baseline, around 28% purchased the product on average, with some variations across treatment groups. Unsurprisingly, the baseline demand was at its lowest (around 10% on average) for the control group compared to other treatment groups. On the contrary, the

[16]Appendix C of Jensen et al. (2018) contains balance checks for randomly assigned treatments.

baseline demand was at its highest (around 47% on average) for the group that received both knowledge and sales period-specific discount treatments. Also, the households that received only period-specific discount coupons purchased the product more than those that received one-time knowledge treatment (35% vs. 19%). Surprisingly, the baseline knowledge regarding the index insurance product seems more uniform across treatment groups. This variable reflects households' knowledge regarding how the insurance product works, and they are calculated based on households' answers to the knowledge questions asked in each survey rounds.[17] The average knowledge score seems to be around 47%.

The following three rows indicate that the average household in the survey has a head aged around 48 years, who is 62% likely to be male, and completed around one year of education. The baseline asset index for the households was around 19%.[18] The baseline survey collected information on the risk preferences using a Binswanger (1980) type of incentivized game. Using that information, I have calculated risk aversion dummies shown next (with risk-neutral being the omitted category). On average, around 27% households are extremely risk-averse, and 44% are moderate risk-averse, with some variations across treatment groups. The household-level income, reported in the next row, seems to vary around 10,000 Kenyan Shilling (KSH) per season. Around 75% households report livestock as their main income source and own around 30 Tropical Livestock Units (TLUs) on average.[19] Finally, around 90% households reported drought being the most critical disaster for them. These highlight the importance of a livestock insurance product for the population, particularly one that focuses on drought-related livestock mortality. Thus, the IBLI should be a product in high demand for this region.

Figure 3 presents the demand for the product across treatment arms over the first six sales periods. As can be seen, demand was at its highest in the baseline with a steady decline over six sales periods. All treatment groups seemed to converge to an average demand of below 10% over time. Finally, its worth noting that the control group remained relatively stagnant in terms of their demand over sales periods, while the treatment group receiving both discount and knowledge treatment drastically purchased less over time. However, we should keep in mind the overlapping structure of the contracts while interpreting this figure. Since the households that purchased the product in the second and fifth sales periods had insurance coverage for the third and sixth sales periods, they were less likely to buy the product. Similarly, Figure 4 presents the average knowledge for the product across treatment arms over the first six sales periods. Like the demand, knowledge was at its highest in the baseline but remained relatively more stagnant over time. It is worth noting that the knowledge scores are calculated based on the knowledge questions asked in each survey rounds. As a result, the knowledge scores do not vary across sales periods for the sales periods that are part of the same survey round. The distribution of sales-period-specific discount coupons is the sole factor driving the variation in

[17] Details regarding the knowledge questions are in Appendix B.

[18] Details regarding the calculation are in the footnote of Table. 2.

[19] As mentioned by Ikegami and Sheahan (2014): "*1 TLU is equivalent to 1 cow, 0.7 camel, 10 goat, or 10 sheep/goats (also referred to as "shoats").*"

Table 2: Baseline Summary Statistics by Different Treatment Groups

Variable	Control	Only Discount	Only Knowledge	Discount & Knowledge	Total
Demand for Index Insurance	0.096	0.351	0.193	0.472	0.282
	(0.296)	(0.478)	(0.397)	(0.501)	(0.450)
Knowledge of Index Insurance†	0.429	0.451	0.484	0.531	0.467
	(0.326)	(0.327)	(0.364)	(0.306)	(0.328)
Age of HH Head*	47.250	47.862	46.831	49.632	47.924
	(18.008)	(18.316)	(18.592)	(19.747)	(18.534)
Gender of HH Head (Female=1)	0.365	0.369	0.398	0.393	0.376
	(0.483)	(0.483)	(0.492)	(0.490)	(0.485)
Education of HH Head**	1.233	0.942	1.012	0.914	1.032
	(3.415)	(2.775)	(2.887)	(2.511)	(2.947)
Assets Index	0.215	0.193	0.191	0.161	0.193
	(0.204)	(0.190)	(.190)	(0.166)	(0.191)
Extreme Risk Averse	0.229	0.255	0.253	0.368	0.270
	(0.421)	(0.437)	(0.437)	(0.484)	(0.444)
Moderate Risk Averse	0.438	0.486	0.482	0.344	0.443
	(0.497)	(0.501)	(0.503)	(0.476)	(0.497)
Income (1000 KSH)‡	10.408	11.768	9.074	8.433	10.419
	(19.137)	(26.307)	(16.029)	(17.529)	(21.746)
Main Income Source (Livestock=1)	0.763	0.738	0.723	0.767	0.750
	(0.426)	(0.440)	(0.450)	(0.424)	(0.433)
Total TLUs***	30.346	29.753	27.635	27.267	29.229
	(30.557)	(32.704)	(29.946)	(30.530)	(31.329)
Most Critical Disaster (Drought=1)	0.944	0.917	0.819	0.902	0.912
	(0.231)	(0.276)	(0.387)	(0.298)	(0.283)
Observations	249	325	83	163	820

Notes: †Available for 654 households: 164 control, 274 only discount, 71 only knowledge, and 145 discount & knowledge. ‡Available for 789 households: 239 control, 313 only discount, 80 only knowledge, and 157 discount & knowledge. *Available for 248 control households. **Available for 82 households having only knowledge treatment, 162 households having discount and knowledge treatment. *** Available for 324 households having only discount treatment, 161 households having discount and knowledge treatment. The variables *Demand for IBLI*, *Knowledge of IBLI*, *Income*, and *Total TLUs* use information collected in survey round 2. *Income* captures households' income in the season prior to the first sales period. *Total TLUs* capture total tropical livestock unit (TLU) herded by the household in the year prior to the first sales period. All other information are collected in the baseline survey (survey round 1). *Age*, *Gender*, and *Education* of HH head captures the age of household head, the gender of household head (female=1) and the years of education for household head. *Assets Index* is the average of 6 dummy variables: material for the walls of main dwelling (1-Brick/Block/Cement, 0-otherwise), main flooring material (1-Cement/Tiles, 0-otherwise), whether the household has toilet facility (1-Yes, 0-No), whether they own any land, any donkey, or any poultry (1-Yes, 0-No). *Risk Aversion* dummies are calculated using the classification of Binswanger (1980). Here the omitted category is *Risk Neutral*. *Main Income Source* is a dummy that captures whether the main income source is related to livestock 5 years prior to the survey round 1. *Most Critical Disaster* is a dummy that captures whether drought is ranked first by the household as critical reason for their major livestock loss.

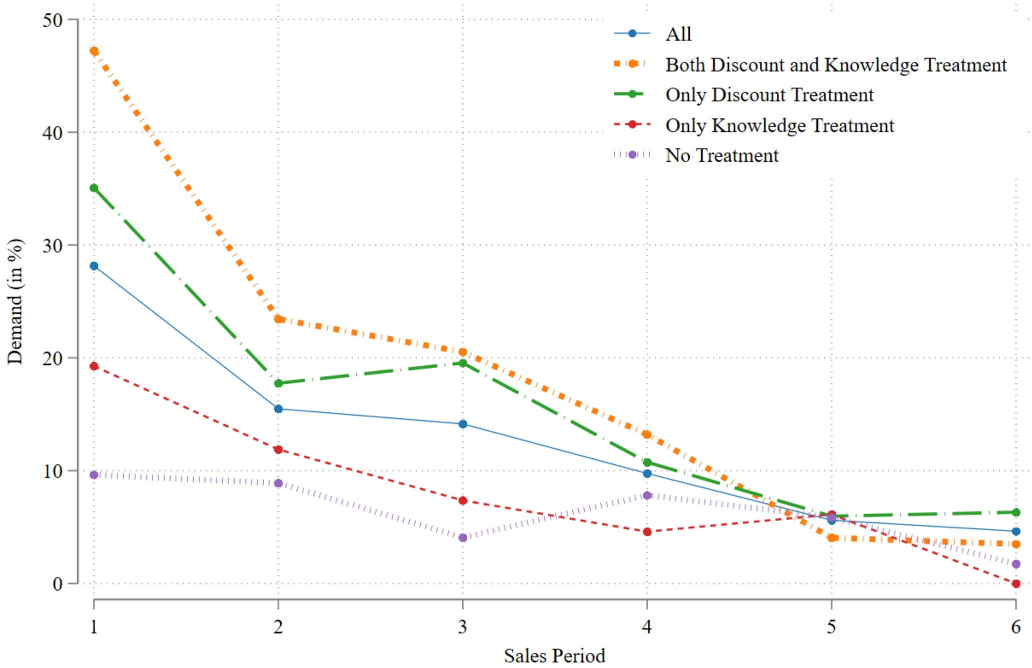

Figure 3: Demand Over Sales Periods by Treatment Groups

average knowledge scores by treatment groups for these sales periods. These figure also fail to explicitly capture the change in the composition of treatment groups across six sales periods as demonstrated by Figure 1. The empirical analysis need to control for these factors.

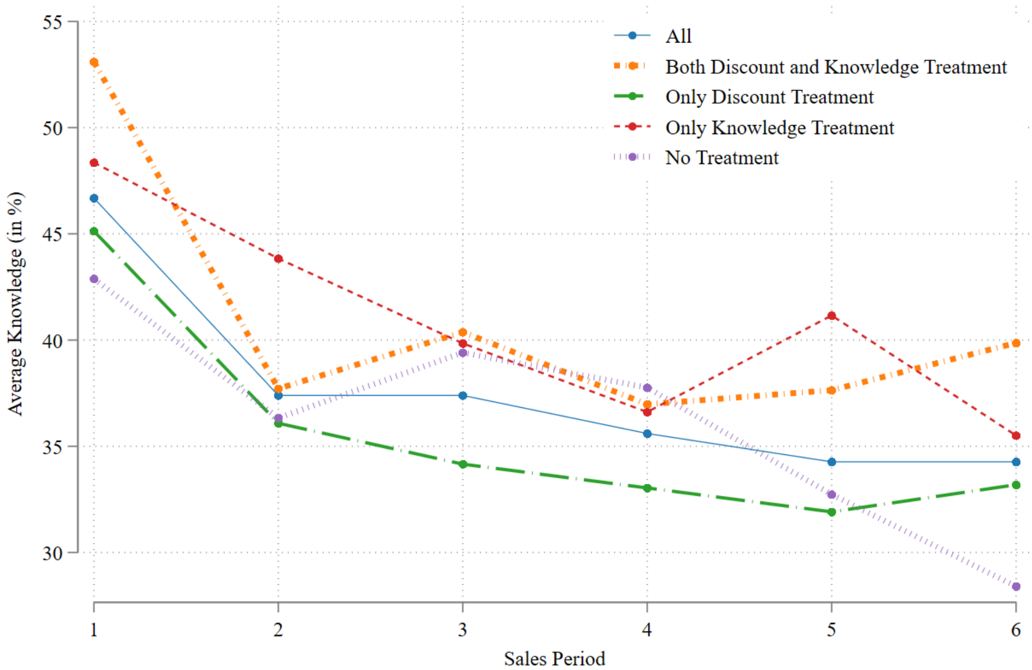

Figure 4: Performance in Knowledge Questions Over Sales Periods by Treatment Groups

4 Empirical Analysis

In this section, I start by analyzing the effect of payout experience on the households' decision to purchase the product and their performances in the knowledge questions. In doing so, I discuss my identification strategy and present the associated results. Subsequently, I focus on understanding the mechanism driving the effect of the payout experience. The final subsection focuses on assessing the impact of exogenous interventions in enhancing the influence of experience on the demand and knowledge for the product.

4.1 Identifying Effects of Payout Experience

I use a differences-in-differences estimation strategy for identifying the effect of payout experience on the demand and knowledge for the product.[20] The first subsection of this section focuses on discussing the estimation strategy in detail and presents the associated results. The last subsection of this section focuses on identifying the mechanisms behind the effects. In doing so, I discuss the associated triple-difference strategy and results.

4.1.1 Differences-in-Differences Strategy and Estimates

I use a differences-in-differences estimation strategy to identify the effect of payout experience on the demand and knowledge for the index insurance product. For this purpose, I use the differences before and after the payout between the group that received the payout and the group that did not.

Figure 5 reports my main outcome variables, before and after the payout. The insurance company made indemnity payouts between sales periods 3 and 4. So, the first three sales periods are before payout, while the last three are after payout. Panel A of the figure focuses on the demand for the product. In the baseline (sales period 1), the average is around 40% for the group that receives payout between the sales periods 3 and 4 (212 households). On the other hand, the group that never received payout (608 households) had an average demand of around 25% in the baseline. These numbers highlight the baseline differences between these two groups in purchasing the product. For the group that received a payout, the average demand increased to 55-60% for the subsequent two periods. As they received the payout before the fourth sales period, it must be so that they were all covered by the insurance product at the time of the payout. It may seem surprising that the average demand was only around 55-60% for this group, while 100% of them had insurance coverage. But, we can explain this by the overlapping structure of the product design. The sales periods 2 and 3 were both in 2011. So, around 60% of the households that received payout got covered in sales period 2, with the rest of them purchasing the insurance in sales period 3 (some of them bought twice to increase coverage). For the group

[20]For a formal discussion on the potential challenges in the causal identification of payout experience, please consult Appendix A.

that never received a payout, the average demand is 0% for both sales periods 2 and 3. This average demand is what we should expect as these are the households that did not get the payout, implying that they did not have insurance coverage at the time of the payouts. After the payout, I observe a sharp decline in the demand for those who received the payout. Simultaneously, the demand increases slightly for the other group. These observations suggest a negative impact of payout on demand for those receiving the payout, along with a positive spillover effect for those not receiving the payout. But, there may be other factors driving these results.

Panel A: Demand

Panel B: Knowledge

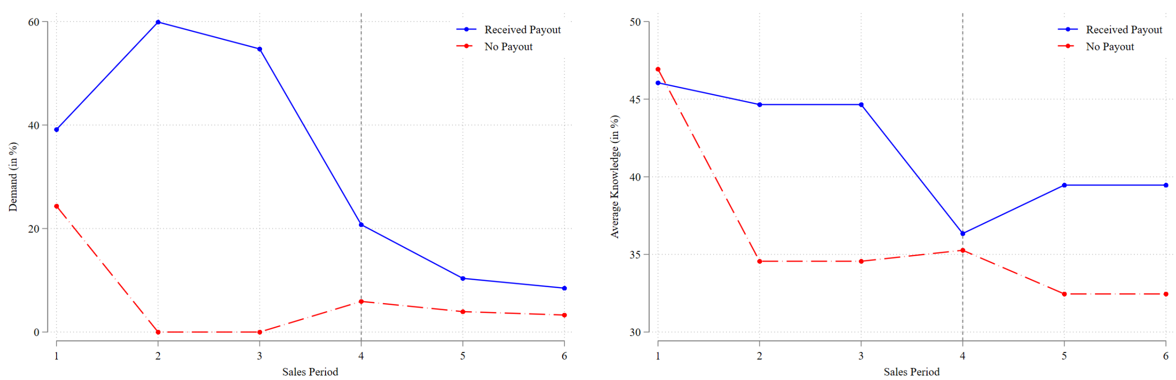

Figure 5: Outcome Variables Before and After Payout Experience

I use the following regression specification to understand the effect of payout experience on demand:

$$Demand_{ijt} = \begin{cases} 1\; if\; Demand^{*}_{ijt} = \alpha^D_0 + \alpha^D_1 Payout_{ij} + \alpha^D_2 Post_t \\ \quad + \alpha^D_3 Payout_{ij} \times Post_t + \lambda^D X_{ijt} + \nu^D_{ijt} > 0 \\ 0\; otherwise, \end{cases} \tag{5}$$

which is a probit specification with $Demand_{ijt}$ being the dummy dependent variable representing the binary decision to purchase the product for household i of index-area j at time t. The dummy variable $Payout_{ij}$ takes 1 for households that received a payout between sales periods 3 and 4, 0 otherwise. $Post_t$ takes 1 for sales periods after payout (i.e., for the sales periods 4, 5, and 6), 0 otherwise. X_{ijt} are observable household-specific controls. The error term ν^D_{ijt} includes index-area fixed effects and random error. The main coefficient of interest is α^D_3, which captures the after-payout effect on those that received a payout.

Regression specification (5) captures the effect on the extensive margin of purchasing or not purchasing the product. However, I am also interested in the intensive margin of the amount of insurance bought. For this purpose, I use the following Tobit specification:

$$TLUI_{ijt} = \begin{cases} TLUI^*_{ijt} \; if \; TLUI^*_{ijt} = \alpha^I_0 + \alpha^I_1 Payout_{ij} + \alpha^I_2 Post_t \\ \qquad + \alpha^I_3 Payout_{ij} \times Post_t + \lambda^I X_{ijt} + \nu^I_{ijt} > 0 \\ 0 \; otherwise, \end{cases} \tag{6}$$

where $TLUI_{ijt}$ is the observed censored variable that captures the number of tropical livestock units insured. $TLUI_{ijt}$ is equals to the latent variable $TLUI^*_{ijt}$ whenever $TLUI^*_{ijt} > 0$, 0 otherwise. Similar to the specification (5), here the main coefficient of interest is α^I_3.

Finally, I focus on the households' performances in the knowledge questions. Panel B of Figure 5 captures this variable. In the baseline, the average knowledge score is around 46% for the group receiving payout between the sales periods 3 and 4 (data available for 186 households). On the other hand, the group that never received payout (data available for 468 households) had an average knowledge score of around 47% in the baseline. Thus, the two groups were similar in the baseline knowledge of the product. For the group that received a payout, the average knowledge score remains around 45% for the next two periods. However, for the group that never received a payout, the average knowledge decreased to around 35% during the same two periods. The higher knowledge score for the households that received a payout, may indicate that they were more interested in the product and thus purchased it simultaneously. It is important to note that I use information from survey round 3 to calculate the knowledge scores for sales periods 2 and 3. Since they reflect the same information, they take the same values. This feature is true for sales periods 5 and 6 as well. Right after the payout, we can observe a sharp decline in the knowledge scores for those who received the payout. Simultaneously, the knowledge scores increase slightly for the other group. These results suggest a negative impact of payout on the knowledge scores for those that received the payout and a positive spillover effect on those that did not receive the payout. In sales periods 5 and 6, we can observe the knowledge scores slightly increase for those who received the payout, with a slight decrease in knowledge scores for the other group.

To understand the causal effect of payout experience on knowledge scores, I use the following regression specification:

$$\begin{aligned} Knowledge_{ijt} = & \alpha^K_0 + \alpha^K_1 Payout_{ij} + \alpha^K_2 Post_t + \alpha^K_3 Payout_{ij} \times Post_t \\ & + \lambda^K X_{ijt} + \nu^K_{ijt}, \end{aligned} \tag{7}$$

where $Knowledge_{ijt}$ captures the households' performance to the knowledge questions. Similar to the last two specifications, here the main coefficient of interest is α^K_3.

The differences-in-differences specifications outlined in regression specifications (5), (6), and (7) exploit the change in outcome variables before and after the payout and the differences between the group that received the payout with the group that did not. In the regressions, I control for the observed differences between the group that received the payout and the one that did not. Thus, identification requires that their unobserved differences remain the same over time. Hence, the identifying assumption for the differences-in-differences strategy is the following:

Identifying Assumption 1: *Unobservable differences between the households receiving and not receiving the payout do not change over time.*

This assumption is the parallel trends assumption of the differences-in-differences estimation strategy. In Figure 5, I observe baseline differences to increase for the outcome variables before the payout in favor of the group receiving the payout. If the same is true for unobserved differences, this can positively bias the differences-in-differences coefficient. Additionally, the figure suggests some positive spillover effect of payout on the outcome variables for the group not receiving a payout. These spillovers can also positively bias the differences-in-differences coefficient. Keeping these limitations of the identification strategy in mind, we can interpret the negative differences-in-differences coefficients (result below) as lower bounds in terms of the absolute value. In other words, with the unobservable differences remaining perfectly the same before the payout between the two groups and absent knowledge spillovers, we should expect more negative differences-in-differences coefficients.

Table 3 presents the differences-in-differences estimates. There are two columns of results per dependent variable. The first column presents the main results. The second column is a slight variation of the same regression specification that drops the $Post_t$ dummy to include the survey round fixed effects instead. The results for $Demand_{ijt}$ are in columns (1) and (2). Receiving the payout decreases demand by 6.3-6.5% for the group that received the payout compared to the group that never did. The result is statistically significant at a 1% level. This is a 77-80% decrease compared to the comparison group mean at the baseline. In terms of the baseline comparison group standard deviation, this is a decrease of 0.2 standard deviations.

As presented in columns (3)-(4), receiving the payout decreases the number of TLU insured by around 41 units for the group that received the payout. This is around a 1025% decrease compared to the baseline comparison group mean, and 7 standard deviations decrease compared to the baseline comparison group SD. The result is significant at the 1% level. Similarly, knowledge scores for the group that received the payout decreased by around 5.9% due to the payout. These results are in columns (5) and (6). Compared to the baseline comparison group mean, this is a decrease of around 15%. It is also a decrease of 0.2 standard deviations compared to the baseline comparison group SD. The result is significant at a 10% level.

The results show that receiving a payout decreases the extensive and intensive margin of demand for the group that received the payout. There is also some evidence in favor of the same group doing worse performances in the knowledge questions. However, there can be different possible mechanisms driving these results. For example, the households receiving a payout may use that money to buy some other product, decreasing their demand for index insurance. This substitution can lead to them losing interest in the insurance product, hence worse performance in the knowledge questions. Through the lens of my theoretical model, I expect these results to be driven by more optimistic households (i.e., those perceiving $\gamma_{ijt} > \gamma^*$) updating their beliefs about γ^* downward upon receiving a payout. The following subsection focuses on identifying the mechanism behind the effect of the payout experience.

Table 3: Effect of Payout Experience: Differences-in-Differences Estimates

	Outcomes					
	Demand		TLU Insured		Knowledge	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Received Payout	0.955***	0.953***	44.420***	44.850***	0.077***	0.076***
(=$Payout_{ij}$)	(0.005)	(0.005)	(10.960)	(11.190)	(0.029)	(0.029)
Post Payout	0.250***		34.690***		-0.001	
(=$Post_t$)	(0.024)		(8.691)		(0.021)	
$Payout_{ij} \times Post_t$	-0.065***	-0.063***	-40.920***	-41.420***	-0.059*	-0.059*
	(0.005)	(0.005)	(10.550)	(10.800)	(0.035)	(0.035)
Baseline Comparison Mean†	0.081	0.081	3.925	3.925	0.383	0.383
(SD)	(0.273)	(0.273)	(5.981)	(5.981)	(0.315)	(0.315)
Survey Round Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	2835	2835	2834	2834	2835	2835
pseudo R^2	0.353	0.363	0.165	0.170		
R^2					0.096	0.096

Notes: Probit marginal effects are reported for demand. * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors clustered at the household level are in parentheses. † Here the comparison group is the households that never received a payout, prior to the payout. For the variable $TLUInsured$, only positive values are considered in the calculation of the comparison group mean and SD. All regressions include a constant term, index-area fixed effects, household characteristics, and other controls. Household characteristics include *lagged demand* (or, *lagged TLU Insured* for the dependent variable TLU insured); *Total TLUs* herded in the year prior to the sales period; *Income* in the season prior to the sales period; *Assets Index* calculated at the baseline; *Extreme and Moderate Risk Aversion* Dummies (with *Risk Neutral* being the omitted category) calculated at the baseline; *Age*, *Age*^{2}, *Gender*, and *Years of Education* of the household head at the baseline; whether *Main Income Source* of the household is related to livestock 5 years prior to the baseline survey; and whether drought is ranked to be the *Most Critical Disaster* by the household in the baseline, for their major livestock loss. Other controls include whether the household received period-specific discount coupon, whether they participated in the knowledge game, whether the household lost any livestock due to drought anytime during a period of one year prior to the sales period, household's demand and knowledge at the baseline, sales period specific average demand and knowledge of other households from the same index-area.

4.1.2 Understanding Mechanism: Triple-Differences Strategy and Estimates

To understand the extent to which the effect of payout experience is due to more optimistic households updating their beliefs about γ^* downward upon receiving a payout, I follow a triple-differences estimation strategy. In addition to using the before and after payout differences between the group receiving the payout and the ones not receiving, here I also use the differences in baseline perception. I proxy for baseline perception of the households regarding the product with their baseline demand. By doing so, I implicitly assume that the households are more likely to purchase the product if they have a more optimistic perception about it.[21] As we can see from Panel A of Figure 5, only 40% of the group receiving payout purchased the product in the baseline. Similarly, the demand was 25% for the group that never received the payout. This observation implies the existence of within-group variation in baseline demand for these two groups. This variation is the additional one I use in the triple-differences estimation strategy, on top of the differences-in-differences variations discussed in the last sub-section. The triple-differences estimates use the following set of specifications:

$$Demand_{ijt} = \begin{cases} 1 \; if \; Demand^*_{ijt} = \beta^D_0 + \beta^D_1 Payout_{ij} + \beta^D_2 Post_t + \beta^D_3 Payout_{ij} \times Post_t \\ \quad + \beta^D_4 Perception_{ij} + \beta^D_5 Payout_{ij} \times Perception_{ij} + \beta^D_6 Post_t \times Perception_{ij} \\ \quad + \beta^D_7 Payout_{ij} \times Post_t \times Perception_{ij} + \delta^D X_{ijt} + \epsilon^D_{ijt} > 0 \\ 0 \; otherwise, \end{cases} \tag{8}$$

$$TLUI_{ijt} = \begin{cases} TLUI^*_{ijt} \; if \; TLUI^*_{ijt} = \beta^I_0 + \beta^I_1 Payout_{ij} + \beta^I_2 Post_t + \beta^I_3 Payout_{ij} \times Post_t \\ \qquad + \beta^I_4 Perception_{ij} + \beta^I_5 Payout_{ij} \times Perception_{ij} + \beta^I_6 Post_t \times Perception_{ij} \\ \qquad + \beta^I_7 Payout_{ij} \times Post_t \times Perception_{ij} + \delta^I X_{ijt} + \epsilon^I_{ijt} > 0 \\ \quad 0 \; otherwise, \end{cases} \tag{9}$$

$$\begin{aligned} Knowledge_{ijt} =& \beta^K_0 + \beta^K_1 Payout_{ij} + \beta^K_2 Post_t + \beta^K_3 Payout_{ij} \times Post_t + \beta^K_4 Perception_{ij} \\ & + \beta^K_5 Payout_{ij} \times Perception_{ij} + \beta^K_6 Post_t \times Perception_{ij} \\ & + \beta^K_7 Payout_{ij} \times Post_t \times Perception_{ij} + \delta^K X_{ijt} + \epsilon^K_{ijt}, \end{aligned} \tag{10}$$

where $Perception_{ij}$ is equal to 1 if the baseline demand is 1, and 0 if the baseline demand is 0. Here the coefficients of interest are β^D_7, β^I_7, and β^K_7. The triple-differences identification needs a weaker identifying assumption than the differences-in-differences strategy discussed in the last subsection. In particular, it needs the following identifying assumption:

Identifying Assumption 2: *Households with different baseline demand (perception) react similarly for changes in unobserved differences over time between the group that received the payout and the group that did not.*

Table 4 reports the estimates following the triple differences strategy. Similar to Table 3, there are two columns of results per dependent variable: the first column presents the main re-

[21] Under my theoretical framework, more optimistic agents anticipate a higher γ^*. Thus, I expect more optimistic households to have a higher demand.

sults, and the second column presents the results for a slight variation in regression specification that drops the $Post_t$ dummy to include the survey round fixed effects.

Table 4: The Mechanism for Effect of Payout Experience: Triple-Differences Estimates

	Outcomes					
	Demand		TLU Insured		Knowledge	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Received Payout	0.952***	0.950***	46.020***	46.020***	0.086**	0.086**
(= $Payout_{ij}$)	(0.006)	(0.006)	(11.510)	(11.610)	(0.037)	(0.037)
Post Payout	0.240***		35.860***		-0.007	
(= $Post_t$)	(0.025)		(8.691)		(0.024)	
Baseline Demand	0.001	0.002	-0.266	-0.262	0.045	0.045
(= $Perception_{ij}$)	(0.001)	(0.001)	(0.360)	(1.071)	(0.035)	(0.035)
$Payout_{ij} \times Post_t$	-0.060***	-0.059***	-41.430***	-41.540***	-0.031	-0.031
	(0.006)	(0.006)	(10.470)	(10.590)	(0.043)	(0.043)
$Payout_{ij} \times Perception_{ij}$	0.003	0.002	1.870***	1.609*	-0.021	-0.021
	(0.002)	(0.002)	(0.663)	(0.954)	(0.059)	(0.059)
$Post_t \times Perception_{ij}$	0.004	0.003	2.898*	2.939*	0.022	0.022
	(0.003)	(0.003)	(1.712)	(1.742)	(0.042)	(0.042)
$Payout_{ij} \times Post_t \times Perception_{ij}$	-0.005***	-0.005***	-4.253*	-3.954*	-0.070	-0.070
	(0.002)	(0.002)	(2.334)	(2.299)	(0.073)	(0.073)
Baseline Comparison Mean†	0.081	0.081	3.925	3.925	0.383	0.383
(SD)	(0.273)	(0.273)	(5.981)	(5.981)	(0.315)	(0.315)
Survey Round Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	2835	2835	2834	2834	2835	2835
pseudo R^2	0.354	0.365	0.166	0.170		
R^2					0.099	0.099

Notes: Probit marginal effects are reported for demand. * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors clustered at the household level are in parentheses. † Here the comparison group is the households that never received a payout, prior to the payout. For the variable $TLUInsured$, only positive values are considered in the calculation of the comparison group mean and SD. All regressions include a constant term, index-area fixed effects, household characteristics, and other controls. Household characteristics include *lagged demand* (or, *lagged TLU Insured* for the dependent variable TLU insured); *Total TLUs* herded in the year prior to the sales period; *Income* in the season prior to the sales period; *Assets Index* calculated at the baseline; *Extreme and Moderate Risk Aversion* Dummies (with *Risk Neutral* being the omitted category) calculated at the baseline; *Age*, *Age*^{2}, *Gender*, and *Years of Education* of the household head at the baseline; whether *Main Income Source* of the household is related to livestock 5 years prior to the baseline survey; and whether drought is ranked to be the *Most Critical Disaster* by the household in the baseline, for their major livestock loss. Other controls include whether the household received period-specific discount coupon, whether they participated in the knowledge game, whether the household lost any livestock due to drought anytime during a period of one year prior to the sales period, household's knowledge at the baseline, sales period specific average demand and knowledge of other households from the same index-area.

The results for $Demand_{ijt}$ are in columns (1) and (2). For the households having a higher demand for the product in the baseline, receiving a payout leads to a 0.5% decrease in demand. The result is statistically significant at a 1% level. This is a 6% decrease, compared to the comparison group mean at the baseline. In comparison to the baseline comparison group standard deviation, this is a decrease of 0.02 standard deviations. In terms of the number of TLU insured, receiving the payout decreases the number of TLU insured by around 4 units for the households having a higher demand for the product in the baseline. These results are in columns

(3)-(4). This is around a 100-108% decrease compared to the baseline comparison group mean, and around 0.7 standard deviations decrease compared to the baseline comparison group SD. The result is most significant at the 10% level.

For the knowledge scores, receiving the payout leads to a decrease of around 7% for the households having a higher demand for the product in the baseline. We can see this in columns (5) and (6). Compared to the baseline comparison group mean, this is a decrease of around 18%. It is also a decrease of around 0.2 standard deviations compared to the baseline comparison group SD. The result is, however, statistically insignificant.

These results suggest that the effect of payout on the outcome variables can, at least in part, be explained by optimistic households updating their beliefs about γ^* downwards following a payout. In particular, I find this to be true for the extensive and intensive margin of demand and not for the households' interest in the product (proxied by their performances in the knowledge questions).

4.2 Assessing the Impact of Exogenous Interventions

Finally, to understand the impact of exogenous discount and knowledge interventions on the outcome variables, I use the following set of regression specifications:

$$Demand_{ijt} = \begin{cases} 1 \; if \; Demand^*_{ijt} = \gamma_0^D + \gamma_1^D Payout_{ijt} + \gamma_2^D DE_{ijt} + \gamma_3^D T_{ijt} \\ \quad + \gamma_4^D Payout_{ijt} \times T_{ijt} + \gamma_5^D DE_{ijt} \times T_{ijt} + \gamma_6^D X_{ijt} + u_{ijt}^D > 0 \\ 0 \; otherwise, \end{cases} \tag{11}$$

$$\begin{aligned} Knowledge_{ijt} = & \gamma_0^K + \gamma_1^K Payout_{ijt} + \gamma_2^K DE_{ijt} + \gamma_3^K T_{ijt} + \gamma_4^K Payout_{ijt} \times T_{ijt} \\ & + \gamma_5^K DE_{ijt} \times T_{ijt} + \gamma_6^K X_{ijt} + u_{ijt}^K. \end{aligned} \tag{12}$$

Regression specification (11) is a probit regression for the dummy dependent variable $Demand_{ijt}$. Similarly, Ordinary Least Square (OLS) regression specification (12) captures the effect on the household's performance in the knowledge questions $Knowledge_{ijt}$. $T_{ijt} = (d_{ijt}, k_{ij})'$ is a 2×1 vector, where d_{ijt} captures whether the household received sales period specific discount coupon and, k_{ij} captures whether they received the one-time knowledge treatment. Like the last section, the dummy variable $Payout_{ijt}$ captures whether the household had a payout experience before the sales period t. DE_{ijt} (DE stands for Disaster Experience) is a dummy variable that captures whether the household lost any livestock due to drought anytime within the year before sales period t. X_{ijt} controls for both time-invariant and time-varying household characteristics. The error terms (u_{ijt}^D and u_{ijt}^K) include index-area fixed effects, survey round fixed effects, and random error.

We should not interpret the coefficients of $Payout_{ijt}$ and DE_{ijt} to be causal, as there are unobserved baseline differences between the households having these experiences and not having these experiences correlated with the outcome variables. However, due to the random assignments of the interventions, we can causally interpret the coefficients of T_{ijt} and its interactions with $Payout_{ijt}$ and DE_{ijt}.

Table 5: Interaction of Payout and Disaster Experience with Exogenous Interventions

	Outcomes					
		Demand			Knowledge	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Payout	0.068***	0.149***	0.177***	0.023	0.018	0.003
	(0.020)	(0.043)	(0.049)	(0.022)	(0.033)	(0.036)
Disaster Experience	-0.011	-0.025	-0.039*	0.011	0.020	0.016
	(0.009)	(0.018)	(0.021)	(0.013)	(0.019)	(0.022)
Discount Treatment		0.055***	0.071***		-0.011	-0.026
		(0.012)	(0.014)		(0.015)	(0.017)
Knowledge Treatment		0.018	0.001		0.054**	0.047**
		(0.016)	(0.018)		(0.022)	(0.024)
Payout × Discount Treatment		-0.047***	-0.068***		0.042	0.053
		(0.015)	(0.015)		(0.032)	(0.034)
Payout × Knowledge Treatment		-0.021	-0.023		-0.073*	-0.082*
		(0.019)	(0.024)		(0.043)	(0.046)
Disaster Experience × Discount Treatment		0.026	0.031		-0.019	-0.004
		(0.021)	(0.026)		(0.021)	(0.024)
Disaster Experience × Knowledge Treatment		-0.011	-0.002		0.005	-0.010
		(0.017)	(0.022)		(0.027)	(0.030)
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	No	No	Yes	No	No	Yes
Observations	3856	3856	2835	3520	3520	2835
pseudo R^2	0.092	0.113	0.117			
R^2				0.069	0.075	0.095

Notes: Probit marginal effects are reported for demand. * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors clustered at the household level are in parentheses. All regressions include a constant term, survey round fixed effects, and index-area fixed effects. Household characteristics include *lagged demand*; *Total Tropical Livestock Units* herded in the year prior to the sales period; *Income* in the season prior to the sales period; *Assets Index* calculated at the baseline; *Extreme and Moderate Risk Aversion* Dummies (with *Risk Neutral* being the omitted category) calculated at the baseline; *Age*, *Age*^{2}, *Gender*, and *Years of Education* of the household head at the baseline; whether *Main Income Source* of the household is related to livestock 5 years prior to the baseline survey; and whether drought is ranked to be the *Most Critical Disaster* by the household in the baseline, for their major livestock loss. Other controls include household's demand and knowledge at the baseline, and sales period specific average demand and knowledge of other households from the same index-area.

Columns (1)-(3) of Table 5 report the results for the dependent variable $Demand_{ijt}$. Unsurprisingly, receiving a discount coupon leads to a highly significant increase in demand by 5.5-7.1%. This result is a 57-74% increase from the control group baseline mean of 9.6%. Concerning the baseline control group standard deviation of 0.296, this is an increase of 0.2 standard deviations. Receiving a knowledge treatment, however, leads to no significant increase in demand. Receiving a discount coupon also leads to a significantly lower association between payout experience and demand. Columns (4)-(6) of the table report the results for the dependent variable $Knowledge_{ijt}$. Unsurprisingly, receiving a knowledge treatment leads to a highly significant increase in knowledge scores by 4.7-5.4%. This result is an 11-13% increase from the control group baseline mean of 42.9%. Concerning the control group's baseline standard deviation of 0.326, this is an increase of 0.1-0.2 standard deviations. Receiving a discount coupon, however, leads to no significant change in knowledge scores. Knowledge treatment also significantly decreases the association between payout experience and knowledge scores. We can observe no other significant heterogeneity in payout and disaster experiences for the exogenous interventions.

These results suggest that receiving a discount coupon mechanically leads to higher demand due to lower costs. Similarly, receiving knowledge interventions improve households' performances in the knowledge questions. However, combining discount intervention with a payout leads to a decrease in demand, while combining knowledge intervention with a payout leads to a decline in the households' interest in the product (proxied by their performances in the knowledge questions).

5 Summary and Concluding Remarks

In this study, I focus on the role of experience in learning about an index insurance product. My theoretical framework formalizes a scenario where agents are learning about the covariate risk they face, as well as the mapping of this covariate risk to the index insurance product that insures against it. The model makes ambiguous predictions regarding the effect of receiving a payout for the index insurance product, with the effect being dependent on the agents' level of optimism about the product design. The model also predicts positive impacts of disaster experiences on demand and interest for the product, ceteris paribus.

As there is already a body of evidence supporting my predictions for disaster experience, I focus primarily on identifying the impact of payout experience. I use a differences-in-differences identification strategy for this purpose. My results show that receiving a payout decreases the extensive and intensive margin of demand for the group that received the payout, with the same group performing worse in answering knowledge questions after the payout. In the subsequent analysis, I use a triple-differences identification strategy to identify the causal mechanism of such an effect. I find that the impact of payout on the outcome variables, at least in part, can be explained by optimistic households updating their beliefs about the product design downwards following a payout.

These results suggest that information frictions drive the demand and interest for index insurance schemes higher than optimal. Receiving a payout helps households to learn the product design, which leads to lower demand and interest in the product. The result is similar to that of Clarke and Kalani (2011), which shows that behavioral biases lead agents to demand higher than optimal. Correcting for the behavioral biases lowers the demand instead of increasing them. My results also support the theoretical findings of Clarke (2016) that rationalize the low demand for index insurance products.

Additional analysis suggests that while receiving a discount intervention mechanically increases demand, it also increases households' chances of receiving a payout leading them to optimally lower their demand.[22] Similarly, knowledge interventions mechanically increase households' knowledge regarding how the product works but helps them update their interest in the product downwards only after these households experience payout. These results are similar to the findings in Cai et al. (2020). However, in their study, the payouts improve demand. These findings suggest that discount and knowledge interventions can enhance households' learning from experience. However, on their own, they may not help in overcoming information frictions.

References

AHMED, S., C. MCINTOSH, AND A. SARRIS (2020): "The Impact of Commercial Rainfall Index Insurance: Experimental Evidence from Ethiopia," *American Journal of Agricultural Economics*, 102, 1154–1176.

BARNETT, B. J., C. B. BARRETT, AND J. R. SKEES (2008): "Poverty Traps and Index-Based Risk Transfer Products," *World Development*, 36, 1766–1785.

BELISSA, T. K., R. LENSINK, AND M. VAN ASSELDONK (2020): "Risk and ambiguity aversion behavior in index-based insurance uptake decisions: Experimental evidence from Ethiopia," *Journal of Economic Behavior & Organization*, 180, 718–730.

BERTRAM-HUEMMER, V. AND K. KRAEHNERT (2017): "Does Index Insurance Help Households Recover from Disaster? Evidence from IBLI Mongolia," *American Journal of Agricultural Economics*, 100, 145–171.

BESLEY, T. AND A. CASE (1993): "Modeling technology adoption in developing countries," *The American Economic Review*, 83, 396–402.

BINSWANGER, H. P. (1980): "Attitudes Toward Risk: Experimental Measurement in Rural India," *American Journal of Agricultural Economics*, 62, 395–407.

BJERGE, B. AND N. TRIFKOVIC (2018): "Extreme weather and demand for index insurance in rural India," *European Review of Agricultural Economics*, 45, 397–431.

CAI, H., Y. CHEN, H. FANG, AND L.-A. ZHOU (2015a): "The Effect of Microinsurance on Economic Activities: Evidence from a Randomized Field Experiment," *Review of Economics and Statistics*, 97, 287–300.

CAI, J., A. DE JANVRY, AND E. SADOULET (2020): "Subsidy Policies and Insurance Demand," *American Economic Review*, 110, 2422–2453.

[22] This is the *scope effect* discussed in Cai et al. (2020).

CAI, J., A. D. JANVRY, AND E. SADOULET (2015b): "Social Networks and the Decision to Insure," *American Economic Journal: Applied Economics*, 7, 81–108.

CAI, J. AND C. SONG (2017): "Do disaster experience and knowledge affect insurance take-up decisions?" *Journal of Development Economics*, 124, 83–94.

CARTER, M. R., A. DE JANVRY, E. SADOULET, AND A. SARRIS (2014): "Index-based weather insurance for developing countries: A review of evidence and a set of propositions for up-scaling," Working Papers P111, FERDI.

CHANTARAT, S., A. G. MUDE, C. B. BARRETT, AND M. R. CARTER (2012): "Designing Index-Based Livestock Insurance for Managing Asset Risk in Northern Kenya," *Journal of Risk and Insurance*, 80, 205–237.

CLARKE, D. AND G. KALANI (2011): "Microinsurance decisions: evidence from Ethiopia," in *CSAE 25th Anniversary Conference*.

CLARKE, D. J. (2016): "A Theory of Rational Demand for Index Insurance," *American Economic Journal: Microeconomics*, 8, 283–306.

COLE, S., X. GINÉ, J. TOBACMAN, P. TOPALOVA, R. TOWNSEND, AND J. VICKERY (2013): "Barriers to Household Risk Management: Evidence from India," *American Economic Journal: Applied Economics*, 5, 104–135.

COLE, S., X. GINÉ, AND J. VICKERY (2017): "How Does Risk Management Influence Production Decisions? Evidence from a Field Experiment," *The Review of Financial Studies*, 30, 1935–1970.

COLE, S., D. STEIN, AND J. TOBACMAN (2014): "Dynamics of Demand for Index Insurance: Evidence from a Long-Run Field Experiment," *American Economic Review*, 104, 284–290.

DERCON, S., R. V. HILL, D. CLARKE, I. OUTES-LEON, AND A. S. TAFFESSE (2014): "Offering rainfall insurance to informal insurance groups: Evidence from a field experiment in Ethiopia," *Journal of Development Economics*, 106, 132–143.

ELABED, G., M. F. BELLEMARE, M. R. CARTER, AND C. GUIRKINGER (2013): "Managing basis risk with multiscale index insurance," *Agricultural Economics*, 44, 419–431.

ELABED, G. AND M. CARTER (2014): "Ex-ante impacts of agricultural insurance: Evidence from a field experiment in Mali," *Unpublished Manuscript*.

ELABED, G. AND M. R. CARTER (2015): "Compound-risk aversion, ambiguity and the willingness to pay for microinsurance," *Journal of Economic Behavior & Organization*, 118, 150–166.

FOSTER, A. D. AND M. R. ROSENZWEIG (1995): "Learning by Doing and Learning from Others: Human Capital and Technical Change in Agriculture," *Journal of Political Economy*, 103, 1176–1209.

GEBREKIDAN, T., Y. GUO, S. BI, J. WANG, C. ZHANG, J. WANG, AND K. LYU (2019): "Effect of index-based livestock insurance on herd offtake: Evidence from the Borena zone of southern Ethiopia," *Climate Risk Management*, 23, 67–77.

GINÉ, X., D. KARLAN, AND M. NGATIA (2013): "Social networks, financial literacy and index insurance," *Working Paper*.

HILL, R. V., N. KUMAR, N. MAGNAN, S. MAKHIJA, F. DE NICOLA, D. J. SPIELMAN, AND P. S. WARD (2019): "Ex ante and ex post effects of hybrid index insurance in Bangladesh," *Journal of Development Economics*, 136, 1–17.

HILL, R. V., M. ROBLES, AND F. CEBALLOS (2016): "Demand for a Simple Weather Insurance Product in India: Theory and Evidence," *American Journal of Agricultural Economics*, 98, 1250–1270.

IKEGAMI, M. AND M. SHEAHAN (2014): "Index Based Livestock Insurance (IBLI) Marsabit Household Survey Codebook," *International Livestock Research Institute*.

JANZEN, S., N. MAGNAN, C. MULLALLY, S. SHIN, I. B. PALMER, J. ODUOL, AND K. HUGHES (2020a): "Can Experiential Games and Improved Risk Coverage Raise Demand for Index Insurance? Evidence from Kenya," *American Journal of Agricultural Economics*, 103, 338–361.

JANZEN, S. A. AND M. R. CARTER (2018): "After the Drought: The Impact of Microinsurance on Consumption Smoothing and Asset Protection," *American Journal of Agricultural Economics*, 101, 651–671.

JANZEN, S. A., M. R. CARTER, AND M. IKEGAMI (2020b): "Can insurance alter poverty dynamics and reduce the cost of social protection in developing countries?" *Journal of Risk and Insurance*.

JENSEN, N. AND C. BARRETT (2016): "Agricultural Index Insurance for Development," *Applied Economic Perspectives and Policy*, 39, 199–219.

JENSEN, N. D., C. B. BARRETT, AND A. G. MUDE (2016): "Index Insurance Quality and Basis Risk: Evidence from Northern Kenya," *American Journal of Agricultural Economics*, 98, 1450–1469.

——— (2017): "Cash transfers and index insurance: A comparative impact analysis from northern Kenya," *Journal of Development Economics*, 129, 14–28.

JENSEN, N. D., A. G. MUDE, AND C. B. BARRETT (2018): "How basis risk and spatiotemporal adverse selection influence demand for index insurance: Evidence from northern Kenya," *Food Policy*, 74, 172–198.

JOVANOVIC, B. AND Y. NYARKO (1996): "Learning by Doing and the Choice of Technology," *Econometrica*, 64, 1299.

KARLAN, D., R. OSEI, I. OSEI-AKOTO, AND C. UDRY (2014): "Agricultural Decisions after Relaxing Credit and Risk Constraints," *The Quarterly Journal of Economics*, 129, 597–652.

MATSUDA, A., K. TAKAHASHI, AND M. IKEGAMI (2019): "Direct and indirect impact of index-based livestock insurance in Southern Ethiopia," *The Geneva Papers on Risk and Insurance - Issues and Practice*, 44, 481–502.

MIRANDA, M. J. AND K. FARRIN (2012): "Index Insurance for Developing Countries," *Applied Economic Perspectives and Policy*, 34, 391–427.

MOGGE, L. AND K. KRAEHNERT (2022): "The Effect of Risk Perception on the Demand for Index Insurance in Mongolia," *Unpublished Manuscript*.

NORITOMO, Y. AND K. TAKAHASHI (2020): "Can Insurance Payouts Prevent a Poverty Trap? Evidence from Randomised Experiments in Northern Kenya," *The Journal of Development Studies*, 56, 2079–2096.

PATT, A., P. SUAREZ, AND U. HESS (2010): "How do small-holder farmers understand insurance, and how much do they want it? Evidence from Africa," *Global Environmental Change*, 20, 153–161.

PLATTEAU, J.-P., O. D. BOCK, AND W. GELADE (2017): "The Demand for Microinsurance: A Literature Review," *World Development*, 94, 139–156.

SANTERAMO, F. G. (2018): "Imperfect information and participation in insurance markets: evidence from Italy," *Agricultural Finance Review*, 78, 183–194.

SERFILIPPI, E., M. CARTER, C. GUIRKINGER, E. SERFILIPPI, M. CARTER, AND C. GUIRKINGER (2015): "Certain and Uncertain Utility and Insurance Demand: Results From a Framed Field Experiment in Burkina Faso," *Unpublished Manuscript*.

STEIN, D. (2016): "Dynamics of Demand for Rainfall Index Insurance: Evidence from a Commercial Product in India," *The World Bank Economic Review*, lhw045.

STERN, J. (2019): "Why Don't Poor Farmers want rainfall insurance? The role of trust and cognitive load," *Unpublished Manuscript*.

TAKAHASHI, K., M. IKEGAMI, M. SHEAHAN, AND C. B. BARRETT (2016): "Experimental Evidence on the Drivers of Index-Based Livestock Insurance Demand in Southern Ethiopia," *World Development*, 78, 324–340.

TAKAHASHI, K., Y. NORITOMO, M. IKEGAMI, AND N. D. JENSEN (2020): "Understanding pastoralists' dynamic insurance uptake decisions: Evidence from four-year panel data in Ethiopia," *Food Policy*, 95, 101910.

TIMU, A. G., C. R. GUSTAFSON, M. IKEGAMI, AND N. JENSEN (2018): "Indemnity Payouts, Learning from others and Index Insurance Uptake," 2018 Annual Meeting, August 5-7, Washington, D.C. 274495, Agricultural and Applied Economics Association.

Appendices

A Payout Experience: Identification Problem

The main problem in identifying the causal effects of payout experience is that the households having the experience differ from the other households. Let me discuss why I cannot just regress $Demand_{jjt}$ or $Knowledge_{ijt}$ on $Payout_{ijt}$ and interpret the regression coefficients as representing a causal relationship. The dummy variable $Payout_{ijt}$ captures whether the household had a payout experience before the sales period t. Thus, in such regressions, the coefficient of $Payout_{ijt}$ would capture two separate sets of comparison:

1. **Within-group Comparison:** Comparing the households that received the payout before and after the payout.

2. **Between-group Comparison:** Comparing the households that received the payout with the households that did not.

The within-group comparison is problematic because changes that happened over time unrelated to the payout experience may have an impact on the outcome variables. If not controlled for, we may wrongfully attribute these changes as the causal effect of payout experience. However, the survey round fixed effects should account for part of this bias. Particularly the part that is common to every household in the sample. For causal identification, the more problematic comparison here is the between-group comparison. The households receiving the payout are not similar to the ones not receiving. For starters, the households receiving the payout had insurance coverage at the time of the payout, while the others did not. These two sets of households also differ in their observable demographics, as well as baseline knowledge and demand. However, we can control for all these observable differences in the regression. But, I cannot account for the unobserved differences. Even though household fixed effects can be a solution, I cannot use them in the non-linear probit specification for dummy dependent variable $Demand_{ijt}$.

To understand the direction of selection bias in the coefficient of $Payout_{ijt}$ in the regression of $Demand_{jjt}$ or $Knowledge_{ijt}$ on $Payout_{ijt}$, focus on the heterogeneity analysis of payout experience in Table A.6. To keep it comparable with Table 5, I keep the specifications same as (11) and (12). Columns (1)-(3) presents the results for the dependent variable $Demand_{ijt}$. Column (1) is repeating the results from column (3) of Table 5. Column (2) reports the results without the survey round fixed effects. Thus, this column does not control for part of the bias from the within-group comparison discussed in the last sub-section. The coefficient of $Payout_{ijt}$ in column (2) is lower than that of column (1), suggesting that the survey round fixed effects correct for a downward bias in the coefficient. In column (3), I restrict the sample to the households that ever received a payout. Thus, restricting the coefficient to reflect only the within-group comparison. For within-group comparison, the coefficient is negative and insignificant. This result suggests that the between-group comparison creates an upward bias in the coefficient. Thus, not controlling for it in the regression is overestimating the coefficient of $Payout_{ijt}$ for the dependent variable $Demand_{ijt}$.

Columns (4)-(6) present the results for the dependent variable $Knowledge_{ijt}$. Column (4) is repeating the results from column (6) of Table 5. Column (5) reports the results without the survey round fixed effects. The coefficient of $Payout_{ijt}$ in column (5) is very close to that of column (4), suggesting that controlling for the survey round fixed effects creates no bias correction in the coefficient. When I restrict the sample to the households that ever received a payout in column (6), the coefficient is negative at a 10% level of significance. This result suggests that the between-group comparison creates an upward bias in the coefficient. Thus, not controlling for it in the regression is overestimating the coefficient of $Payout_{ijt}$ for the dependent variable $Knowledge_{ijt}$.

Table A.6: Heterogeneity Analysis of Payout Experience: Full Sample vs. Restricted Sample

	Outcomes					
		Demand			Knowledge	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Payout	0.177***	0.158***	-0.112	0.003	-0.002	-0.099*
	(0.049)	(0.045)	(0.081)	(0.036)	(0.036)	(0.058)
Disaster Experience	-0.039*	-0.030	-0.033	0.016	0.018	0.018
	(0.021)	(0.022)	(0.070)	(0.022)	(0.022)	(0.040)
Discount Treatment	0.071***	0.070***	0.194***	-0.026	-0.025	-0.046
	(0.014)	(0.015)	(0.055)	(0.017)	(0.017)	(0.047)
Knowledge Treatment	0.001	-0.002	0.159**	0.047**	0.047*	0.044
	(0.018)	(0.018)	(0.068)	(0.024)	(0.024)	(0.066)
Payout × Discount Treatment	-0.068***	-0.071***	-0.232***	0.053	0.054	0.072
	(0.015)	(0.016)	(0.064)	(0.034)	(0.034)	(0.053)
Payout × Knowledge Treatment	-0.023	-0.020	-0.142**	-0.082*	-0.081*	-0.072
	(0.024)	(0.025)	(0.059)	(0.046)	(0.046)	(0.070)
Disaster Experience × Discount Treatment	0.031	0.036	0.006	-0.004	-0.004	0.003
	(0.026)	(0.027)	(0.072)	(0.024)	(0.024)	(0.047)
Disaster Experience × Knowledge Treatment	-0.002	0.001	-0.122*	-0.010	-0.010	0.012
	(0.022)	(0.023)	(0.070)	(0.030)	(0.030)	(0.058)
Survey Round Fixed Effects	Yes	No	No	Yes	No	No
Restricted Sample	No	No	Yes	No	No	Yes
Observations	2835	2835	861	2835	2835	861
pseudo R^2	0.117	0.106	0.228			
R^2				0.095	0.094	0.078

Notes: Probit marginal effects are reported for demand. * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors clustered at the household level are in parentheses. Restricted sample focuses only on the households that receive payout at least once during the study periods. All regressions include a constant term, index-area fixed effects, household characteristics, and other controls. Household characteristics include *lagged demand*; *Total Tropical Livestock Units* herded in the year prior to the sales period; *Income* in the season prior to the sales period; *Assets Index* calculated at the baseline; *Extreme and Moderate Risk Aversion* Dummies (with *Risk Neutral* being the omitted category) calculated at the baseline; *Age*, *Age*^{2}, *Gender*, and *Years of Education* of the household head at the baseline; whether *Main Income Source* of the household is related to livestock 5 years prior to the baseline survey; and whether drought is ranked to be the *Most Critical Disaster* by the household in the baseline, for their major livestock loss. Other controls include household's demand and knowledge at the baseline, and sales period specific average demand and knowledge of other households from the same index-area.

B Knowledge Questions

Knowledge Question 1: How often do you have to pay a premium in order to remain insured?
Answers: Don't Know/ Remain insured until compensated/ Once every two years/ Once every six months/ Once every year
Right Answer: Once every year

Knowledge Question 2: If you did not receive indemnity payout (compensation) from the livestock insurance, would you expect to receive your premium back?
Answers: Don't Know/ Yes/ No
Right Answer: No

Knowledge Question 3: What institution will provide you indemnity payout if there is a payout?
Answers: Don't Know/ Equity Bank/ ILRI/ UAP Insurance/ APA Insurance/ Government/ NGO
Right Answer: UAP Insurance for sales periods 1-3, APA Insurance for sales periods 4-6.

For each knowledge questions, I code 0- Wrong, 1- Right. Then the $Knowledge_{ijt}$ variable is constructed as:

$$Knowledge_{ijt} = 1/3(Knowledge^1_{ijt} + Knowledge^2_{ijt} + Knowledge^3_{ijt})$$

where $Knowledge^m_{ijt}$ represents their performance in Knowledge Question m.

C Robustness Checks

Table C.7: Diff-in-Diff Estimates with Ordinary Least Squares for Demand and TLU Insured

	Outcomes					
		Demand			TLU Insured	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Received Payout	0.573***	0.569***	0.573***	1.600***	1.668***	1.670***
(=$Payout_{ij}$)	(0.012)	(0.014)	(0.014)	(0.150)	(0.175)	(0.175)
Post Payout	0.044***	0.103***		0.164***	0.134	
(=$Post_t$)	(0.005)	(0.019)		(0.054)	(0.086)	
$Payout_{ij} \times Post_t$	-0.485***	-0.488***	-0.492***	-1.380***	-1.508***	-1.516***
	(0.019)	(0.022)	(0.023)	(0.163)	(0.209)	(0.209)
Baseline Comparison Mean†	0.081	0.081	0.081	3.925	3.925	3.925
(SD)	(0.273)	(0.273)	(0.273)	(5.981)	(5.981)	(5.981)
Control Variables	No	Yes	Yes	No	Yes	Yes
Survey Round Fixed Effects	No	No	Yes	No	No	Yes
Observations	4100	2835	2835	4098	2834	2834
R^2	0.310	0.310	0.316	0.054	0.065	0.066

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors clustered at the household level are in parentheses. † Here the comparison group is the households that never received a payout, prior to the payout. For the variable $TLUInsured$, only positive values are considered in the calculation of the comparison group mean and SD. All regressions include a constant term. Control variables include index-area fixed effects, household characteristics, and other controls. Household characteristics include *lagged demand* (or, *lagged TLU Insured* for the dependent variable TLU insured); *Total TLUs* herded in the year prior to the sales period; *Income* in the season prior to the sales period; *Assets Index* calculated at the baseline; *Extreme and Moderate Risk Aversion* Dummies (with *Risk Neutral* being the omitted category) calculated at the baseline; *Age*, *Age*^{2}, *Gender*, and *Years of Education* of the household head at the baseline; whether *Main Income Source* of the household is related to livestock 5 years prior to the baseline survey; and whether drought is ranked to be the *Most Critical Disaster* by the household in the baseline, for their major livestock loss. Other controls include whether the household received period-specific discount coupon, whether they participated in the knowledge game, whether the household lost any livestock due to drought anytime during a period of one year prior to the sales period, household's demand and knowledge at the baseline, sales period specific average demand and knowledge of other households from the same index-area.

Table C.8: Triple-Diff Estimates with Ordinary Least Squares for Demand and TLU Insured

	Outcomes					
		Demand			TLU Insured	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Received Payout	0.558***	0.547***	0.552***	1.186***	1.253***	1.258***
(=$Payout_{ij}$)	(0.014)	(0.016)	(0.016)	(0.117)	(0.151)	(0.151)
Post Payout	0.034***	0.101***		0.087***	0.063	
(=$Post_t$)	(0.006)	(0.019)		(0.020)	(0.141)	
Baseline Demand	0.000	0.017	0.020	0.000	-0.002	-0.005
(=$Perception_{ij}$)	(0.000)	(0.013)	(0.013)	(0.001)	(0.036)	(0.036)
$Payout_{ij} \times Post_t$	-0.458***	-0.448***	-0.454***	-1.019***	-1.125***	-1.137***
	(0.022)	(0.028)	(0.028)	(0.121)	(0.164)	(0.165)
$Payout_{ij} \times Perception_{ij}$	0.038	0.057**	0.054*	1.057***	1.007***	0.998***
	(0.026)	(0.028)	(0.028)	(0.345)	(0.367)	(0.367)
$Post_t \times Perception_{ij}$	0.040***	0.011	0.009	0.320	0.273	0.276
	(0.014)	(0.019)	(0.019)	(0.214)	(0.240)	(0.240)
$Payout_{ij} \times Post_t \times Perception_{ij}$	-0.084**	-0.092*	-0.089*	-1.044**	-0.925**	-0.915**
	(0.042)	(0.047)	(0.047)	(0.421)	(0.443)	(0.443)
Baseline Comparison Mean†	0.081	0.081	0.081	3.925	3.925	3.925
(SD)	(0.273)	(0.273)	(0.273)	(5.981)	(5.981)	(5.981)
Control Variables	No	Yes	Yes	No	Yes	Yes
Survey Round Fixed Effects	No	No	Yes	No	No	Yes
Observations	4100	2835	2835	4098	2834	2834
R^2	0.311	0.311	0.318	0.065	0.069	0.070

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors clustered at the household level are in parentheses. † Here the comparison group is the households that never received a payout, prior to the payout. For the variable $TLUInsured$, only positive values are considered in the calculation of the comparison group mean and SD. All regressions include a constant term. Control variables include index-area fixed effects, household characteristics, and other controls. Household characteristics include *lagged demand* (or, *lagged TLU Insured* for the dependent variable TLU insured); *Total TLUs* herded in the year prior to the sales period; *Income* in the season prior to the sales period; *Assets Index* calculated at the baseline; *Extreme and Moderate Risk Aversion* Dummies (with *Risk Neutral* being the omitted category) calculated at the baseline; *Age*, *Age*^{2}, *Gender*, and *Years of Education* of the household head at the baseline; whether *Main Income Source* of the household is related to livestock 5 years prior to the baseline survey; and whether drought is ranked to be the *Most Critical Disaster* by the household in the baseline, for their major livestock loss. Other controls include whether the household received period-specific discount coupon, whether they participated in the knowledge game, whether the household lost any livestock due to drought anytime during a period of one year prior to the sales period, household's demand and knowledge at the baseline, sales period specific average demand and knowledge of other households from the same index-area.