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

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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 suggest the negative effect of payout experience and the positive effect of disaster experience in shaping the relationship between exogenous discount interventions and demand. Additionally, I find that receiving a payout negatively impacts extensive and intensive margins of demand. Subsequently, I find evidence that the effect of payout on the extensive margin of demand, at least in part, can be explained by optimistic households updating their beliefs about the product design downwards following a payout. My analysis suggests that discount interventions on their own may not be able to help overcome information frictions. Yet, these interventions can help households overcome these frictions by allowing them to learn from experience. However, contrary to the common belief, such learning may lead to a decrease in demand.

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

Keywords: Index Insurance, Learning, Agriculture, Kenya.

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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 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 (Cai and Song, 2017; Bjerge and Trifkovic, 2018; Cai et al., 2020). 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 investigate the effect of payout and disaster experience in shaping the demand and knowledge for an index insurance product in rural Kenya. The objective of this study is twofold. First, to provide evidence on the impact of different experiences. Second, to understand the mechanism behind such an impact through the lens of resolving information frictions.

I develop a theoretical model that focuses on households learning by doing and abstracts away from the social learning aspects of learning from experience.[4] The model 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

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

[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.

[4] This is motivated by the availability of information in the dataset used in this study.

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' optimism regarding the product design. Additionally, the model predicts the positive impacts of disaster experiences on demand and knowledge for the product, ceteris paribus.

To test the predictions of my theoretical model, I use data from Index-Based Livestock Insurance (IBLI), Kenya. I exploit the randomized interventions in the data to understand the effect of disaster and payout experiences in shaping the impact of these interventions on the demand and knowledge for the product. Additionally, 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. The analysis controls for social learning effects and focuses on the learning by doing aspect of learning from experience, following my theoretical framework.[5]

The empirical results suggest the negative effect of payout experience and the positive effect of disaster experience in shaping the relationship between exogenous discount interventions and demand. Additionally, I find that receiving a payout negatively impacts demand. These results hold for both extensive and intensive margins of demand, but not for the knowledge regarding the product. Subsequently, I find that the effect of payout on the extensive margin of demand, at least in part, can be explained by optimistic households updating their beliefs about the product design downwards following a payout.

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.

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 from experience. Their argument for expecting a learning effect applies to index insurance products since insurance is an experience good. In addition,

[5] I use leave-out means of demand and knowledge as controls for the social learning effects. Leave-out means are widely used in the literature to control for peer effects. However, caution needs to be taken in causally interpreting the coefficients of these variables (Angrist, 2014).

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 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), 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, focusing on the learning-by-doing aspect of learning from experience.

Finally, I provide some evidence for policy directions on using interventions to improve learning from experience for index insurance products. The role of discount and knowledge interventions in increasing demand for index insurance products is well recognized.[6] 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 understanding how these interventions interact with learning from experience. To the best of my knowledge, Cai et al. (2020) is the only 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 $c_{ijt} \leq A_{ijt} + f(A_{ijt})$

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

with $A_{ijt+1} \geq 0$, where $f(\cdot)$ is a fixed production function that does not change over time.[7] 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 $A_{ijt+1} = (A_{ijt} + f(A_{ijt}) - c_{ijt})(1 - \mu_{ijt+1})$.[8] Thus, the household's optimization problem is:

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

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.[9] 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

[7] 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.

[8] 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 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.

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

information regarding the basis risk associated with the product and makes their decisions accordingly. The household's optimization problem becomes:

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

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.[10] 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:[11]

<u>Case 1:</u> $\gamma_{ijt} < \gamma^*$. 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

[10] This assumes that even though households observe θ and they observe the demand for other households (at least in the extensive margin), they don't observe whether the payout has been made if they didn't purchase the product themselves.

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

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,[12] 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:

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.

[12] This is because once they receive a payout they can observe the per-unit return of $(\gamma^*\theta_{jt+1} + \nu_{jt+1} - s)$. As they observe θ_{jt+1} and know s, given their belief of ν_{jt+1}, they can learn about γ^* from this. However, it is a noisy learning process due to the presence of ν.

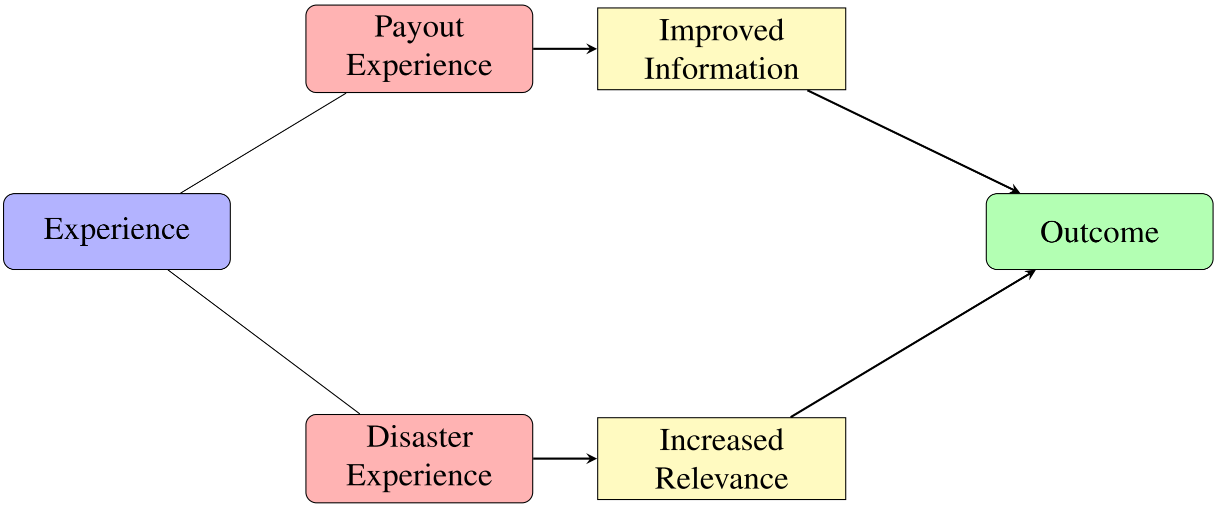


Figure 1: Effect of different experiences on final outcome

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), Dougherty et al. (2020), 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 mainly focus on testing Hypothesis 1. I indirectly test Hypothesis 2, but do not focus on directly testing it 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.

3 Data and Descriptives

The objective of this section is to describe the dataset I use in this study and how it relates to the theoretical framework described in the last section. 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 discussing how this data can be used to test my theoretical predictions.

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.[13] 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.[14] 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.

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.[15] 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.[16] If the calculated area-average livestock mortality in an index region was

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

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

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

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

higher than a certain threshold, payouts were made to all households covered by the insurance in that region.[17] 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.[18] 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 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.[19]

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 non-transferable to other households and only valid for the sales period when they got distributed. More details on these

[17]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.

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

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

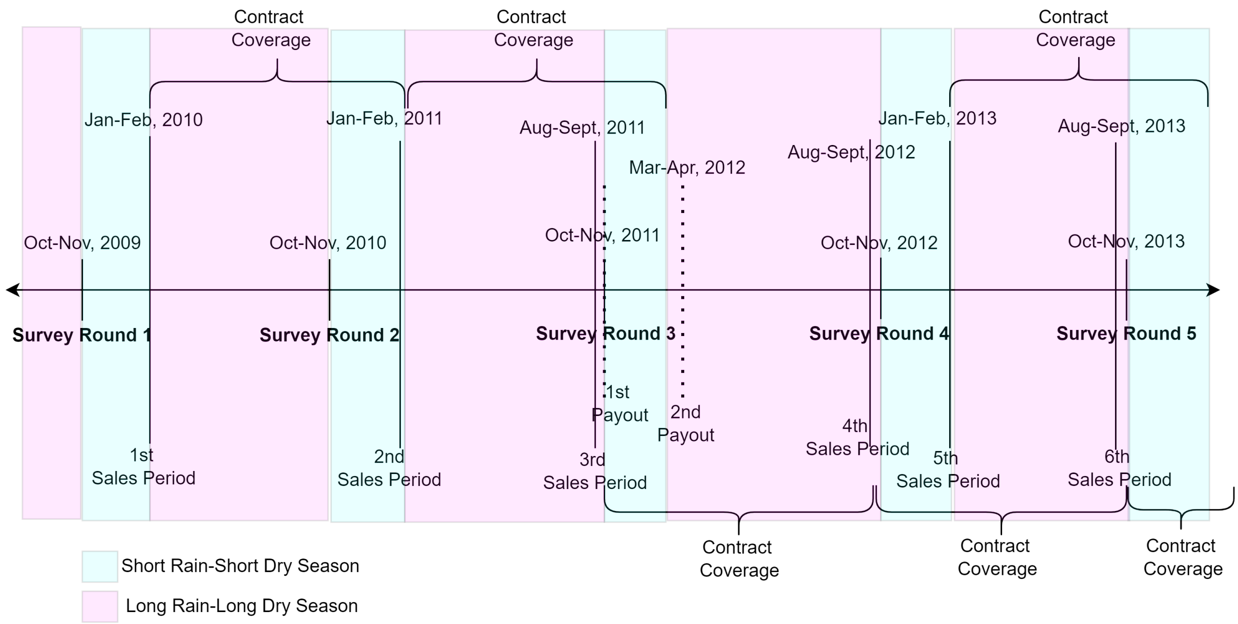


Figure 2: Timeline of IBLI Marsabit

interventions and associated descriptive figures are in Appendix A.

3.3 Bringing Theory and Data Together

As can be seen in Figure 2, the insurance company made two indemnity payouts between sales periods 3 and 4. The first round of payouts was made in all four index regions, while the second one only covered two regions. These introduce the key source of variation for my analysis. Among the balanced panel of 820 households, the payout information is constructed for 784 households: 176 of them received the payout, while the other 608 did not.[20] For my main empirical analysis, I exploit the variation between the group that received the payout and the group that did not, before and after the payout.

I also construct a measure of disaster experience based on whether the household reported

[20] I construct an objective measure of whether the households received a payout. In the first indemnity payout (Oct-Nov, 2011, made for Mar 2011- Sept 2011), the insurance company made payouts for all four index areas to everyone covered from the 2nd sales period (Mar 2011-Feb 2012 contract). The second indemnity payout (Mar-Apr, 2012, made for Oct 2011-Feb 2012) was made in only two index areas to everyone covered from the 2nd sales period (Mar 2011-Feb 2012 contract) and 3rd sales period (Oct 2011-Sept, 2012 contract). Both payouts were announced after sales period 3, so people didn't buy insurance in the 3rd sales period after observing people receiving payouts after the 2nd sales period. Thus, from the 4th sales period onwards, anyone purchasing a contract in the 2nd sales period has received two payouts. Anyone buying insurance only in the 3rd sales period (i.e., not in the 2nd) and residing in the two index areas where payouts were made after the 3rd sales period, should have also received a payout. Then there are people not covered after the 2nd sales period, covered after the 3rd but not part of the index areas where payouts were given (36 households). These people started purchasing the product from not purchasing before observing any payouts. They may not fit well in the comparison group as they started changing their behavior (demand) before treatment(payout). So I drop them from both treatment (payout) and comparison (non-payout) groups. Hence, here the comparison is between those that received a payout in the first and/or second indemnity payout period, and those that never received a payout (also did not purchase the product in the 3rd sales period).

losing any livestock due to drought (the exogenous shock being insured by the IBLI) anytime during one year before the sales period. I interact the measure of payout and disaster experience with the exogenous discount interventions (randomly distributed before every sales period) to indirectly test the impact of these experiences on the outcome variables. This is done by testing the impact of payout and disaster experience on the relationship between the discount interventions and the outcome variables.

Table 1 presents the summary statistics by whether or not the households received the payout. The variables in Panel A use total or average over all 6 sales periods; Panel B variables use information collected in the baseline survey[21] (survey round 1) about the time-invariant characteristics; and Panel C variables use information collected in survey round 2 (first survey round after the product became available for purchase).

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 Panel A in Table 1 shows that households on average purchased the insurance product 0.7 times out of the 6 sales periods. There is a significant difference between the group that received the payout (purchased approximately 2 times) and the ones that did not (purchased approximately 0.4 times). This is not surprising as receiving the payout is conditional on purchase. However, as shown in the second row, conditional on purchasing the product there is no significant difference between the number of Tropical Livestock Units (TLUs) insured.[22] Given the overlapping design of the contracts described above, the households can already be covered by an insurance contract in any sales period. This will decrease their likelihood of purchasing insurance (or the number of TLUs insured conditional on purchasing insurance) at any sales period. In practice, such a possibility occurred only during 3 out of the 6 sales periods, namely during sales periods 3, 5, and 6. On average, as shown in the third row, these happened 0.3 times out of the 3 sales periods, with a significant difference between the group that received the payout (approximately 1 time) and the ones that did not (approximately 0.1 times). Also not surprising due to the conditionality of payout on purchase. The variable in the fourth row of Panel A 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.[23] The numbers show a significant difference in average knowledge scores between the two groups. This may be because the households receiving the payout are more interested in the product as they purchase it and hence they have better knowledge scores. Similarly, the group receiving the payout received more period-specific discount coupons on average. These coupons are possibly one of the reasons leading them to purchase more insurance, increasing the likelihood of receiving a payout. However, I observe no significant difference in disaster

[21]Except whether the household received the one-time knowledge treatment. This information is only available from survey round 2 onwards.

[22]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").*"

[23]Details regarding the knowledge questions are in Appendix C.

experience between the two groups. This may be due to measurement errors or a demonstration of high basis risk associated with the product.

Table 1: Summary Statistics by Payout Experience

Group	No Payout	Received Payout	Combined	Differences
Panel A: Frequency over time				
Average no. of time Purchased IBLI	0.375	1.989	0.737	-1.614***
	(0.025)	(0.069)	(0.035)	(0.060)
Average no. of TLUs Insured†	4.161	4.274	4.200	-0.114
	(0.592)	(0.489)	(0.422)	(0.889)
No. of time Covered by an overlapping contract	0.099	1.023	0.306	-0.924***
	(0.013)	(0.047)	(0.020)	(0.036)
Average Knowledge Scores	0.348	0.421	0.364	-0.073***
	(0.008)	(0.014)	(0.007)	(0.017)
No. of time Discount Coupons received	3.732	4.131	3.821	-0.399***
	(0.049)	(0.078)	(0.042)	(0.100)
No. of time reported Experiencing a Disaster	2.992	2.909	2.973	0.083
	(0.061)	(0.111)	(0.053)	(0.127)
Panel B: Time-invariant baseline characteristics				
Age of HH Head††	48.712	47.369	48.410	1.342
	(0.764)	(1.325)	(0.663)	(1.589)
Gender of HH Head	0.362	0.381	0.366	-0.019
(Female=1)	(0.020)	(0.037)	(0.017)	(0.041)
Education of HH Head†††	1.104	0.880	1.054	0.224
	(0.125)	(0.198)	(0.107)	(0.256)
Observations	608	176	784	-

(continued on next page)

The first three rows of Panel B in Table 1 show that the average household in the survey has a head aged around 48 years, who is 63% likely to be male, who completed around one year of education, and has a baseline asset index of 19.5%.[24] There is no significant difference in these demographics between the group that received the payout and the group that did not. 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 44.5% are moderate risk-averse, with no significant differences between the two groups. The following row shows that around 75% households report livestock as their main income source, again with no significant differences between the groups. However, as presented in the next two rows, there are significant differences between the two groups in whether they reported drought being the most critical disaster and whether they migrated in the year before the baseline. The group that received a payout is more likely to report drought as

[24] Details regarding the calculation of the asset index are in the footnote of Table 1.

Table 1: Summary Statistics by Payout Experience (continued)

Group	No Payout	Received Payout	Combined	Differences
Panel B: Time-invariant baseline characteristics (continued)				
Assets Index	0.189	0.212	0.195	-0.023
	(0.008)	(0.016)	(0.007)	(0.016)
Extreme Risk Averse	0.273	0.244	0.267	0.029
	(0.018)	(0.032)	(0.016)	(0.038)
Moderate Risk Averse	0.434	0.483	0.445	-0.049
	(0.020)	(0.038)	(0.018)	(0.043)
Main Income Source	0.745	0.750	0.746	-0.005
(Livestock=1)	(0.018)	(0.033)	(0.016)	(0.037)
Most Critical Disaster	0.898	0.955	0.911	-0.057**
(Drought=1)	(0.012)	(0.016)	(0.010)	(0.024)
Recently Migrated	0.725	0.557	0.688	0.169***
	(0.018)	(0.038)	(0.017)	(0.039)
Participated in Knowledge Game††††	0.293	0.307	0.296	-0.014
	(0.018)	(0.035)	(0.016)	(0.039)
Panel C: Time-varying characteristics in the baseline				
Baseline Demand	0.243	0.420	0.283	-0.177***
	(0.017)	(0.037)	(0.016)	(0.038)
Baseline no. of TLUs Insured†	3.925	4.341	4.069	-0.416
	(0.513)	(0.499)	(0.377)	(0.793)
Baseline Knowledge Scores‡	0.469	0.486	0.473	-0.016
	(0.015)	(0.026)	(0.013)	(0.031)
Income (in 1000 Kenyan shilling) ‡‡	10.468	10.874	10.558	-0.406
	(0.935)	(1.504)	(0.800)	(1.927)
Total TLUs ‡‡‡	28.994	27.637	28.688	1.357
	(1.317)	(2.043)	(1.119)	(2.680)
Baseline average Demand of others	0.281	0.300	0.285	-0.019***
	(0.003)	(0.006)	(0.003)	(0.007)
Baseline average Knowledge Scores for others‡	0.464	0.490	0.470	-0.027***
	(0.003)	(0.005)	(0.003)	(0.006)
Observations	608	176	784	-

Notes: * p<0.10, ** p<0.05, *** p<0.01. †Uses information for 208 households: 136 with no payout and 72 received payout. ††Information available for 783 households: 607 with no payout and 176 received payout.† † †Information available for 782 households: 607 with no payout and 175 received payout. ††††Although time-invariant, the information is only available from survey round 2 onwards. ‡Information available for 621 households: 468 with no payout and 153 received payout. ‡‡Information available for 754 households: 587 with no payout and 167 received payout. ‡ ‡ ‡Information available for 781 households: 605 with no payout and 176 received payout. *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. *Recently Migrated* is a dummy that captures whether the household migrated in the year prior to the baseline. *Income* captures households' income in the season prior to the sales period. *Total TLUs* capture total tropical livestock unit (TLU) herded by the household in the year prior to the first sales period. *Baseline average Demand of others* and *Baseline average Knowledge Scores for others* capture baseline sales period specific average demand and knowledge of other households from the same index-area, respectively.

their most critical source of disaster (95.5%) compared to the group that did not (89.8%), with a combined average of 91%. Thus, the product is more *relevant* to the group that received a payout. This is probably the reason they are more likely to purchase insurance that leads to a payout. On the other hand, the group that received no payout is more likely to have migrated in the year before the baseline (72.5%) compared to the group that received a payout (55.7%), with a combined migration rate of 68.8%. If migration is a proxy for socioeconomic stability, then we would expect the households that did not migrate to be more stable and thus more likely to purchase insurance leading to a payout. Thus, this statistically significant difference is not surprising. Finally, the last row of Panel B shows that both groups were equally likely to be selected (around 30%) for participating in the one-time knowledge game.

The first two rows of Panel C in Table 1 show that the group that received the payout was more likely to purchase the insurance at the baseline compared to the group that did not, with no significant difference in the number of TLUs insured conditional on the purchase. This result is similar to that of the first two rows in Panel A. As the following three rows show, there is no significant difference between these two groups in baseline knowledge score (around 47%), income (around 10500 Kenyan shillings), and the total number of TLUs (around 29). However, there are differences in network effects for these two groups, as presented in the last two rows of the table. In the baseline, households from the same index area were more likely to purchase the product and perform better in the knowledge questions for the group that received the payout later on. Thus there is a possibility of significant differences in network effects for other sales periods between these two groups, which needs to be controlled for in the regressions.

4 Empirical Analysis

In this section, I first focus on the impact of exogenous discount interventions on the households' demand for the product and their performances in the knowledge questions. In particular, I pay attention to the interaction of these interventions with the households' payout and disaster experience. This exercise helps me understand how different experiences may differently shape the effect of discounts on my outcome variables of interest. Next, I focus on analyzing the impact of payout experience on the outcome variables. 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.

4.1 Assessing the Impact of Exogenous Discount Interventions

I use the following set of three regression specifications to understand the impact of exogenous discount interventions on the outcome variables and the interaction of these interventions with households' experience. The first one 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:

$$Demand_{ijt} = \begin{cases} 1 \; if \; Demand^*_{ijt} = \gamma^D_0 + \gamma^D_1 Payout_{ijt} + \gamma^D_2 DE_{ijt} + \gamma^D_3 d_{ijt} \\ \quad + \gamma^D_4 Payout_{ijt} \times d_{ijt} + \gamma^D_5 DE_{ijt} \times d_{ijt} + \gamma^D_6 X_{ijt} + u^D_{ijt} > 0 \\ 0 \;\; otherwise, \end{cases} \tag{5}$$

where 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 reported losing any livestock due to drought anytime within the year before sales period t; d_{ijt} captures whether the household received sales period specific discount coupon; X_{ijt} controls for both time-invariant and time-varying household characteristics.

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:

$$TLUI_{ijt} = \begin{cases} TLUI^*_{ijt} \; if \; TLUI^*_{ijt} = \gamma^I_0 + \gamma^I_1 Payout_{ijt} + \gamma^I_2 DE_{ijt} + \gamma^I_3 d_{ijt} \\ \quad\quad + \gamma^I_4 Payout_{ijt} \times d_{ijt} + \gamma^I_5 DE_{ijt} \times d_{ijt} + \gamma^I_6 X_{ijt} + u^I_{ijt} > 0 \\ 0 \quad\quad otherwise, \end{cases} \tag{6}$$

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.

Finally, the following Ordinary Least Square (OLS) regression specification (7) captures the effect on the household's performance in the knowledge questions $Knowledge_{ijt}$:

$$\begin{aligned} Knowledge_{ijt} = & \gamma^K_0 + \gamma^K_1 Payout_{ijt} + \gamma^K_2 DE_{ijt} + \gamma^K_3 d_{ijt} + \gamma^K_4 Payout_{ijt} \times d_{ijt} \\ & + \gamma^K_5 DE_{ijt} \times d_{ijt} + \gamma^K_6 X_{ijt} + u^K_{ijt}. \end{aligned} \tag{7}$$

All the error terms in the above three specifications (u^D_{ijt}, u^I_{ijt}, and u^K_{ijt}) include index-area fixed effects, sales period fixed effects, and a random error.

Table 2 reports the associated results. 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 sales period-specific discount interventions, we can causally interpret the coefficients of d_{ijt} and its interactions with $Payout_{ijt}$ and DE_{ijt}. There are two columns of results per dependent variable. The first column presents the results without controlling for other household characteristics, and the second column controls for these characteristics (both time-invariant and time-varying).

Columns (1)-(2) report the results for the dependent variable $Demand_{ijt}$: the extensive margin of demand. Unsurprisingly, receiving a discount coupon leads to a highly significant increase in demand by 6.2-7.1%. This is a 26-29% increase from the baseline mean of 24.2%. In terms of the baseline standard deviation of 0.429, this is an increase of 0.1-0.2 standard deviations.

Table 2: Interaction of Payout and Disaster Experience with Exogenous Interventions

	Outcomes					
	Demand		TLU Insured		Knowledge	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Payout	0.242***	0.237***	6.962***	6.580***	0.011	0.022
(=$Payout_{ijt}$)	(0.066)	(0.063)	(2.029)	(1.897)	(0.041)	(0.043)
Disaster Experience	-0.061***	-0.073***	-2.558**	-2.971**	-0.001	0.004
(=DE_{ijt})	(0.021)	(0.022)	(1.068)	(1.154)	(0.025)	(0.024)
Received Discount	0.071***	0.062***	3.777***	3.357**	0.017	0.021
(=d_{ijt})	(0.015)	(0.016)	(1.423)	(1.412)	(0.021)	(0.021)
$Payout_{ijt} \times d_{ijt}$	-0.055***	-0.062***	-3.760*	-4.042**	0.032	0.004
	(0.018)	(0.017)	(1.933)	(1.980)	(0.047)	(0.047)
$DE_{ijt} \times d_{ijt}$	0.060**	0.079***	1.970**	2.437**	-0.025	-0.032
	(0.026)	(0.029)	(1.004)	(1.039)	(0.027)	(0.027)
Baseline Mean†	0.242	0.242	2.872	2.872	0.482	0.482
(SD)	(0.429)	(0.429)	(3.238)	(3.238)	(0.330)	(0.330)
Household Characteristics	No	Yes	No	Yes	No	Yes
Observations	4704	3937	4695	3928	4165	3937
pseudo R^2	0.147	0.192	0.073	0.088		
R^2					0.055	0.121

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 use survey weights at the baseline. †For the variable $TLUInsured$, only positive values are considered in the calculation of the mean and SD. All regressions include a constant term, sales period specific fixed effects, and index-area fixed effects. Household characteristics include whether the household is already *Covered* by an overlapping insurance contract; *Total Tropical Livestock Units* herded in the year prior to the sales period; *Income* in the season prior to the sales period; sales period specific *average demand and knowledge score of other households* from the same index-area; *Age*, *Gender*, and *Years of Education* of the household head at the baseline; *Assets Index* calculated at the baseline; *Extreme and Moderate Risk Aversion* dummies (with *Risk Neutral* being the omitted category) calculated at the baseline; whether *Main Income Source* of the household is related to livestock 5 years prior to the baseline survey; whether drought is ranked to be the *Most Critical Disaster* by the household in the baseline, for their major livestock loss; whether the household recently migrated in the baseline; and whether the household received the one-time knowledge treatment.

Receiving a payout significantly weakens the effect of the discount on demand. For those that received a payout, receiving a discount coupon leads to an increase in demand by only around 1-2%. On the other hand, experiencing a disaster significantly strengthens the effect of the discount on demand to around 13-14%. Columns (3)-(4) report the results for the dependent variable $TLUI_{ijt}$: the intensive margin of demand. The effects have a similar direction as that of the extensive margin. Receiving a discount coupon leads to a highly significant increase in the number of TLUs insured by 3.4-3.8 units. This is an 116.9-131.5% increase concerning the baseline mean of 2.872 units and a 1-1.2 standard deviation increase concerning the baseline standard deviation of 3.238 units. Receiving a payout significantly weakens, and experiencing a disaster significantly strengthens, the effect of the discount on the number of TLUs insured. Finally, columns (5)-(6) of the table report the results for the dependent variable $Knowledge_{ijt}$. No significant impacts can be observed for this variable, with the effect sizes being small.

These results show that experiencing a payout leads to a decrease in the positive effect of discount on demand, and experiencing a disaster leads to an increase in the positive effect of discount on demand. The findings hold for both extensive and intensive margins of demand. They suggest the negative impact of payout experience and the positive impact of disaster experience on demand. The latter provides some support in favor of my theoretical prediction that experiencing a disaster increases the relevance (and thus demand) for the product. More investigation is required for the former. Through the lens of my theoretical model, I expect at least part of this negative effect of payout experience to be driven by the optimistic households updating their belief regarding the product. This is something I investigate in the following sub-section in more detail.

4.2 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.[25] The first subsection of this section focuses on discussing the estimation strategy in detail and presents the associated results. The last subsection focuses on identifying the mechanisms behind the effects. In doing so, I discuss the associated triple-difference strategy and results.

4.2.1 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.

Ideally, I would like to use the following regression specification:

$$Outcome_{ijt} = \alpha_0 + \alpha_1 Payout_{ij} + \alpha_2 Post_t + \alpha_3 Payout_{ij} \times Post_t + \lambda X_{ijt} + \nu_{ijt}, \tag{8}$$

where $Payout_{ij}$ is a dummy that takes 1 for households that receives a payout at least once; $Post_t$ dummy takes 1 after the payout; X_{ijt} includes other characteristics; ν_{ijt} includes index

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

area fixed effects along with a random error term. Here the outcome variables of interest are the demand (both extensive and intensive margin) and the knowledge scores. The coefficient of interest α_3 captures the after-payout effect on those that received a payout, using those that did not receive a payout as the comparison group.

As usual, the differences-in-differences strategy relies on the parallel trend assumption. Although I can control for observable differences between the two groups by including time-invariant and time-varying characteristics in X_{ijt}, the unobservable differences cannot be controlled for in the regression. However, as long as these differences remain constant over time, they do not impose any threat to the identification strategy. As I have panel data, instead of controlling for household-specific time-invariant observable characteristics, I can include household fixed effects. These fixed effects control for both observable and unobservable household-specific time-invariant characteristics. As these fixed effects are perfectly collinear with the time-invariant covariates, regression equation (8) needs to be augmented accordingly. So, instead of using the regression specification (8), I use the following specification:

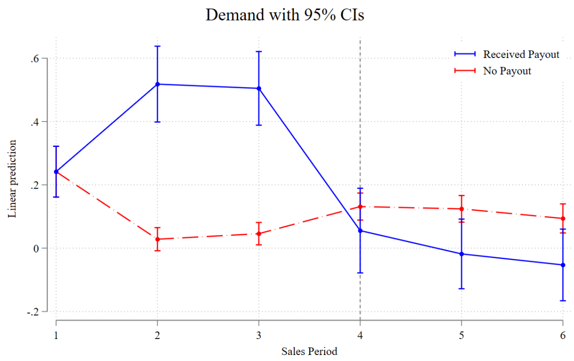
$$Outcome_{ijt} = \alpha_0 + \alpha_2 Post_t + \alpha_3 Payout_{ij} \times Post_t + \lambda X_{ijt} + \sigma_i + \nu_{ijt}, \tag{9}$$

where X_{ijt} includes time-variant characteristics, δ_i is household fixed effects, and ν_{ijt} is a random error term.

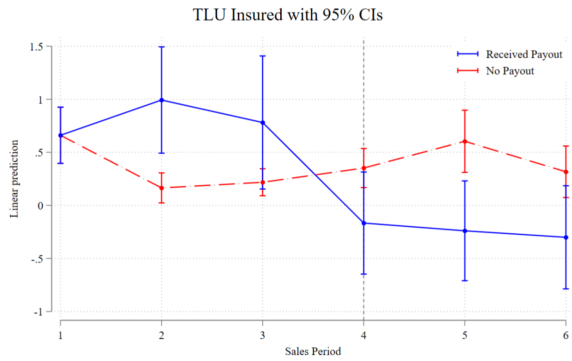
Note that the regression specification (9) does not control for time-varying unobservable characteristics. To understand whether the differences-in-differences estimator is unbiased, or the direction and extent of bias, if any, I turn to Figure 3. The figure reports my outcome variables of interest over time, after partialling out the effects of household fixed effects and time-varying covariates. The insurance company made indemnity payouts between sales periods 3 and 4. So, the first three sales periods are before the payout, while the last three are after the payout.

Four interesting observations can be made regarding the figures for the extensive and intensive margins of demand. First, we can observe a trend reversal between the two groups following the payout. Second, the demand for the group that did not receive a payout remains relatively more stable over time, compared to the group that did. Third, the parallel trend assumption is violated for the trend between sales periods 1 and 2, as we can observe differences in trends between the two groups for this period. These differences must be driven by time-varying unobservable characteristics. The question is: how do these characteristics bias the differences-in-differences estimates? On the other hand, the trend between sales periods 2 and 3 seems more parallel. Unfortunately, I do not have more pre-payout periods to verify the parallel trend assumption over a longer period. Finally, if the pre-payout differences in trend between the two groups are driven by time-varying unobservable characteristics, these characteristics are driving the demand for the group receiving the payout higher than the group not receiving the payout. Thus, if not controlled for, these unobservable time-varying characteristics should have a positive bias on the coefficient α_3 in regression (9). Thus, we can interpret the negative differences-in-differences coefficients (result below) as lower bounds in the absolute value.

Panel A: Extensive Margin of Demand



Panel B: Intensive Margin of Demand



Panel C: Knowledge Scores

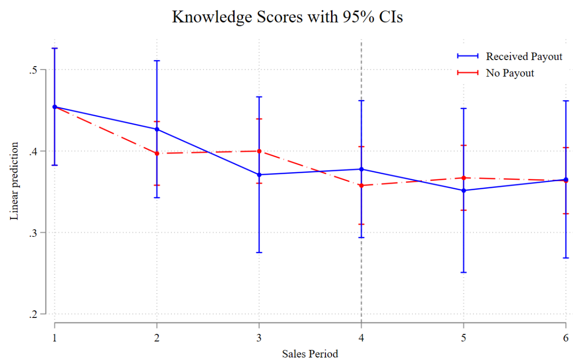


Figure 3: Outcome variables by payout status over time

In Panel C of Figure 3, I focus on the households' performances in the knowledge questions over time, after partialling out the effects of household fixed effects and time-varying covariates. For this variable, there doesn't seem to be much difference in levels and trends between the two groups, before or after the payout. This is reflected in the small insignificant differences-in-differences coefficient for this outcome variable (result below).

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 sales period fixed effects instead. All results are restricted to the observations from the sales periods 2 to 6. This is because, in the following subsection, I use the baseline demand as a proxy for baseline perception in a triple differences specification. So, the first sales period has to be dropped from the regressions to avoid having the same variable as a dependent variable in the regressions. I restrict the difference-in-differences results to the same sample, to make it comparable with the triple differences results.

The results for $Demand_{ijt}$ are in columns (1) and (2). Receiving the payout decreases demand by around 62% for the group that received the payout compared to the group that never did. The result is statistically significant at a 1% level. Compared to the pre-payout mean of the dependent variable, this is around a 340% decrease. It is also a 1.6 standard deviation decrease compared to the pre-payout standard deviation of the dependent variable.

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

	Outcomes					
	Demand		TLU Insured		Knowledge	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Post Payout	0.143***		0.211**		-0.022	
	(0.021)		(0.093)		(0.024)	
Received Payout × Post Payout	-0.616***	-0.619***	-1.448***	-1.458***	-0.017	-0.015
	(0.039)	(0.038)	(0.244)	(0.254)	(0.046)	(0.046)
Pre Payout Mean Dep. Var.†	0.183	0.183	2.492	2.492	0.406	0.406
(SD)	(0.387)	(0.387)	(2.936)	(2.936)	(0.316)	(0.316)
Sales Period Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	3354	3354	3352	3352	3354	3354
R^2	0.351	0.353	0.037	0.039	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. All regressions use survey weights at the baseline. † For the variable $TLUInsured$, only positive values are considered in the calculation of the mean and SD. All regressions include a constant term, household fixed effects and time-varying household characteristics. Time-varying household characteristics include whether the household received period-specific *Discount* coupon; whether the household lost any livestock due to drought anytime during a period of one year prior to the sales period (their *Disaster Experience*); whether the household is already *Covered* by an overlapping insurance contract; *Total TLUs* herded in the year prior to the sales period; *Income* in the season prior to the sales period; sales period specific *Average Demand and Knowledge of Other Households* from the same index-area.

As presented in columns (3)-(4), receiving the payout decreases the number of TLU insured by around 1.4-1.5 units for the group that received the payout. This is around a 58-59% decrease compared to the pre-payout dependent variable mean, and 0.5 standard deviations decrease compared to the pre-payout dependent variable SD. The result is significant at the 1% level. Knowledge scores for the group that received the payout decreased by around 1.5-1.7% due to the payout. These results are in columns (5) and (6). Compared to the pre-payout dependent variable means, this is a decrease of around 4%. It is also a decrease of 0.04-0.05 standard deviations compared to the pre-payout dependent variable SD. The result is statistically insignificant.

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 performing worse in the knowledge questions, although the effect is not statistically significant. There can be different possible mechanisms driving these results. For example, the households receiving a payout may use that money to buy other goods and services, decreasing their demand for index insurance. Similarly, if the households treat the insurance product as a gamble, they may be less likely to purchase it again after it pays off one time. 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.

I should also note that the above results (for the intensive and extensive margins of demand) rely on the assumption of a parallel trend. Although I argue that the deviation from the trend, as observed in Figure 3, leads to the coefficients in Table 3 being lower bounds in terms of the absolute value, it is a good idea to check the robustness of these results concerning alternate specifications. In Appendix D, I present the robustness of my results concerning two such alternate specifications: inverse propensity score weighting similar to Alem and Broussard (2017), and synthetic differences-in-differences following the methodology proposed by Arkhangelsky et al. (2021).

4.2.2 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.[26] Only 42% of the group receiving payout purchased the product in the baseline. Similarly, the demand was 25% for the comparison group that never received the payout. This 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 regression specification:

$$\begin{aligned} Outcome_{ijt} =& \beta_0 + \beta_1 Post_t + \beta_2 Payout_{ij} \times Post_t + \beta_3 Post_t \times Perception_{ij} \\ &+ \beta_4 Payout_{ij} \times Post_t \times Perception_{ij} + \phi X_{ijt} + \sigma_i + \epsilon_{ijt}, \end{aligned} \tag{10}$$

where $Perception_{ij}$ is equal to 1 if the baseline demand is 1, and 0 if the baseline demand is 0; ϵ_{ijt} is a random error term. Here the coefficient of interest is β_4. The triple-differences identification needs a weaker identifying assumption than the differences-in-differences strategy discussed in the last subsection (Olden and Møen, 2022). In particular, I just need to assume that the households with different baseline demand react similarly for changes in unobserved differences over time between the group that received the payout and the group that did not.

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

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 results, and the second column presents the results for a slight variation in regression specification that drops the $Post_t$ dummy to include the sales period fixed effects. The results are also restricted to the observations from the sales periods 2 to 6, to avoid having baseline demand as a dependent variable in the regressions.

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

	Outcomes					
	Demand		TLU Insured		Knowledge	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Post Payout	0.128***		0.083		-0.023	
	(0.022)		(0.168)		(0.026)	
Received Payout × Post Payout	-0.568***	-0.571***	-1.355***	-1.362***	0.002	0.004
	(0.043)	(0.042)	(0.205)	(0.211)	(0.058)	(0.059)
Post Payout × Baseline Demand	0.068**	0.068**	0.524	0.524	0.007	0.007
	(0.030)	(0.030)	(0.431)	(0.431)	(0.060)	(0.060)
Received Payout × Post Payout× Baseline Demand	-0.147*	-0.145*	-0.485	-0.489	-0.045	-0.046
	(0.082)	(0.082)	(0.478)	(0.481)	(0.098)	(0.098)
Pre Payout Mean Dep. Var.†	0.183	0.183	2.492	2.492	0.406	0.406
(SD)	(0.387)	(0.387)	(2.936)	(2.936)	(0.316)	(0.316)
Sales Period Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	3354	3354	3352	3352	3354	3354
R^2	0.355	0.357	0.039	0.041	0.066	0.066

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors clustered at the household level are in parentheses. All regressions use survey weights at the baseline. † For the variable $TLUInsured$, only positive values are considered in the calculation of the mean and SD. All regressions include a constant term, household fixed effects and time-varying household characteristics. Time-varying household characteristics include whether the household received period-specific *Discount* coupon; whether the household lost any livestock due to drought anytime during a period of one year prior to the sales period (their *Disaster Experience*); whether the household is already *Covered* by an overlapping insurance contract; *Total TLUs* herded in the year prior to the sales period; *Income* in the season prior to the sales period; 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 14.5-14.7% decrease in demand. The result is statistically significant at 10% level. This is around an 80% decrease, compared to the pre-payout dependent variable mean. In comparison to the pre-payout dependent variable SD, this is a decrease of 0.4 standard deviations. In terms of the number of TLU insured, receiving the payout decreases the number of TLU insured by around 0.5 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 19-20% decrease compared to the pre-payout dependent variable, and around 0.2 standard deviations decrease compared to the pre-payout dependent variable SD. The result is statistically insignificant.

For the knowledge scores, receiving the payout leads to a decrease of around 4.5% 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 pre-payout dependent variable mean, this is a decrease of around 11%. It is also a decrease of around 0.1 standard deviations compared to the pre-payout dependent variable SD. The result is also 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 significantly true only for the extensive margin of demand. I should note that I find stronger results using the inverse propensity score weighting, instead of the survey weights. These results are in the Appendix D. However, we need to be cautious while interpreting the results that use inverse propensity score weighting as they rely on the propensity score estimator being correctly specified (Sant'Anna and Zhao, 2020).

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 the positive impacts of disaster experiences on demand and knowledge for the product, ceteris paribus.

The empirical results suggest the negative effect of payout experience and the positive effect of disaster experience in shaping the relationship between exogenous discount interventions and (extensive and intensive margins) of demand. I further focus on identifying the impact of the 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. 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 extensive margin of demand, 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.

The empirical results also suggest 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.[27] These results are similar to the findings in Cai et al. (2020).

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

However, in their study, the payouts improve demand. These findings suggest that discount interventions can enhance households' learning from experience. However, contrary to the common belief, such learning may lead to a decrease in demand.

In this study, I focus on an index-insurance product insuring assets. However, I should note that the majority of the index insurance products in low-income countries insure stochastic income streams instead (Chantarat et al., 2012; Boyd and Bellemare, 2022). So, the findings of this study can not be generalized to the majority of index products currently available in the market. We need further research to understand the mechanism through which experience affects the learning for such products, which is beyond the scope of this study.

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Appendices

A IBLI Kenya: Details on the Interventions

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.

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 A.5: 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 A.5 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.

Table A.6: 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)
Recently Migrated	0.731	0.698	0.771	0.595	0.695
	(0.444)	(0.460)	(0.423)	(0.492)	(0.461)
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. *Recently Migrated* is a dummy that captures whether the household migrated in the year prior to the baseline

Table A.6 describes the baseline summary statistics by different treatment groups.[28] The first row of table A.6 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 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. 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%. 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 TLUs on average. Also, 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. Finally, around 70% of the households migrated in the year prior to the baseline.

Figure 4 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 5 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 average knowledge scores by treatment groups for these sales periods.

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

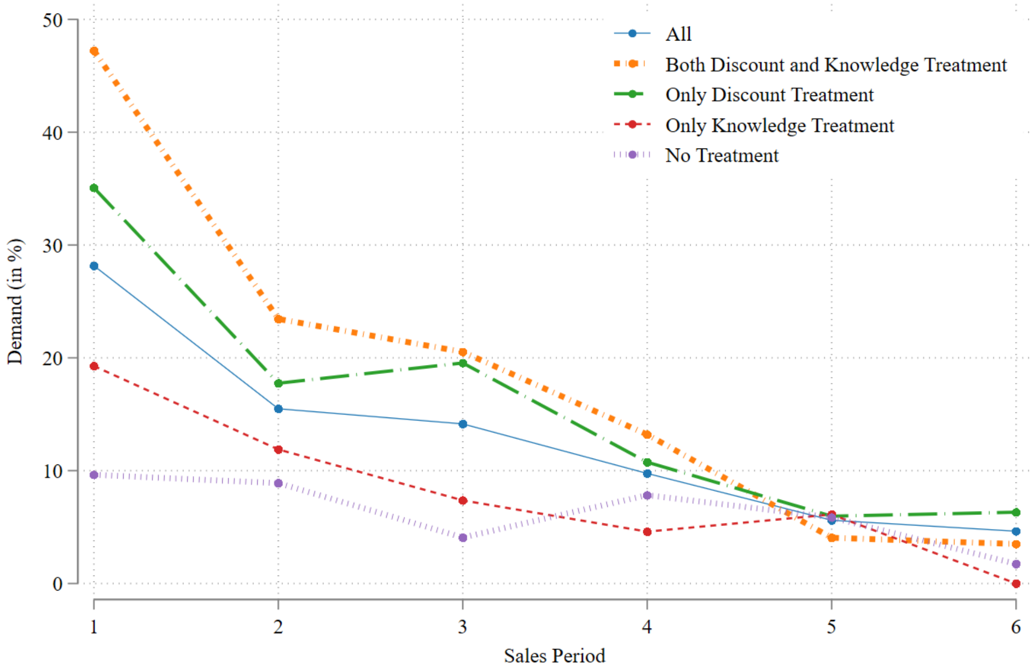

Figure 4: Demand Over Sales Periods by Treatment Groups

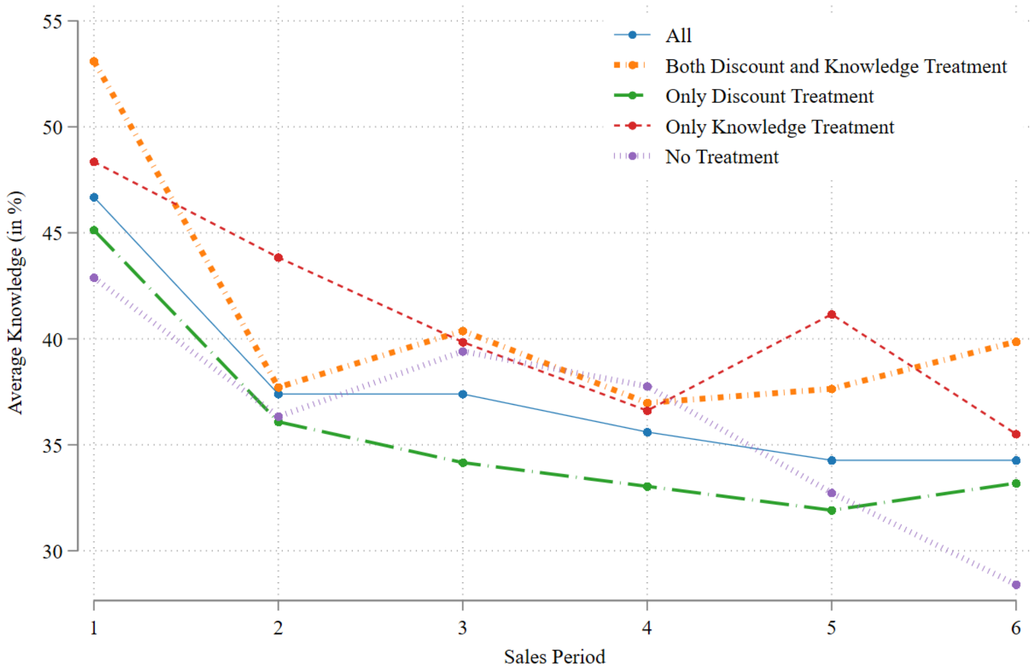

Figure 5: Performance in Knowledge Questions Over Sales Periods by Treatment Groups

These figure also fail to explicitly capture the change in the composition of treatment groups across six sales periods as demonstrated by Figure A.5. The empirical analysis need to control for these factors.

B 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 the outcome variables 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 sales period 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 offer 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 the outcome variables on $Payout_{ijt}$, focus on the heterogeneity analysis of payout experience in Table B.7. To keep it comparable with Table 2, I keep the specifications same as (5), (6), and (7). Columns (1)-(3) presents the results for the dependent variable $Demand_{ijt}$. Column (1) is repeating the results from column (2) of Table 2. Column (2) reports the results without the sales period fixed effects. Thus, this column does not control for part of the bias from the within-group comparison discussed above. The coefficient of $Payout_{ijt}$ in column (2) is similar to that of column (1), suggesting that the sales period fixed effects do not make much difference for 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 significant at a 1% level. The 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 $TLUI_{ijt}$. Similar to

$Demand_{ijt}$, between-group comparison creates an upward bias in the coefficient and the sales period fixed effects do not make a lot of difference. Finally, Columns (4)-(6) present the results for the dependent variable $Knowledge_{ijt}$. Similar to Table 2, I do not observe any significant impacts for this variable, with the effect sizes being small.

Table B.7: Heterogeneity Analysis of Payout Experience: Full Sample vs. Restricted Sample

	Outcomes								
		Demand			TLU Insured			Knowledge	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Payout	0.237***	0.220***	-0.319***	6.580***	5.808***	-2.484**	0.022	0.019	-0.043
(= $Payout_{ijt}$)	(0.063)	(0.063)	(0.104)	(1.897)	(1.804)	(1.069)	(0.043)	(0.042)	(0.064)
Disaster Experience	-0.073***	-0.089***	-0.195***	-2.971**	-3.346***	-1.975**	0.004	0.005	-0.058
(= DE_{ijt})	(0.022)	(0.025)	(0.070)	(1.154)	(1.257)	(0.820)	(0.024)	(0.023)	(0.046)
Discount Treatment	0.062***	0.064***	0.196***	3.357**	3.060**	1.814***	0.021	0.021	-0.030
(= d_{ijt})	(0.016)	(0.017)	(0.070)	(1.412)	(1.297)	(0.620)	(0.021)	(0.020)	(0.050)
$Payout_{ijt} \times d_{ijt}$	-0.062***	-0.072***	-0.261***	-4.042**	-4.234**	-2.604***	0.004	0.002	0.056
	(0.017)	(0.019)	(0.081)	(1.980)	(2.073)	(0.996)	(0.047)	(0.047)	(0.066)
$DE_{ijt} \times d_{ijt}$	0.079***	0.090***	0.075	2.437**	2.536**	0.863	-0.032	-0.032	-0.019
	(0.029)	(0.031)	(0.078)	(1.039)	(1.042)	(0.716)	(0.027)	(0.027)	(0.062)
Sales Period Fixed Effects	Yes	No	No	Yes	No	No	Yes	No	No
Restricted Sample	No	No	Yes	No	No	Yes	No	No	Yes
Observations	3937	3937	953	3928	3928	952	3937	3937	953
pseudo R^2	0.192	0.162	0.282	0.088	0.074	0.117			
R^2							0.121	0.119	0.110

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 use survey weights at the baseline. 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, and household characteristics. Household characteristics include whether the household is already *Covered* by an overlapping insurance contract; *Total Tropical Livestock Units* herded in the year prior to the sales period; *Income* in the season prior to the sales period; sales period specific *average demand and knowledge score of other households* from the same index-area; *Age*, *Gender*, and *Years of Education* of the household head at the baseline; *Assets Index* calculated at the baseline; *Extreme and Moderate Risk Aversion* dummies (with *Risk Neutral* being the omitted category) calculated at the baseline; whether *Main Income Source* of the household is related to livestock 5 years prior to the baseline survey; whether drought is ranked to be the *Most Critical Disaster* by the household in the baseline, for their major livestock loss; whether the household recently migrated in the baseline; and whether the household received the one-time knowledge treatment.

C 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.

D Robustness Checks

Table D.8: Robustness of Differences-in-Differences Estimates (w.r.t inverse propensity score weighting)

	Outcomes					
	Demand		TLU Insured		Knowledge	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Post Payout	0.173***		0.151		0.017	
	(0.030)		(0.163)		(0.028)	
Received Payout $\times$ Post Payout	-0.642***	-0.643***	-2.135***	-2.145***	-0.100**	-0.099**
	(0.034)	(0.034)	(0.381)	(0.400)	(0.040)	(0.040)
Pre Payout Mean Dep. Var.†	0.183	0.183	2.492	2.492	0.406	0.406
(SD)	(0.387)	(0.387)	(2.936)	(2.936)	(0.316)	(0.316)
Sales Period Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	3348	3348	3346	3346	3348	3348
R^2	0.369	0.373	0.058	0.061	0.080	0.081

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors clustered at the household level are in parentheses. All regressions use propensity score weights calculated using the data at the baseline. † For the variable $TLUInsured$, only positive values are considered in the calculation of the mean and SD. All regressions include a constant term, household fixed effects and time-varying household characteristics. Time-varying household characteristics include whether the household received period-specific *Discount* coupon; whether the household lost any livestock due to drought anytime during a period of one year prior to the sales period (their *Disaster Experience*); whether the household is already *Covered* by an overlapping insurance contract; *Total TLUs* herded in the year prior to the sales period; *Income* in the season prior to the sales period; sales period specific *Average Demand and Knowledge of Other Households* from the same index-area.

Table D.9: Robustness of Triple-Differences Estimates (w.r.t inverse propensity score weighting)

	Outcomes					
	Demand		TLU Insured		Knowledge	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Post Payout	0.181***		-0.039		-0.019	
	(0.035)		(0.391)		(0.029)	
Received Payout × Post Payout	-0.593***	-0.595***	-1.489***	-1.492***	-0.032	-0.030
	(0.043)	(0.042)	(0.190)	(0.198)	(0.045)	(0.045)
Post Payout × Baseline Demand	-0.003	-0.004	0.421	0.418	0.069	0.070
	(0.033)	(0.033)	(0.503)	(0.497)	(0.050)	(0.050)
Received Payout × Post Payout× Baseline Demand	-0.116*	-0.117*	-1.401**	-1.425**	-0.142*	-0.143*
	(0.069)	(0.069)	(0.625)	(0.640)	(0.079)	(0.079)
Pre Payout Mean Dep. Var.†	0.183	0.183	2.492	2.492	0.406	0.406
(SD)	(0.387)	(0.387)	(2.936)	(2.936)	(0.316)	(0.316)
Sales Period Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	3348	3348	3346	3346	3348	3348
R^2	0.372	0.376	0.062	0.065	0.085	0.086

Notes: * p<0.10, ** p<0.05, *** p<0.01. Robust standard errors clustered at the household level are in parentheses. All regressions use propensity score weights calculated using the data at the baseline. † For the variable $TLUInsured$, only positive values are considered in the calculation of the mean and SD. All regressions include a constant term, household fixed effects and time-varying household characteristics. Time-varying household characteristics include whether the household received period-specific *Discount* coupon; whether the household lost any livestock due to drought anytime during a period of one year prior to the sales period (their *Disaster Experience*); whether the household is already *Covered* by an overlapping insurance contract; *Total TLUs* herded in the year prior to the sales period; *Income* in the season prior to the sales period; sales period specific *Average Demand and Knowledge of Other Households* from the same index-area.

Table D.10: Synthetic Differences-in-Differences Estimates

	Outcomes					
	Demand		TLU Insured		Knowledge	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
ATT	-0.603***	-0.507***	-1.933***	-1.733***	-0.047*	-0.047
	(0.045)	(0.032)	(0.333)	(0.303)	(0.027)	(0.030)
Pre Payout Mean Dep. Var.†	0.183	0.183	2.492	2.492	0.406	0.406
(SD)	(0.387)	(0.387)	(2.936)	(2.936)	(0.316)	(0.316)
Time-varying characteristics	No	Yes	No	Yes	No	Yes
Observations	4704	4704	4650	4650	2472	2472

Notes: * p<0.10, ** p<0.05, *** p<0.01. Bootstrapped standard errors are in parentheses. † For the variable $TLUInsured$, only positive values are considered in the calculation of the mean and SD. All regressions include index-area fixed effects and time-invariant household characteristics. Time-invariant characteristics are interacted with the post payout dummy in the regressions. These characteristics include *Assets Index*, whether drought is ranked as the *Most Critical Disaster* by the household as reason for their major livestock loss, and whether the household *Recently Migrated* in the year prior to the baseline. Time-varying household characteristics include whether the household received period-specific *Discount* coupon; whether the household lost any livestock due to drought anytime during a period of one year prior to the sales period (their *Disaster Experience*); whether the household is already *Covered* by an overlapping insurance contract; sales period specific *Average Demand of Other Households* from the same index-area.