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

*Preliminary and Incomplete - Please do not Cite without Permission*

Aranya Chakraborty*

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**Abstract**

This research focuses on understanding the role of experience in learning about index insurance products. In particular, I study the effect of payout and disaster experience in shaping the demand and interest for index insurance products. I develop a theoretical model that formalizes the scenario where households are learning about the covariate risk they face, as well as the mapping of this covariate risk to the index insurance product that insures against it. The model predicts the impact of payout experience to be dependent on the households' level of optimism/ pessimism regarding the product design, and a positive effect of disaster experience on the demand and interest for the product. I test these predictions using data from Index-Based Livestock Insurance (IBLI), Kenya. My results show that receiving a payout decreases the extensive and intensive margin of demand for the group that received the payout, with the same group losing some interest in the product after the payout. Moreover, I find evidence that the effect of payout on the outcome variables can be explained by optimistic households updating their beliefs about the product design downward, following a payout. Additional analysis suggests that policy interventions on their own may not be able to help households overcome information frictions. However, interventions can be used as tools for enhancing households' learning from experience.

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

**Keywords:** Index Insurance, Learning, Agriculture, Kenya.

## 1 Introduction

In recent decades, index-based insurance has been advocated as a potential solution to the problem of missing insurance markets in subsistence agriculture (Carter et al., 2014).

*Department of Economics, McGill University; Canada. E-mail: aranya.chakraborty@mail.mcgill.ca.

The idea is to condition the insurance payoff on some objectively observed index, which is highly correlated with the individual-specific outcome but cannot be influenced by the actions taken by these individuals.[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., 2018; Janzen et al., 2020a), lack of financial knowledge (Patt et al., 2010; Cai et al., 2020), lack of trust in the insurer (Cole et al., 2013; Stern, 2019), and several behavioral factors (Elabed and Carter, 2015; Serfilippi et al., 2015; Belissa et al., 2020).[3] The literature also advocates learning from experience as a potential solution to the problem. However, the role of experience in learning for index insurance products has been under-explored in the literature.

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

I develop a theoretical model that formalizes the scenario where households are learning about the covariate risk they face, as well as the mapping of this covariate risk to the index insurance product that insures against it. I argue that experiencing disasters helps households learn about the covariate risk they face, without affecting their perception of the

[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, according to which index insurance payouts are made.

product design. On the other hand, having a payout experience leads them only to update their beliefs regarding the product design. This is the design concerning the mapping of their covariate risk to the index insurance product. The model predicts ambiguous impacts of receiving a payout on the demand and interest in the product. In particular, I find the impacts to be dependent on the households' level of optimism/ pessimism regarding the product design. Additionally, the model predicts positive impacts of disaster experiences on demand and interest for the product, ceteris paribus.

To test the predictions of my theoretical model, I use data from Index-Based Livestock Insurance (IBLI), Kenya. Exploring the effects in terms of randomized discount and knowledge interventions in correlational analysis help me in understanding the policy directions. To causally identify the effects of receiving a payout I use a differences-in-differences identification strategy. This is done after discussing the identification problem in detail, along with understanding the direction and source of bias in the correlation analysis. Additionally, I use a triple-differences identification strategy to understand the mechanism behind the causal effect of receiving a payout.

Correlational analysis suggests a significant relationship between payout experience and the demand for the product. Causal analysis shows that receiving a payout decreases the extensive and intensive margin of demand for the group that received the payout, with the same group losing some interest in the product after the payout. I find that the effect of payout on the outcome variables, at least in part, can be explained by optimistic households updating their beliefs about the product design downward, following a payout.

In terms of policy interventions, correlational analysis suggests that receiving a discount intervention mechanically increase demand (due to lower cost). However, it also increases households' chances of receiving a payout, which will lead them to optimally lower their demand. Similarly, while receiving a knowledge intervention mechanically increase households' knowledge regarding how the product works, it also helps them update their interest in the product downwards once a payout is received.

My study makes three contributions to the existing literature. First, I identify the causal effect of payout experience on the demand and interest 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 are also found to be increasing 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 interest for index insurance products. In particular, I focus on the role played by households' perceptions regarding the product design. Additionally, this study is also among the first that uses a differences-in-differences identification strategy to identify the causal effect of payout experience.

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

Finally, I provide policy directions on how to use interventions to improve learning from experience for index insurance products. The role of discount and knowledge interventions in improving demand for index insurance products is well recognized.[4] Discount interventions are heavily used to increase initial adoption. In fact, the demand for index insurance products are found to be highly price sensitive (Jensen and Barrett, 2016). Additionally, knowledge interventions are used as the tools of overcoming information frictions (Carter et al., 2014). However, relatively less attention has been paid to providing policy directions on how to use these interventions in improving learning from experience. To the best of my knowledge, Cai et al. (2020) is the only other study that explores 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 discuss the data and present descriptive statistics. Section 3 presents my theoretical framework and highlights the main hypotheses for this study. The first half of Section 4 focuses on presenting the results from correlation analysis and analyzing challenges in causal identification. In the second half of Section 4, I discuss my identification strategies and present associated results. Finally, Section 5 summarizes my findings and makes concluding remarks.

# 2 Data and Descriptives

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

## 2.1 Background

Index-Based Livestock Insurance (IBLI) was introduced as a pilot in the Marsabit District of Northern Kenya in 2010, and in the Borena Zone of Southern Ethiopia in 2012.[5] 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

[4] Examples can be found in Giné et al. (2013), Takahashi et al. (2016), and Cai et al. (2020).
[5] Details regarding the project can be found in `https://ibli.ilri.org/index/`.

livestock mortality.[6] For this study, the focus is on the Kenyan pilot, where the survey and implementations were conducted by the International Livestock Research Institute (ILRI), Cornell University, the BASIS Research Program at the University of California, Davis, and Syracuse University together with their implementing partners Equity Bank, UAP Insurance Company, APA Insurance Company, and Takaful Insurance of Africa.

Marsabit district was divided into five index regions for IBLI distribution.[7] The insurance was available to all households in these regions, who could self-select themselves into getting a contract. The region is characterized by its bi-modal rain pattern. The insurance was also designed to be offered twice every year, 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. This structure of overlapping contracts was intended to reduce credit and liquidity constraints of the households (Chantarat et al., 2012). Figure 1 demonstrates the bi-modal rain pattern observed in the region, IBLI sales periods, coverage periods, and the possibility of overlapping payout.

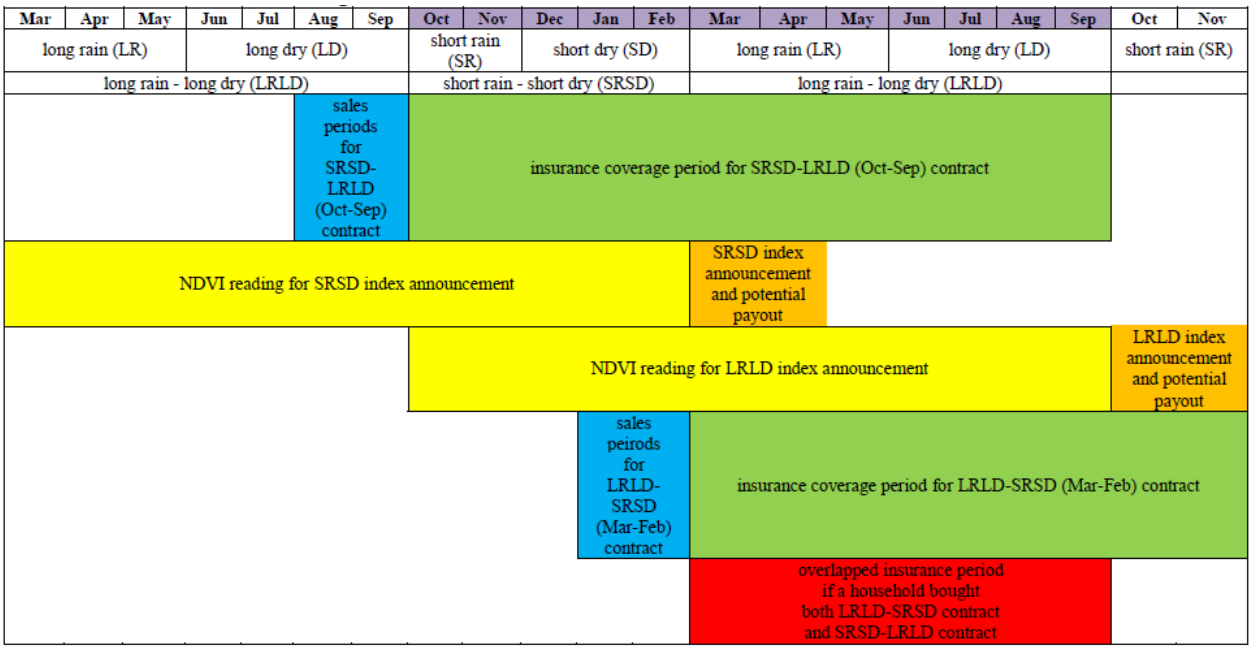


Figure 1: Time Structure of IBLI Marsabit (Source: Ikegami and Sheahan (2014))

The NDVI was used as the main input to calculate the area-average livestock mortality rate for each index region.[8] If the calculated area-average livestock mortality in an index

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

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

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

region was higher than a certain threshold, payouts were made to all households who were covered by the insurance in that region.[9] The total payout to a household depended on the amount of coverage bought by the household (household-specific) and the difference of the calculated livestock mortality from the threshold (index-area specific).

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 (shown in Figure 2).

## 2.2 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.[10] This resulted in a final sample of 924 households.

Figure 2 outlines the details regarding the timing of household survey rounds. The baseline survey was conducted in 2009, with annual follow-up rounds after the introduction of the product in 2010-2013. In 2015, two years after the survey round 5 in 2013, survey round 6 was conducted. For this study, I focus on the first 5 rounds of the household survey. The reason behind this is twofold. 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 huge drop in the associated demand. If included, this can highly bias my results. Additionally, the reference period hugely differs for survey round 6, compared to the past survey rounds. This can potentially lead to a reference period bias if I include survey round 6 for my analysis. The sample size of 924 households was originally intended to be

[9]This threshold was 15% for the first 5 sales periods. After that, consumers were given the option to choose between 10% and 15% threshold levels, with different associated premium rates.

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

| Date | Activity |
|---|---|
| October-November 2009 | Household survey round 1 |
| January-February 2010 | 1st IBLI sales period |
| October-November 2010 | Household survey round 2 |
| January-February 2011 | 2nd IBLI sales period |
| August-September 2011 | 3rd IBLI sales period |
| October-November 2011 | Household survey round 3 |
| October-November 2011 | 1st IBLI indemnity payout |
| March-April 2012 | 2nd IBLI indemnity payout |
| August-September 2012 | 4th IBLI sales period |
| October-November 2012 | Household survey round 4 |
| January-February 2013 | 5th IBLI sales period |
| August-September 2013 | 6th IBLI sales period |
| March-April 2013 | 3rd IBLI indemnity payout |
| October-November 2013 | Household survey round 5 |
| January-February 2014 | 7th IBLI sales period |
| March-April 2014 | 4th IBLI indemnity payout |
| August-September 2014 | 8th IBLI sales period |
| October-November 2014 | 5th IBLI indemnity payout |
| January-February 2015 | 9th IBLI sales period |
| March-April 2015 | 6th IBLI indemnity payout |
| August 2015 | 7th IBLI indemnity payout |
| August-September 2015 | 10th IBLI sales period |
| October-November 2015 | Household survey round 6 |

Figure 2: Timeline for IBLI Marsabit (Source: Ikegami and Sheahan (2014))

repeated in each round of the survey, to be able to construct a panel of these households. However, some households could not be traced down in later periods and, replacement households were found. For this study, my focus is on the balanced panel of 820 households who were successfully surveyed in all 5 rounds.[11]

The IBLI product was made available to all households in the Marsabit District. However, for impact evaluation purposes, exogenous knowledge and discount treatments were randomly distributed in the surveyed regions. The knowledge treatment was implemented in the form of an IBLI knowledge game that was randomized and implemented only once just before the first sales period. The discount treatment came in terms of discount coupons, which were randomized independently in each sales period and were only valid for the sales period it was distributed. Thus, during each sales period, households can be identified as being part of one of the four different treatment groups:

1. **Control Group:** The households that were not given the one-time knowledge

[11] An analysis of the related attrition can be found in Jensen et al. (2018).

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 1: Composition of Treatment Groups over Sales Periods

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

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

Table 1 describes the composition of these treatment groups across 6 sales periods for the balanced panel of 820 households. The indemnity payouts, as can be seen from Figure 2, occurred 3 times during the first 5 rounds of survey. However, since I do not consider the survey round 6, my focus is on the effects of first two indemnity payouts that took place in October-November, 2011 and March-April, 2012.

## 2.3 Descriptive Statistics

Table 2 describes the baseline summary statistics by different treatment groups. The survey collected information on households' insurance purchase decisions, which included not only their decision to buy/not buy the insurance but also the type and number of animals insured conditional on the purchase of the insurance. The first row of table 2 uses the dummy variable that recorded households' binary decision to buy/not buy the insurance, at the baseline. At the baseline, around 28% households purchased the product on average, with significant variations across treatment groups. Unsurprisingly, the demand is lower in the control group with only around 10% households purchasing the product. The demand is highest for the group that received both knowledge and period-specific discount treatments with around 47% households purchasing the product. 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 to be more uniform across treatment groups. This knowledge variable reflects households' knowledge regarding how the insurance product works and they are calculated based on households' answers to the knowledge questions asked in each survey rounds.[12] The average knowledge score seems to be around 47%, with only the group that receives both discount and knowledge treatment being significantly different than the control (53% vs. 43%). The lack of variation in the knowledge regarding how the product works, together with the significant variations in the demand for the product across treatment arms may indicate the absence of information frictions driving the demand down. But, the knowledge score is simply an indicator of households' interest in the product and is not reflective of their perception regarding the product. Thus, the knowledge score cannot be used to understand the presence (or lack of) information frictions.

The following three rows indicate that the average household in the survey has a household head aged around 48 years, who is 62% likely to be male, and completed around 1 year of education. The baseline asset index for the households also turned out to be

[12]Details regarding the knowledge questions can be found in Appendix A.

Table 2: Baseline Summary Statistics by Different Treatment Groups

| Variable | Control | Only Discount | Only Knowledge | Discount & Knowledge | Total |
|---|---|---|---|---|---|
| Demand for Index Insurance | 0.096 | 0.351 | 0.193 | 0.472 | 0.282 |
| | (0.296) | (0.478) | ( 0.397) | (0.501) | (0.450) |
| Knowledge of Index Insurance$^{\dagger}$ | 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 | 0.365 | 0.369 | 0.398 | 0.393 | 0.376 |
| (Female=1) | (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)$^{\ddagger}$ | 10.408 | 11.768 | 9.074 | 8.433 | 10.419 |
| | (19.137) | (26.307) | (16.029) | (17.529) | (21.746) |
| Main Income Source | 0.763 | 0.738 | 0.723 | 0.767 | 0.750 |
| (Livestock=1) | (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 | 0.944 | 0.917 | 0.819 | 0.902 | 0.912 |
| (Drought=1) | (0.231) | (0.276) | (0.387) | (0.298) | (0.283) |
| Observations | 249 | 325 | 83 | 163 | 820 |

*Notes:* $^{\dagger}$Available for 654 households: 164 control, 274 only discount, 71 only knowledge, and 145 discount & knowledge. $^{\ddagger}$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.

around 19%.[13] These characteristics vary little to none across treatment arms. The baseline household survey collected information on households' risk preferences using a Binswanger (1980) type of incentivized game. Using that information, I have calculated risk aversion dummies that are shown next (with risk-neutral being the omitted category). On average, around 27% households are extreme risk-averse and 44% are moderate risk-averse, with some variations across treatment groups. The households' income, reported in the next row, seems to vary around 10,000 Kenyan Shilling (KSH) per season. Around 75% households report livestock as being their main income source and own around 30 Tropical Livestock Units (TLUs) on average.[14] Finally, around 90% households reported drought being the most critical disaster for them. These highlights the importance of a livestock insurance product for the population, particularly one that focuses on drought-related livestock mortality. Thus, the IBLI should be a product in high demand for this region.

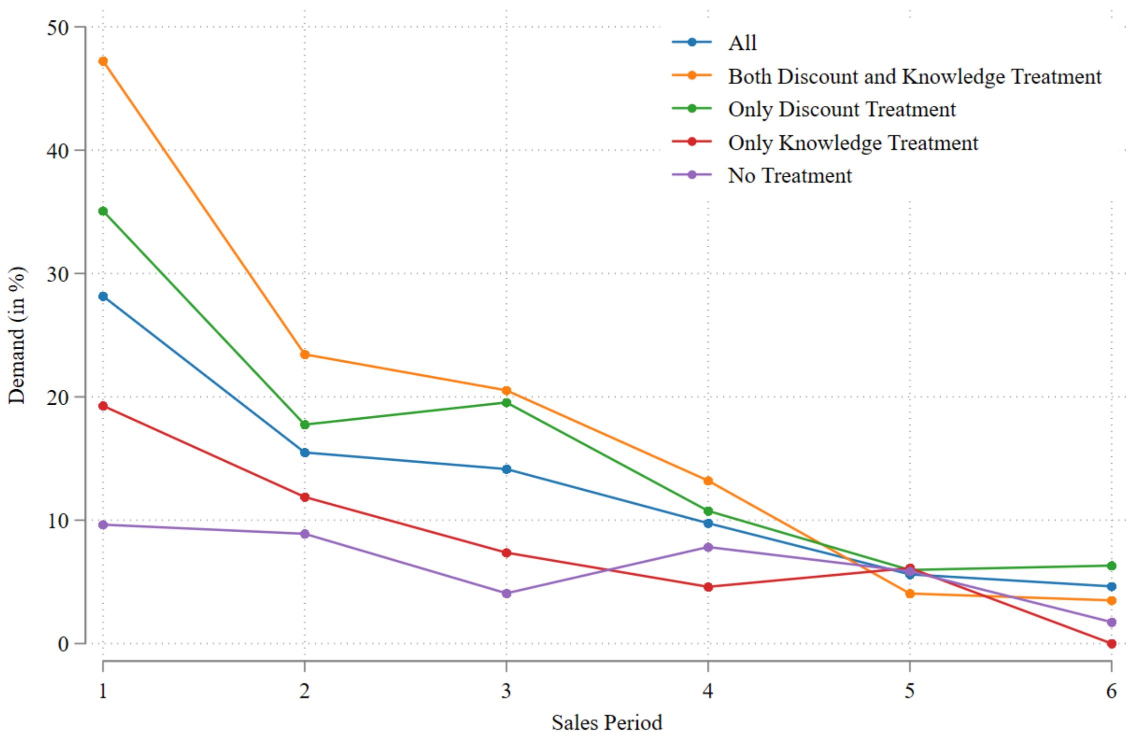


Figure 3: Demand Over Sales Periods by Treatment Groups

Figure 3 presents the demand for the product over the first 6 sales periods, across

[13]Details regarding the calculation can be found in the footnote of Table. 2.

[14]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").*"

treatment arms. As can be seen, the demand for all of the treatment groups was at its highest in the baseline with a steady decline over 6 sales periods. All treatment groups seemed to be converging 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 decreased their demand over time. However, it is important to keep in mind the overlapping structure of the contracts for the 3rd and 6th sales periods, whereas the households that purchased the product in the 2nd and 5th sales periods would be covered by the insurance product and thus less likely to purchase the product again. This figure also fails to explicitly capture the change in the composition of treatment groups across 6 periods as demonstrated by Figure 1.

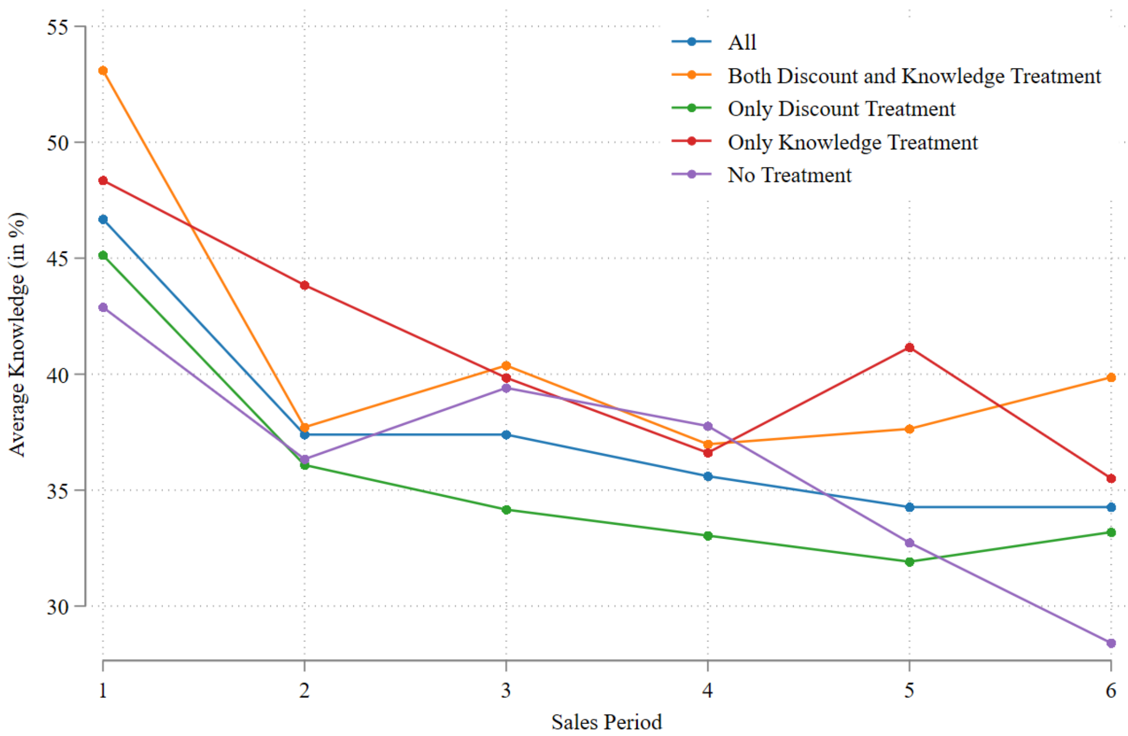


Figure 4: Performance in Knowledge Questions Over Sales Periods by Treatment Groups

Similarly, figure 4 presents the average knowledge for the product over the first 6 sales periods, across treatment arms. Similar to 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 of that, for the sales periods that are part of the same survey round (sales periods 2 and 3 for survey round 3 and sales periods 5 and 6 for survey round 5), the knowledge scores do not vary across sales periods. This is represented in the figure by the average knowledge of all households for these sales periods. The average knowledge score varies by treatment group for these sales periods though, due to the distribution of discount coupons by sales period leading to the change in the composition of treatment groups.

# 3 Theoretical Framework

In this section, I first present a theoretical model of index insurance following Janzen et al. (2020b). After presenting their basic framework, I relax some simplifying assumptions of the model to introduce the possibility of learning, which is discussed next.

## 3.1 Index Insurance without Learning

Consider household $i$ from index-area $j$ to have a asset holding $A_{ijt}$ at period $t$. The household decides between how much to consume at this period ($c_{ijt}$) and how much to save as assets for the next period ($A_{ijt+1}$). The household is credit constrained such that $c_{ijt} \leq A_{ijt} + f(A_{ijt})$ with $A_{ijt+1} \geq 0$, where $f(\cdot)$ is a fixed production function that does not change over time.[15] The household face two types of shocks: a covariate shock $\theta_{jt}$ that is common to all other households living in the same index-area as them, and an idiosyncratic shock $\epsilon_{ijt}$ 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 choose their consumption ($c_{ijt}$). After that they realize the composite shock $\mu_{ijt+1} := (\theta_{jt+1} + \epsilon_{ijt+1})$, which determines their next

[15] 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.

period's asset holding $A_{ijt+1} = (A_{ijt} + f(A_{ijt}) - c_{ijt})(1 - \mu_{ijt+1})$.[16] Thus, the household's optimization problem can be represented as:

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

where $u(\cdot)$ represents the household's period-specific utility function, with $\beta$ being the discount factor.

Now, suppose that there exists an index insurance product that insures the household against the covariate shock $\theta_{jt}$ but not the idiosyncratic shock $\epsilon_{ijt}$. 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 assumed to be fixed at $p$. For their purpose Janzen et al. (2020b) assume $i(\theta_{jt}) = \theta_{jt}$ and it is a common knowledge, which has three implications. First, for the insurer, 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.[17] Second, the consumers also believe the index to represent the covariate risk perfectly. More specifically, there is no deviation between the objective value of $i(\theta_{jt})$ and its subjective perception to the consumer. Finally, the basis

[16] 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 possibility can be important from the perspective of an insurer, which is beyond the scope of this paper.

[17] *Design risk* is represented by the prediction error of the index in capturing the covariate risk.

risk associated with the product, for both the insurer and insurees, is represented by the household specific idiosyncratic risk $\epsilon_{ijt}$. Under these assumptions, the household have perfect 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 next subsection I relax the assumption that the index perfectly observes the covariate risk. Additionally, I consider the scenario where the households are not fully informed about the correlation between the index and the covariate risk. This implies that they need to make decisions based on their beliefs regarding the correlation and can potentially learn about it over time. As a result, the subjective perception of the basis risk will be different for the households than its objective counterpart, absent complete learning.

## 3.2 Index Insurance with Learning

In this sub-section I relax the assumption that the index perfectly observe the covariate risk. To do so, I consider the index to be represented by $\iota(\theta_{jt})$ instead of $i(\theta_{jt})$, where $\iota(\theta_{jt}) = \gamma^* \theta_{jt} + \nu_{jt}$. The parameter $\gamma^* \in [0, 1]$ helps in mapping households' covariate risk $\theta_{jt}$ to the index $\iota(\theta_{jt})$, with $\nu_{jt}$ being the zero mean random error in mapping. In terms of the dataset, $\iota(\theta_{jt})$ can be interpreted as the predicted area-average livestock mortality. So, essentially, here I assume the predicted value of area-average livestock mortality to be a linear function of its actual value. The insurer does not observe $\theta_{jt}$, so makes the payout contingent on $\iota(\theta_{jt})$. Similar to the last sub-section, for $I_{ijt} > 0$ per-unit return $\delta'_{jt}$ 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 $\gamma^*$ 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 $\nu_{jt}$ in $\iota(\theta_{jt})$ stops them from learning the true value of $\gamma^*$ 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 $\gamma^*$ and their expectation on $\theta_{jt+1}$ determine their demand for period $(t + 1)$ through two different channels.

### 3.2.1 Payout Experience

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

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

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

**<u>Case 1:</u>** $\gamma_{ijt} < \gamma^*$. Since $\gamma^*$ 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 $\gamma^*$. This is because if $\gamma^* < \frac{s}{\theta_{jt+1}}$, then $\gamma_{ijt} < \frac{s}{\theta_{jt+1}}$ and the household learn nothing new about the product. As a consequence, the demand should remain lower than optimal. However, if the household receives a payout, they observe the per-unit return of $(\gamma^*\theta_{jt+1} + \nu_{jt+1} - s)$. Having observed $\theta_{jt+1}$ and knowing $s$, this helps them update their beliefs for $\gamma^*$ 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 $\nu$ and realize the true value of $\gamma^*$.

**<u>Case 2:</u>** $\gamma_{ijt} > \gamma^*$. Since $\gamma^*$ 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 $\gamma^*$ downwards if $\gamma_{ijt} > \frac{s}{\theta_{jt+1}}$. Similarly, receiving a payout helps them update their beliefs for $\gamma^*$ downwards. This will bring the demand closer to the optimal, for both scenarios.

Thus, receiving 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 $\gamma^*$ in the population. The households knowledge regarding how the

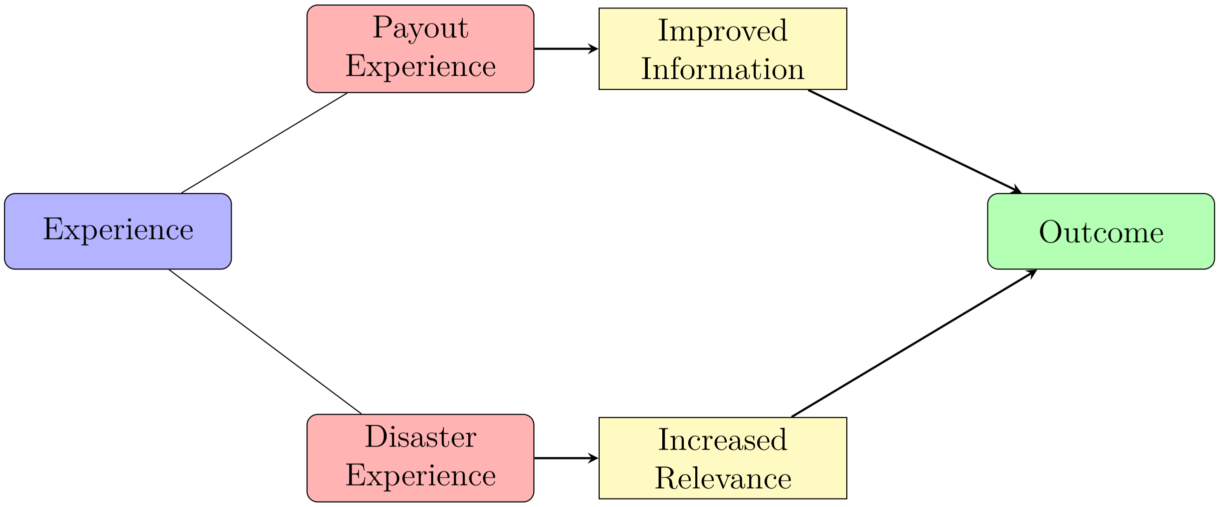

Figure 5: Effect of different experiences on final outcome

product works, indicative of their interest in the product, should be affected in the similar fashion. Thus, the effects need to be understood empirically for a given population. The upper half of Figure 5 presents this channel. Here payout experience leads to improved information about the product (in terms of the model, this translates to improved knowledge about $\gamma^*$). The effect of this increase in information on the demand and interest for the product is ambiguous. As explained above, the effect particularly depends on the proportions of the households over and under-estimating $\gamma^*$ prior to the payout experience. This 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 interest 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 decrease in demand and interest for the product.*

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

### 3.2.2 Disaster Experience

Let me now concentrate on the consequences of experiencing a disaster. In particular, here, I consider the disaster that is being insured for by the index-insurance product. In terms of the theoretical framework, this would mean observing high values of $\theta$. For the context of the dataset, this would mean experiencing high area-average livestock mortality. As described earlier, households only observe $\theta_{jt+1}$ after purchasing insurance for the period $t+1$. Thus, they need to make their insurance decisions based on expectations regarding $\theta_{jt+1}$. The realization of $\theta_{jt}$ matters for this purpose.

To understand this more concretely, consider the possibility that $\theta_{jt} \in \{\theta^H, \theta^L\}$. Here $\theta^H$ denotes a high value of covariate shock, and $\theta^L$ denotes a low value of covariate shock. Additionally, assume household $i$'s subjective belief of $\theta_{jt+1}$ 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 $\theta_{jt+1}$ is more likely to be $\theta^H$, if in the last period $\theta_{jt}$ was $\theta^H$. On the other hand, the same household believes $\theta_{jt+1} = \theta^L$ to be more likely, if in the last period $\theta_{jt}$ was $\theta^L$. This would impact the choice of $I_{ijt}$ in the optimization (4), by affecting the calculation of expected utility.

More specifically, high $\theta_{jt}$ will make the households perceive high $\theta_{jt+1}$ to be more likely, increasing their demand and interest for the insurance product. Similarly, low $\theta_{jt}$ will make the households perceive low $\theta_{jt+1}$ to be more likely, decreasing their demand and interest for the product. This is in line with the empirical findings of Cai and Song (2017) and Bjerge and Trifkovic (2018). It is important to note that this assumes households asset levels to remain same over periods $t$ and $t+1$. If the households lose their assets due to high $\theta_{jt}$, 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 5 presents this channel. Here disaster experience leads to increased relevance for the product, which leads to higher demand and interest for the product (ceteris paribus, through the expectation of $\theta$ in the model). This leads to my second hypothesis:

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

# 4 Empirical Analysis

In this section, I begin with the correlation analysis of payout and disaster experience. In doing so, I first present the correlation coefficients of payout and disaster experiences with the households' demand and interest for the product. Then, I discuss the potential challenges in identifying the causal effects and provide additional results to understand the direction of bias in the correlation coefficients. As the correlational analysis mostly shows support for the effect of payout experience, next I move to causally identify the effect of payout experience on the households' decision to purchase the product and their performances in the knowledge questions. In doing so, I discuss my identification strategy and present the associated results. Subsequently, I focus on understanding the mechanism driving the causal effect of the payout experience.

## 4.1 Correlation Analysis: Payout and Disaster Experience

### 4.1.1 Results

I start by presenting the correlation coefficients of payout and disaster experiences with the households' demand for the product as well as their performances in the knowledge questions. I use the following set of regression specifications for this purpose:

$$Demand_{ijt} = \begin{cases} 1 \; if \; Demand^{*}_{ijt} = \gamma_0^D + \gamma_1^D Payout_{ijt} + \gamma_2^D DE_{ijt} + \gamma_3^D T_{ijt} \\ \quad + \gamma_4^D Payout_{ijt} \times T_{ijt} + \gamma_5^D DE_{ijt} \times T_{ijt} + \gamma_6^D X_{ijt} + u_{ijt}^D > 0 \\ 0 \; otherwise, \end{cases} \tag{5}$$

$$
\begin{aligned}
Knowledge_{ijt} = & \gamma_0^K + \gamma_1^K Payout_{ijt} + \gamma_2^K DE_{ijt} + \gamma_3^K T_{ijt} + \gamma_4^K Payout_{ijt} \times T_{ijt} \\
& + \gamma_5^K DE_{ijt} \times T_{ijt} + \gamma_6^K X_{ijt} + u_{ijt}^K .
\end{aligned} \tag{6}
$$

Regression specification (5) is a probit regression for the dummy dependent variable $Demand_{ijt}$, which represents the binary decision to purchase the product for household $i$ of index-area $j$ at time $t$. Similarly, Ordinary Least Square (OLS) regression specification (6) captures the effect on the household's performance in the knowledge questions represented by the variable $Knowledge_{ijt}$. $T_{ijt} = (d_{ijt}, k_{ij})'$ is a $2 \times 1$ vector, where $d_{ijt}$ captures whether the household received sales period specific discount coupon and, $k_{ij}$ captures whether they received the one-time knowledge treatment. The dummy variable $Payout_{ijt}$ capture whether the household had a payout experience prior to the sales period $t$. $DE_{ijt}$ (DE stands for Disaster Experience) is a dummy variable that captures whether the household lost any livestock due to drought anytime during a period of one year prior to the sales period $t$. $X_{ijt}$ controls for both time-invariant and time-varying household characteristics. The error terms ($u_{ijt}^D$ and $u_{ijt}^K$) include index-area fixed effects, survey round fixed effects, and random error.

I discuss in the next subsection that the coefficients of $Payout_{ijt}$ and $DE_{ijt}$ cannot be interpreted causally. This is because of unobserved differences between the households that had these experiences and the households that did not. However, the coefficients of $T_{ijt}$ and its interactions with $Payout_{ijt}$ and $DE_{ijt}$ can be interpreted causally. This is because the treatments were randomly allocated.

Columns (1)-(3) of Table 3 reports the results for the dependent variable $Demand_{ijt}$. Overall, payout experience is highly positively correlated with demand with the coefficients being significant at 1% level. On the other hand, the correlation between disaster experience and demand is negative and mostly insignificant. Unsurprisingly, receiving a discount coupon leads to a highly significant increase in demand by 5.5-7.1%. This is a 57-74% increase from the control group baseline mean of 9.6%. In comparison with the control group baseline standard deviation of 0.296, this is an increase of 0.2 standard deviations.

Receiving a knowledge treatment, however, leads to no significant increase in demand. Receiving a discount coupon also leads to significantly lower correlation between payout experience and demand. No other significant heterogeneity can be observed in payout and disaster experiences for the exogenous interventions.

Table 3: Correlational Analysis of Disaster and Payout Experience

| | Outcomes | | | | | |
|---|---|---|---|---|---|---|
| | | Demand | | | Knowledge | |
| Variables | (1) | (2) | (3) | (4) | (5) | (6) |
| Payout | 0.068*** | 0.149*** | 0.176*** | 0.023 | 0.018 | 0.004 |
| | (0.020) | (0.043) | (0.049) | (0.022) | (0.033) | (0.036) |
| Disaster Experience | -0.011 | -0.025 | -0.040* | 0.011 | 0.020 | 0.016 |
| | (0.009) | (0.018) | (0.022) | (0.013) | (0.019) | (0.022) |
| Discount Treatment | | 0.055*** | 0.071*** | | -0.011 | -0.025 |
| | | (0.012) | (0.014) | | (0.015) | (0.017) |
| Knowledge Treatment | | 0.018 | 0.002 | | 0.054** | 0.047** |
| | | (0.016) | (0.018) | | (0.022) | (0.024) |
| Payout × Discount Treatment | | -0.047*** | -0.067*** | | 0.042 | 0.053 |
| | | (0.015) | (0.016) | | (0.032) | (0.034) |
| Payout × Knowledge Treatment | | -0.021 | -0.024 | | -0.073* | -0.082* |
| | | (0.019) | (0.024) | | (0.043) | (0.046) |
| Disaster Experience × Discount Treatment | | 0.026 | 0.030 | | -0.019 | -0.004 |
| | | (0.021) | (0.026) | | (0.021) | (0.024) |
| Disaster Experience × Knowledge Treatment | | -0.011 | -0.002 | | 0.005 | -0.010 |
| | | (0.017) | (0.022) | | (0.027) | (0.030) |
| Household Characteristics | Yes | Yes | Yes | Yes | Yes | Yes |
| Other Controls | No | No | Yes | No | No | Yes |
| Observations | 3856 | 3856 | 2835 | 3520 | 3520 | 2835 |
| pseudo $R^2$ | 0.092 | 0.113 | 0.116 | | | |
| $R^2$ | | | | 0.069 | 0.075 | 0.095 |

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

Columns (4)-(6) of Table 3 reports the results for the dependent variable $Knowledge_{ijt}$. Here I observe positive but insignificant correlation coefficients for both payout and disaster experience. Unsurprisingly, receiving a knowledge treatment leads to a highly significant increase in knowledge scores by 4.7-5.4%. This is a 11-13% increase from the control group baseline mean of 42.9%. In comparison with the control group baseline standard deviation of 0.326, this is an increase of 0.1-0.2 standard deviations. Receiving a discount coupon, however, leads to no significant change in knowledge scores. Receiving a knowledge treatment also leads to a significant decrease in correlation between payout experience and knowledge scores. No other significant heterogeneity can be observed in payout and disaster experiences for the exogenous interventions.

These results show that receiving a discount coupon mechanically leads to higher demand, due to lower cost. Similarly, receiving knowledge interventions improve households' performances in the knowledge questions. However, combining discount intervention with a payout leads to a decrease in demand, while combining knowledge intervention with a payout leads to a decrease in interest for the product. In addition, a positive and significant correlation can be observed between payout experience and demand. On the other hand, the correlation between the outcomes and disaster experience is mostly insignificant.

### 4.1.2 Identification Problem

The main problem in identifying the causal effects of payout and disaster experience is that the households that had these experiences are different from the households that did not. As the correlation coefficients mostly provide evidence for payout experience, let me discuss why I cannot interpret those 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 regressions (5) and (6), the coefficient of $Payout_{ijt}$ captures 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 of changes that happened over time unrelated to the payout experience, may have an impact on the outcome variables. If not controlled for, these changes can be wrongfully attributed as being the causal effect of payout experience. However, the survey round fixed effects should account for part of this bias. Particularly, the part that is common to every households in the sample. For causal identification, the more problematic comparison here is the between-group comparison. The households that received the payout are not similar to the households that did not. For starters, the households that receive the payout were covered by the insurance at the time of the payout, while the households that did not receive the payout were not.[18] These two sets of households also differ in terms of their observable demographics, as well as baseline knowledge and demand. However, all of these differences can be controlled for in the regression. This is what I do in Table 3. But, the unobserved differences cannot be accounted for. Even though household fixed effects can be a solution, they cannot be used in the non-linear probit specification for dummy dependent variable $Demand_{ijt}$. Thus the coefficients of payout and disaster experience in Table 3 should not be mistaken as being causal.

#### 4.1.3 Payout Experience: Heterogeneity Analysis

To understand the direction of selection bias in the coefficient of $Payout_{ijt}$ in Table 3, I turn to the heterogeneity analysis of payout experience. This is presented in Table 4. Columns (1)-(3) presents the results for the dependent variable $Demand_{ijt}$. Column (1) is repeating the results from column (3) of Table 3. Column (2) reports the results without the survey round fixed effects. Thus, this column does not control for part of the bias from the within-group comparison discussed in the last sub-section. The coefficient of $Payout_{ijt}$ in column (2) is lower than that of column (1). This suggests that the survey round fixed effects correct for a downward bias in the coefficient. In column (3) I restrict the sample to the households that ever received a payout. This restricts the coefficient to reflect only the within-group comparison. For within-group comparison, the coefficient turns out to be negative at a 10% level of significance. This also suggests that the between-group comparison discussed in the last sub-section, eliminated in column (3), is creating an upward bias to the coefficient. Thus, not controlling for it in the regression is overestimating

[18]Recognition of this in the existing literature and the associated solution.

the coefficient of $Payout_{ijt}$ in Table 3 for the dependent variable $Demand_{ijt}$.

Table 4: Heterogeneity Analysis of Payout Experience: Full Sample vs. Restricted Sample

| | Outcomes | | | | | |
|---|---|---|---|---|---|---|
| | | Demand | | | Knowledge | |
| Variables | (1) | (2) | (3) | (4) | (5) | (6) |
| Payout | 0.176*** | 0.161*** | -0.158* | 0.004 | 0.002 | -0.094* |
| | (0.049) | (0.045) | (0.088) | (0.036) | (0.036) | (0.057) |
| Disaster Experience | -0.040* | -0.023 | -0.011 | 0.016 | 0.017 | 0.012 |
| | (0.022) | (0.022) | (0.069) | (0.022) | (0.022) | (0.040) |
| Discount Treatment | 0.071*** | 0.074*** | 0.203*** | -0.025 | -0.025 | -0.049 |
| | (0.014) | (0.015) | (0.055) | (0.017) | (0.017) | (0.047) |
| Knowledge Treatment | 0.002 | 0.000 | 0.162** | 0.047** | 0.047** | 0.042 |
| | (0.018) | (0.018) | (0.068) | (0.024) | (0.024) | (0.066) |
| Payout × Discount Treatment | -0.067*** | -0.072*** | -0.236*** | 0.053 | 0.053 | 0.073 |
| | (0.016) | (0.016) | (0.064) | (0.034) | (0.034) | (0.053) |
| Payout × Knowledge Treatment | -0.024 | -0.022 | -0.143** | -0.082* | -0.082* | -0.071 |
| | (0.024) | (0.025) | (0.058) | (0.046) | (0.046) | (0.070) |
| Disaster Experience × Discount Treatment | 0.030 | 0.030 | -0.013 | -0.004 | -0.004 | 0.008 |
| | (0.026) | (0.027) | (0.071) | (0.024) | (0.024) | (0.047) |
| Disaster Experience × Knowledge Treatment | -0.002 | 0.001 | -0.128* | -0.010 | -0.010 | 0.014 |
| | (0.022) | (0.023) | (0.068) | (0.030) | (0.030) | (0.057) |
| Survey Round Fixed Effects | Yes | No | No | Yes | No | No |
| Restricted Sample | No | No | Yes | No | No | Yes |
| Observations | 2835 | 2835 | 861 | 2835 | 2835 | 861 |
| pseudo $R^2$ | 0.116 | 0.101 | 0.221 | | | |
| $R^2$ | | | | 0.095 | 0.094 | 0.077 |

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

Columns (4)-(6) present the results for the dependent variable $Knowledge_{ijt}$. Column (4) is repeating the results from column (6) of Table 3. Column (5) reports the results without the survey round fixed effects. The coefficient of $Payout_{ijt}$ in column (5) is almost the same as that of column (4). This suggests that controlling for the survey round fixed effects creates no bias correction in the coefficient. When I restrict the sample to the households that ever received a payout in column (6), the coefficient turns out to be negative at a 10% level of significance. This suggests that the between-group comparison is creating an upward bias to the coefficient. Thus, not controlling for it in the regression is overestimating the coefficient of $Payout_{ijt}$ in Table 3 for the dependent variable $Knowledge_{ijt}$.

## 4.2 Identifying Effects of Payout Experience

Having understood the extent and direction of bias in the coefficient of $Payout_{ijt}$ in regressions (5) and (6), the next step is to try identifying the causal effect of payout experience. For that purpose, I use a differences-in-differences estimation strategy. The first subsection of this section focuses on discussing the estimation strategy in detail and presents the associated results. The last subsection of this section focuses on identifying the mechanisms behind the effects. In doing so, I discuss the associated triple-difference strategy and results.

### 4.2.1 Differences-in-Differences Strategy and Estimates

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

Panel A: Demand

Panel B: Knowledge

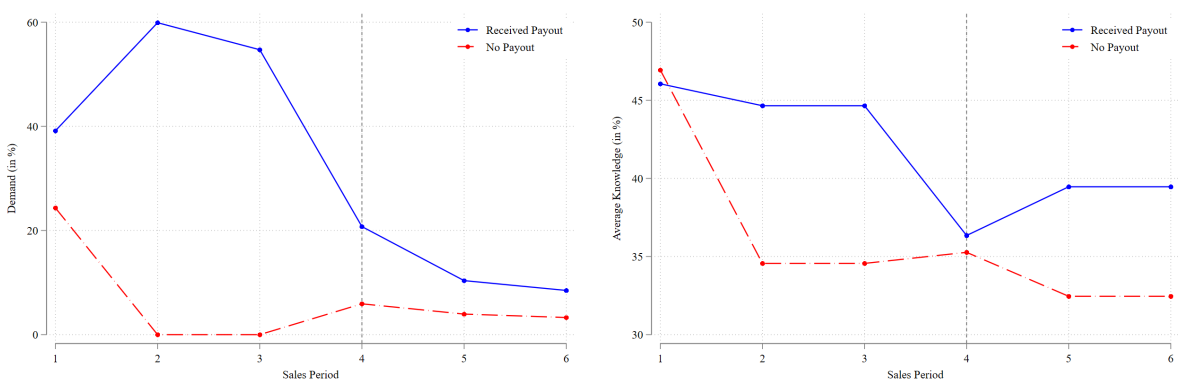


Figure 6: Outcome Variables Before and After Payout Experience

Figure 6 reports my main outcome variables, before and after the payout. The indemnity payouts were given between the sales periods 3 and 4. So, first 3 sales periods are before payout, while the last 3 of them are after payout. Panel A of the figure focuses on the demand for the product. In the baseline (sales period 1), the average demand is around 40% for the group that receives payout between the sales periods 3 and 4 (212 households). On the other hand, the group that never received payout (608 households) had an average demand of around 25% in the baseline. This highlights the presence of baseline differences between these two groups, at least in terms of purchasing the product. For the group that received payout, the average demand increases to 55-60% for the next two periods. As they received the payout before the fourth sales period, they must all be covered by the insurance product at the time of the payout. It may seem surprising that the average demand was only around 55-60% for this group, while 100% of them were covered by the product. But, this can be explained by the overlapping structure of the product design and that sales periods 2 and 3 were both in 2011. So, around 60% of the households that received payout got covered in sales period 2, with the rest of them purchasing the insurance in sales period 3 (some households purchased twice, to increase coverage). For the group that never received payout, the average demand is 0% for both sales periods 2 and 3. This is by design, as these are the households that did not get the payout, thus they were not covered when the payouts were made. After the payout, I observe a sharp decline in the demand for those who received the payout. Simultaneously, the demand increases slightly for the other group. This suggests a negative effect of payout on demand for those that received the payout and a positive spillover effect on those that did not receive the payout. But, there may be other factors driving these results.

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

$$Demand_{ijt} = \begin{cases} 1 \; if \; Demand^{*}_{ijt} = \alpha^D_0 + \alpha^D_1 Payout_{ij} + \alpha^D_2 Post_t \\ \quad + \alpha^D_3 Payout_{ij} \times Post_t + \lambda^D X_{ijt} + \nu^D_{ijt} > 0 \\ 0 \; otherwise, \end{cases} \tag{7}$$

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

Regression specification (7) captures the effect on the extensive margin of purchasing or not purchasing the product. However, in the subsequent analysis, I am also interested in the intensive margin of the amount of insurance bought. For this purpose, I use the following Tobit specification:

$$TLUI_{ijt} = \begin{cases} TLUI^*_{ijt} \; if \; TLUI^*_{ijt} = \alpha^I_0 + \alpha^I_1 Payout_{ij} + \alpha^I_2 Post_t \\ \qquad + \alpha^I_3 Payout_{ij} \times Post_t + \lambda^I X_{ijt} + \nu^I_{ijt} > 0 \\ 0 \; otherwise, \end{cases} \tag{8}$$

where $TLUI_{ijt}$ is the observed censored variable that captures number of tropical livestock units insured. $TLUI_{ijt}$ is equals to the latent variable $TLUI^*_{ijt}$ whenever $TLUI^*_{ijt} > 0$, 0 otherwise. Similar to the specification (7), here the main coefficient of interest is $\alpha^I_3$.

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

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

$$\begin{aligned} Knowledge_{ijt} =& \alpha_0^K + \alpha_1^K Payout_{ij} + \alpha_2^K Post_t + \alpha_3^K Payout_{ij} \times Post_t \\ &+ \lambda^K X_{ijt} + \nu_{ijt}^K, \end{aligned} \tag{9}$$

where $Knowledge_{ijt}$ captures the households' performance to the knowledge questions. Similar to the last two specifications, here the main coefficient of interest is $\alpha_3^K$.

The differences-in-differences specifications outlined in regression specifications (7), (8), and (9) exploits the change in outcome variables before and after the payout, and the differences between the group that received the payout with the group that did not. Since in the regressions, I control for the observed differences between the group that received the payout and the group that did not, I only need to assume that their unobserved differences remain the same over time, for identification. Hence, the identifying assumption for the differences-in-differences strategy can be described as the following:

**Identifying Assumption 1:** *Unobservable differences between the households that receives the payout and the households that did not, do not change over time.*

Table 5 presents the differences-in-differences estimates. There are two columns of results per dependent variable. The first column presents the main results, while the second column is a slight variation of the same regression specification that drops the

$Post_t$ dummy to include the survey round fixed effects instead. The results for $Demand_{ijt}$ are shown in columns (1) and (2). Receiving the payout decreases demand by 6.4-6.7% for the group that received the payout, compared to the group that never did. The result is statistically significant at a 1% level. This is a 79-83% decrease, compared to the comparison group mean at the baseline. In comparison to the baseline comparison group standard deviation, this is a decrease of 0.2 standard deviations.

Table 5: Heterogeneity Analysis of Payout Experience: Differences-in-Differences Estimates

| | Outcomes | | | | | |
|---|---|---|---|---|---|---|
| | Demand | | TLU Insured | | Knowledge | |
| Variables | (1) | (2) | (3) | (4) | (5) | (6) |
| Received Payout | 0.955*** | 0.953*** | 44.210*** | 44.260*** | 0.076*** | 0.076*** |
| (=$Payout_{ij}$) | (0.005) | (0.005) | (10.960) | (11.040) | (0.029) | (0.029) |
| Post Payout | 0.249*** | | 35.650*** | | 0.007 | |
| (=$Post_t$) | (0.028) | | (9.297) | | (0.023) | |
| $Payout_{ij} \times Post_t$ | -0.067*** | -0.064*** | -40.840*** | -40.860*** | -0.058* | -0.058* |
| | (0.005) | (0.005) | (10.560) | (10.650) | (0.035) | (0.035) |
| Baseline Comparison Mean† | 0.081 | 0.081 | 15.863 | 15.863 | 0.383 | 0.383 |
| (SD) | (0.273) | (0.273) | (53.126) | (53.126) | (0.315) | (0.315) |
| Survey Round Fixed Effects | No | Yes | No | Yes | No | Yes |
| Observations | 2835 | 2835 | 2834 | 2834 | 2835 | 2835 |
| pseudo $R^2$ | 0.349 | 0.363 | 0.166 | 0.170 | | |
| $R^2$ | | | | | 0.096 | 0.096 |

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

In terms of the number of TLU insured, presented in columns (3)-(4), receiving the payout decreases the number of TLU insured by around 41 units for the group that received the payout. This is around 258% decrease compared to the baseline comparison group mean, and around 0.8 standard deviations decrease compared to the baseline comparison group SD. This result is also significant at the 1% level. For the knowledge scores, receiving the payout leads to a decrease of around 5.8% for the group that received the payout. This can be seen in columns (5) and (6). Compared to the baseline comparison group mean, this is a decrease of around 15%. It is also a decrease of around 0.2 standard deviations, compared to the baseline comparison group SD. The result is significant at a 10% level.

The above 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 losing interest in the product as captured by their performances in the knowledge questions. However, there can be different possible mechanisms driving these results. For example, the households receiving a payout may use that money to buy some other product, decreasing their demand for the index insurance. 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 $\gamma^*$ downward, upon receiving a payout. The next subsection focuses on identifying the mechanism behind the effect of the payout experience.

#### 4.2.2 Understanding Mechanism: Triple-Differences Strategy and Estimates

To understand the extent to which the effect of payout experience is driven by more optimistic households updating their beliefs about $\gamma^*$ downward upon receiving a payout, I follow a triple-differences estimation strategy. In addition to using the before and after the payout differences between the group that received the payout and the group that did not, here I also use the differences in baseline perception. I proxy for baseline perception of the households regarding the product with their baseline demand. The implicit assumption being that the households that had more optimistic perception regarding the product are more likely to purchase it. As can be seen from Panel A of Figure 6, in the baseline only 40% of the group receiving payout purchased the product. Similarly, the demand was 25% for the group that never received the payout. This means that there is within-group variation in baseline demand for these two groups. This is the additional variation I use in the triple-differences estimation strategy, on top of the differences-in-differences variations discussed in the last sub-section. The triple-differences estimates use the following set of specifications:

$$Demand_{ijt} = \begin{cases} 1 \ if \ Demand^*_{ijt} = \beta^D_0 + \beta^D_1 Payout_{ij} + \beta^D_2 Post_t + \beta^D_3 Payout_{ij} \times Post_t \\ \quad + \beta^D_4 Perception_{ij} + \beta^D_5 Payout_{ij} \times Perception_{ij} + \beta^D_6 Post_t \times Perception_{ij} \\ \quad + \beta^D_7 Payout_{ij} \times Post_t \times Perception_{ij} + \delta^D X_{ijt} + \epsilon^D_{ijt} > 0 \\ 0 \ otherwise, \end{cases} \tag{10}$$

$$TLUI_{ijt} = \begin{cases} TLUI^*_{ijt} \ if \ TLUI^*_{ijt} = \beta^I_0 + \beta^I_1 Payout_{ij} + \beta^I_2 Post_t + \beta^I_3 Payout_{ij} \times Post_t \\ \qquad + \beta^I_4 Perception_{ij} + \beta^I_5 Payout_{ij} \times Perception_{ij} + \beta^I_6 Post_t \times Perception_{ij} \\ \qquad + \beta^I_7 Payout_{ij} \times Post_t \times Perception_{ij} + \delta^I X_{ijt} + \epsilon^I_{ijt} > 0 \\ \quad 0 \ otherwise, \end{cases} \tag{11}$$

$$\begin{aligned} Knowledge_{ijt} = &\beta^K_0 + \beta^K_1 Payout_{ij} + \beta^K_2 Post_t + \beta^K_3 Payout_{ij} \times Post_t + \beta^K_4 Perception_{ij} \\ &+ \beta^K_5 Payout_{ij} \times Perception_{ij} + \beta^K_6 Post_t \times Perception_{ij} \\ &+ \beta^K_7 Payout_{ij} \times Post_t \times Perception_{ij} + \delta^K X_{ijt} + \epsilon^K_{ijt}, \end{aligned} \tag{12}$$

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

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

Table 6 reports the estimates following the triple differences strategy. Similar to Table 5, there are two columns of results per dependent variable: first column presenting the main results, and the second column presenting the results for a slight variation in regression specification that drops the $Post_t$ dummy to include the survey round fixed effects.

Table 6: Heterogeneity Analysis of Payout Experience: Triple-Differences Estimates

| | Outcomes | | | | | |
|---|---|---|---|---|---|---|
| | Demand | | TLU Insured | | Knowledge | |
| Variables | (1) | (2) | (3) | (4) | (5) | (6) |
| Received Payout | 0.954*** | 0.952*** | 45.930*** | 46.010*** | 0.085** | 0.085** |
| (= $Payout_{ij}$) | (0.006) | (0.006) | (11.480) | (11.580) | (0.037) | (0.037) |
| Post Payout | 0.240*** | | 36.790*** | | 0.002 | |
| (= $Post_t$) | (0.029) | | (9.318) | | (0.026) | |
| Baseline Demand | 0.002 | 0.002 | -0.306 | -0.210 | 0.046 | 0.045 |
| (= $Perception_{ij}$) | (0.002) | (0.001) | (0.741) | (0.603) | (0.035) | (0.035) |
| $Payout_{ij} \times Post_t$ | -0.063*** | -0.059*** | -41.500*** | -41.560*** | -0.029 | -0.028 |
| | (0.006) | (0.006) | (10.460) | (10.550) | (0.043) | (0.043) |
| $Payout_{ij} \times Perception_{ij}$ | 0.001 | 0.001 | 1.573* | 1.522* | -0.020 | -0.020 |
| | (0.002) | (0.002) | (0.916) | (0.879) | (0.059) | (0.059) |
| $Post_t \times Perception_{ij}$ | 0.004 | 0.003 | 2.940 | 2.869 | 0.021 | 0.021 |
| | (0.004) | (0.003) | (1.801) | (1.817) | (0.042) | (0.042) |
| $Payout_{ij} \times Post_t \times Perception_{ij}$ | -0.005** | -0.005** | -3.934* | -3.876 | -0.073 | -0.073 |
| | (0.002) | (0.002) | (2.370) | (2.434) | (0.073) | (0.073) |
| Baseline Comparison Mean† | 0.081 | 0.081 | 15.863 | 15.863 | 0.383 | 0.383 |
| (SD) | (0.273) | (0.273) | (53.126) | (53.126) | (0.315) | (0.315) |
| Survey Round Fixed Effects | No | Yes | No | Yes | No | Yes |
| Observations | 2835 | 2835 | 2834 | 2834 | 2835 | 2835 |
| pseudo $R^2$ | 0.350 | 0.364 | 0.167 | 0.170 | | |
| $R^2$ | | | | | 0.098 | 0.098 |

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

The results for $Demand_{ijt}$ are shown in columns (1) and (2). For the households having a higher perception of the product in the baseline, receiving a payout leads to 0.5% decrease in demand. The result is statistically significant at a 5% level. This is a 6% decrease, compared to the comparison group mean at the baseline. In comparison to the baseline comparison group standard deviation, this is a decrease of 0.02 standard deviations. In terms of the number of TLU insured, presented in columns (3)-(4), receiving the payout decreases the number of TLU insured by around 4 units for the households having a higher perception of the product in the baseline. This is around 24-25% decrease compared to the baseline comparison group mean, and around 0.07 standard deviations decrease compared to the baseline comparison group SD. This result is at most significant at the 10% level.

For the knowledge scores, receiving the payout leads to a decrease of around 7.3% for the households having a higher perception of the product in the baseline. This can be seen in columns (5) and (6). Compared to the baseline comparison group mean, this is a decrease of around 19%. It is also a decrease of around 0.2 standard deviations, compared to the baseline comparison group SD. The result is, however, statistically insignificant.

The above 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 $\gamma^*$ downward, following a payout. In particular, this is found to be true for the extensive and intensive margin of demand. No such evidences can be observed on the households' performances in the knowledge questions.

# 5 Summary and Concluding Remarks

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

Correlational analysis shows a significant correlation between payout experience and the demand for the product. I use a differences-in-differences identification strategy to identify the causal effect of receiving a payout. This is done after discussing the identification problem in detail, along with understanding the direction and source of bias in the correlation analysis. My results show that receiving a payout decreases the extensive and intensive margin of demand for the group that received the payout, with the same group losing some interest in the product after the payout. In the subsequent analysis, I use a triple-differences identification strategy to identify the causal mechanism of such an effect. I find that the effect of payout on the outcome variables, at least in part, can be explained by optimistic households updating their beliefs about the product design downward, following a payout.

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

The correlational analysis also suggests that while receiving a discount intervention mechanically increases demand, it also increases households' chances of receiving a payout leading them to optimally lower their demand.[19] Similarly, receiving a knowledge intervention mechanically increases households' knowledge regarding how the product works, but helps them update their interest in the product downwards once a payout is received. These results are similar to the findings in Cai et al. (2020). However, in their study, the payouts are found to be improving demand. These findings suggest that discount and knowledge interventions can be used as tools for enhancing households' learning from experience. However, on their own, they may not help households overcome information frictions.

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

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# Appendices

## A 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 $m$.

## B Robustness Checks