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Creating informed demand and impacts through innovative and cost-effective extension

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Creating informed demand and impacts through innovative and cost-effective extension

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Summary

Insurance is a means of mitigating the impacts of shocks on households and is promoted as a tool to benefit agricultural households in various settings. For example, index-based livestock insurance (IBLI) uses the normalized difference vegetation index (which measures the health and density of vegetation using sensor data) to identify forage anomalies and makes payouts when forage is much scarcer than usual. Such anomalies, usually the result of drought, have been identified as a major cause of poverty in pastoral regions.

IBLI has been sold by commercial firms to pastoralists in Kenya and Ethiopia since 2010 and 2012, respectively; it is currently being used by several government and NGOs to support pastoralists during droughts. The largest such project was the Kenya Livestock Insurance Program (KLIP), which, as part of its social protection and development programming, insured 18,000 targeted pastoral households each year between 2015 and 2022. Although KLIP was not funded in 2022, the De-risking, Inclusion and Value Enhancement of Pastoral Economies in the Horn of Africa project has promised to dramatically increase IBLI coverage across Kenya, Ethiopia, and Somalia.

This study examines how clients' understanding of IBLI determines its impacts. While receiving and using indemnity payouts during a drought does not require any understanding of the product itself, most of its benefits are believed to be the result of adjustments to production strategies in non-shock years. Indeed, a number of studies on IBLI and other agricultural insurance products in lower-income settings have found that coverage leads to higher-risk, higher-return production strategies.

Such adjustments require, among other things, a sound knowledge of the insurance product so that its coverage can be accurately integrated into clients' risk assessments. IBLI is often the first insurance product to which prospective clients are exposed, and its nature as an index product may make it less intuitive than conventional loss-indemnifying insurance.

This study uses a randomized extension treatment to study the role of clients' IBLI knowledge on the product's impacts. The extension treatment—a short, simple video—was developed to be provided to prospective clients via digital devices, to be extremely low-cost, and to be easily disseminated and shareable between individuals. These features are quite different from insurance firms' existing outreach strategies, which rely primarily on the provision of costly training for insurance agents to perform in-person extension activities and can exclude large portions of the population (as insurance agents rarely travel to remote communities to perform such promotional activities).

This report demonstrates that the IBLI extension video increases participants' knowledge of the product, although to a much lesser degree than a separate treatment that provides participants with premium discounts. Tests do not identify any impacts of IBLI coverage on households, nor are there observable variations in impacts across product knowledge. These findings are at odds with research from a similar study on the impacts of insurance and highlight the need for caution when generalizing results found in one study (whether they confirm or reject prevailing beliefs). In this case, there are no obvious reasons in the research design, location, or implementation timing that would make this study any more or less generalizable than other studies on drought-related livestock insurance in Africa.

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Abbreviations and acronyms list

CESD	Center for Epidemiologic Studies Depression
IBLI	Index-based livestock insurance
IHS	Inverse hyperbolic sine
ILRI	International Livestock Research Institute
IU	Index unit
IV	Instrumental variable
REAP	Rural Entrepreneur Access Project
TIA	Takaful Insurance of Africa Ltd
TLU	Tropical livestock unit

1. Introduction

Index-based livestock insurance (IBLI) has been commercially available to pastoralists in Marsabit County in northern Kenya since 2010 and has now scaled up to be available across northern Kenya and in multiple regions of Ethiopia. IBLI was adopted by the Kenyan government under its Kenya Livestock Insurance Program (KLIP), which purchased insurance each year between 2015–2022 for over 18,000 targeted pastoral households; it is now being used by World Food Program initiatives to support pastoralists in Ethiopia and in Zambia.

More recently, the De-risking, Inclusion and Value Enhancement of Pastoral Economies in the Horn of Africa project has promised to dramatically increase IBLI coverage across Kenya, Ethiopia, and Somalia. IBLI feasibility studies have also been commissioned by several other governments and non-government actors, as the first step to developing IBLI products for their pastoral constituents.

The continued scaling of IBLI is not surprising, given its appeal to the donor and academic communities. As a market-based intervention that theoretically promotes increased production and resilience, IBLI could have large knock-on effects in complimentary markets such as credit and livestock inputs and could be self-sustaining after initial public investments (Barrett et. al. 2007; Barnett and Mahul 2007; Jensen and Barrett 2017; Miranda and Farrin 2012).

Evaluations of IBLI find that its coverage benefits households in productive seasons as well as droughts. In drought seasons, poorer households with coverage are less likely to skip meals as a coping mechanism, and other insured households are less likely to sell their productive assets (so-called distress selling, as prices are at their lowest point during droughts and rebuilding productive assets will be more costly later) and have greater milk production (Janzen and Carter 2019; Matsuda, Takahashi, and Ikegami 2019; Noritomo and Takahashi 2020).

In non-drought seasons, households with IBLI coverage increase their investments in intensification, increase expenditures on livestock inputs and engage with livestock markets more actively, thereby increasing household income (Jensen, Barrett, and Mude 2017). Further, households with IBLI coverage have greater subjective welfare even if they have not received any indemnity payouts, presumably because the insurance coverage makes them feel more secure (Tafere, Barrett, and Lentz 2019).

While IBLI is scaling up, and there are proven benefits to its coverage for some households, there are still many unanswered questions. Uppermost of these is: how to increase the value of IBLI and other similar financial tools for consumers? We hypothesize that increasing product knowledge among prospective IBLI clients will increase its average impact on the insured.

Importantly, as much of the literature on basis risk has suggested, there are likely to be households that do not benefit from index products, even if those products are very accurate in tracking average outcomes, and the reality is that index products (including IBLI) are imperfect (Clarke 2016; Jensen, Barrett, and Mude 2016). The hope is that better informed prospective clients will be able to more accurately determine whether they would benefit from insurance coverage and make purchase decisions accordingly.

Further, clients with a better understanding of their insurance coverage may respond to it in a more beneficial way.

To study the role of product knowledge on the impacts of IBLI coverage, we implemented a randomized control trial with 1,875 participants in Samburu County, Kenya. Baseline data were collected in 2018, with follow-ups in 2020 and 2022. The research design included randomized insurance premium discounts and extension treatments, which were factorially implemented across four treatment arms including control.[1] The extension treatment was designed specifically to be low-cost, as insurance firms have frequently reported cost as a main constraint to increased extension by insurance agents.

This report draws on the baseline (2018), midline (2020), and endline (2022) household surveys collected by the International Livestock Research Institute (ILRI); administrative data on Takaful Insurance of Africa Ltd. (TIA)'s insurance sales in 2018, 2019, 2020, and 2021; and implementation information recorded by researchers and enumerators through several channels on the randomized interventions.

This report is organized as follows. Section 2 describes the IBLI product in more detail, as well as the theory of change. Section 3 describes the research questions, the empirical strategy for answering each question, and the survey tool. Section 4 describes the experimental findings. Section 5 provides a cost analysis of the intervention. Section 6 includes a discussion, and Section 7 closes with conclusions and recommendations.

2. Intervention

2.1 Description

IBLI is a commercial product that provides coverage to pastoralists in northern Kenya and southern Ethiopia against extreme negative forage anomalies. In Kenya, the product is based on an anomaly index generated by the eMODIS (Expedited Moderate Resolution Imaging Spectroradiometer) Normalized Difference Vegetation Index C6 product.[2] This product is generated from data collected by the Moderate Resolution Imaging Spectroradiometer, which is on NASA's AQUA satellite. This daily data is processed by the Earth Resources Observation Systems (EROS) of the US Geological Survey (USGS) to create the decadal eMODIS Normalized Difference Vegetation Index C6 product.

These decadal data are then averaged within index units (IUs), which are rangeland areas of approximately 200–2,000 km^2 that have been grouped to form covariate regions. The spatially averaged indices are then averaged over each of the rainy seasons observed in northern Kenya, forming a distinct seasonal index after the long rains and after the short rains, for each season and within each IU. Those indices are

[1] The study also included an analysis of the REAP intervention implemented by the BOMA Project (https://bomaproject.org/); however, assessing this intervention is not within the scope of this report.

[2] As of this report, there have been several changes to the IBLI product. We will focus only on the product that was active between 2018–2022 (when the field activities and data collection described in this report took place).

then normalized by subtracting the long-run mean and dividing that difference by the long-run standard deviation, within each IU and season. The resulting index is a normalized metric of Normalized Difference Vegetation Index anomalies, which is called the IBLI index.[3]

Insurance policies are written using this IBLI index by identifying an attachment point, which triggers a payout, and a payout function that determines the rate of payout against the total sum insured. In the case of Kenya, the payout function is described in Equation 1 below. $Index_{IU,s,year}$ is the index calculated in each IU (IU), in each season (s) and year ($year$), as was described above. $Attachment\ Point_{IU,s}$ is set at the twentieth percentile of the distribution of the index within each IU and season, so that the insurance product should pay out every fifth long-rain, long-dry season and every fifth short-rain, short-dry season in expectation. $Exit_{UI,s}$ is the lowest (worst) index value ever observed in that season within that IU. The seasonal multiplier (θ_s) is a mechanism for allocating risk within the 12-month policies across the two covered seasons; it is equal to 0.42 in short-rain seasons and 0.58 in long-rain seasons.[4]

$$Payout\ Rate\ _{IU,s,year} = \left[\frac{Index_{IU,s,year} - Attachment\ Point_{IU,s}}{Exit_{UI,s} - Attachment\ Point_{IU,s}}\right] * \theta_s \quad (1)$$

Payouts are then calculated by multiplying the payout rate by the total sum insured. In Kenya, the insured value is KES 19,600, KES 14,000, and KES 1,400, for a camel, cattle, and sheep or goat, respectively.[5]

In Kenya, IBLI is sold by commercial insurance firms and regulated by the Kenyan Insurance Regulatory Authority. Insurance purchases take place during two sales windows (January 1–February 28, and August 1–September 30), which are just before each rainy season. In all cases, the index is calculated by a third party (ILRI to date) and provided to all the insurance firms simultaneously.

To date, three insurance firms in Kenya have offered IBLI to the public: APA Insurance, UAP Insurance Company, and TIA. Each of these firms has had to establish and maintain their own infrastructure for market development and supply, which has been challenging and expensive in the remote regions in which IBLI is sold. Insurance firms have frequently sited these expenses as a major barrier to the sustainability of IBLI markets, while prospective clients often report a lack of access to information or insurance agents as a key barrier to purchasing insurance coverage.

2.2 Theory of change

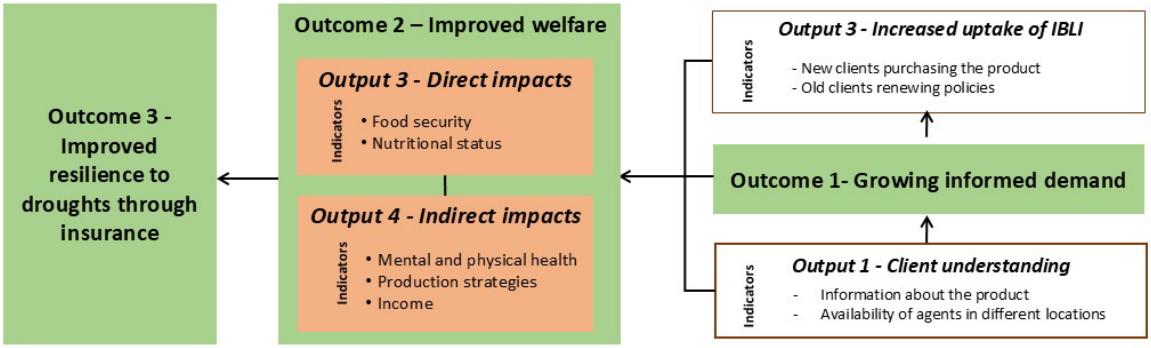
Our theory of change focuses on the role that product knowledge plays in maximizing the expected benefits of insurance coverage. We hypothesis that a lack of product knowledge is not only a barrier to its purchase, but also a barrier to realizing its full potential benefits. If households purchase insurance without a good understanding of

[3] See Vrieling et al. (2016) for more details on the construction of the IBLI index.
[4] The implication is that the risk to the insured value is allocated across two seasons, with the long-rain, long-dry season presenting greater risk.
[5] The mean exchange rate in 2020 was US$ 1 = KES 106.5.

their coverage, they may make suboptimal production or investment decisions; this could not only undermine the benefits of insurance coverage but could increase the likelihood of households being worse off than they would have been without insurance.

At the same time, there are likely to be households that could benefit from insurance coverage but have (wisely) not purchased it due to a lack of understanding of the product, as well as others unlikely to benefit from coverage but have (unwisely) purchased a product that they do not fully understand. Our theory of change is illustrated in Figure 1.

Figure 1: Theory of change between insurance product knowledge and improved resilience



In this study, we test whether the low-cost, direct-to-client extension video increased knowledge of the IBLI product among those that watched it. We then test for the impact of changes to product knowledge on IBLI purchases and the impact of IBLI coverage. We study IBLI's impact on outputs previously shown to be sensitive to insurance, including changes to production strategies (presumably because IBLI mitigated the risk of drought) and outcomes from changes to production strategies (including measures of income, food security, and consumption). Further, studies have found that IBLI provides peace of mind to its clients even when payouts have not taken place (Tafere, Barrett, and Lentz 2019). While we do not have any direct measures of peace of mind, we do calculate the 10-question Center for Epidemiologic Studies Depression (CESD) score.

3. Evaluation

A pre-analysis plan was filed at https://ridie.3ieimpact.org/ on March 11, 2019 under the title "TW13.II/1020 Creating Informed Demand and Impacts through Innovative and Cost-Effective Extension."

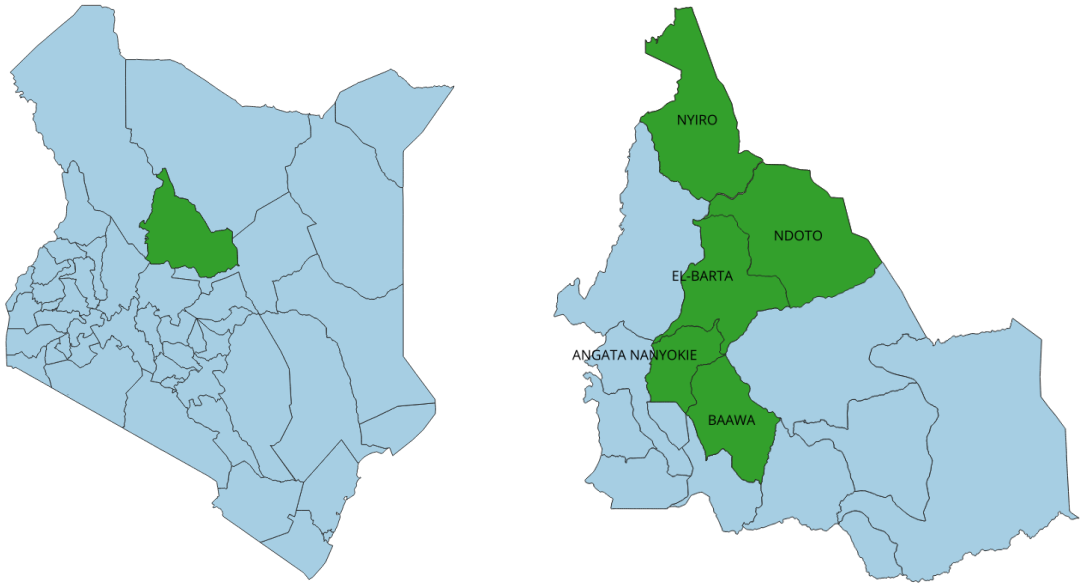
3.1 Sampling

This study takes place in the pastoral regions of Samburu County of northern Kenya, and is part of a study examining the complementarities between IBLI and a graduation program called the Rural Entrepreneur Access Project (REAP) implemented by the BOMA Project. REAP provides entrepreneurship training, business grants, and mentoring support to targeted women.[6]

[6] For more information on REAP, see https://boma.ngo/the-boma-model/.

Samburu County was purposefully selected because as of 2018, IBLI had not yet been sold there but was about to be offered (making it relevant but without contamination), and the county has a large population of pastoralists. Wards in eastern Samburu County were purposefully selected for this study to avoid regions where the REAP intervention had been implemented. Figure 2 includes a map of Kenya with Samburu County highlighted (left panel) and a map of Samburu County with the study wards highlighted (right panel).

Figure 2: Study region in Samburu County of Kenya



Source: American Red Cross (2019)

Within the selected wards, household-level wealth ranking was completed by community members and facilitated by the Boma Project. Women living in households in the bottom three of five wealth categories and deemed mentally and physically capable (those with severe chronic illnesses that prevented their participation were excluded) and interested in participating were eligible for this study.[7] BOMA staff confirmed exclusion characteristics at the household level.

Households were then randomly selected from this pool of REAP-eligible households (proportional to the number of eligible households in the community) to be invited to participate in the study. In addition to REAP-eligible households, a smaller sample of women from a poor but REAP-ineligible category of households (the fourth wealth category, as indicated by the community-based ranking) were also selected from the same *manyattas*[8] to inform on the impacts of insurance on this relatively wealthier (though still poor) group.

In total, 1,875 participants were selected to participate. Households from within each *manyatta* were randomly allocated into each treatment arm: control; extension video;

[7] This process was implemented by the Boma Project and follows their standard process for selecting participants.

[8] A *manyatta* is a temporary grouping of households similar to a small village and can be composed of one or many families.

premium coupon; and extension video and premium coupon (Table 1).There were three types of discount coupons: Coupon A provided a flat rate of 100 percent subsidy for four tropical livestock units (TLUs); Coupon B provided a subsidy that was highest for the first TLU of coverage and fell with each subsequent TLU; and Coupon C provided a subsidy that increased in rate with more purchases, up to six TLUs of coverage. All three coupon types had the same total subsidy available. See Appendix A for more information on the extension videos and discount coupons.

Table 1: Baseline sample assigned to each treatment arm

		Premium discount		**Total**
		No	Yes	
Extension video	No	473	454	927
	Yes	470	478	948
Total		943	932	1,875

The baseline survey was collected in January and February of 2018, just as IBLI was becoming available in Samburu County for the first time. IBLI continued to be available in the study region from that period onwards.[9]

3.2 Design and methods

This research tests the efficacy of a low-cost, direct-to-client extension intervention for improving product knowledge and improving the impacts of IBLI. To do so, it focuses on four central questions.

Research question 1: To what extent can a standardized low-burden, client-targeted extension activity improve understanding of the product relative to the standard extension activities provided by insurance agents?
To examine this, we developed a nine-minute video that provides prospective clients with a summary of the most important product features. In January and February 2020, a randomized subsample of the project participants was treated with the extension video approximately one third of the way through the midline survey.

Later in the same survey, and about 30 minutes after the section in which the video was played for the treatment group, all participants—both control and treated—were asked to answer a set of questions about the IBLI product. The same extension-treated respondents were shown extension videos a second time in January 2021, and a third time during the endline survey in 2022. During this endline survey, all respondents were again asked to answer a set of questions about the IBLI product.

The three main outcomes of interest are: (1) whether the respondent has heard of the IBLI product; (2) the respondent's perspective on the potential value of the IBLI product for pastoralists generally; and (3) respondents' scores on a quiz testing knowledge of a variety of IBLI product features and details of IBLI policies. To assess the impact of the video treatments on our three outcomes of product understanding, we perform the following regression described in Equation 2, where $Product_{it}$ is the outcome for

[9] IBLI had been available in some neighboring counties for a number of years but there were no sales agents in Samburu until 2018.

individual *i* in period *t*, α^1 is the impact of receiving the extension treatment (ET_{it}) in period *t*, α_i^2 is an individual fixed-effect, α_t^3 is a period-level fixed effect, and ε_{it} is the remaining variation.

$$Product_{it} = \alpha^0 + \alpha^1 ET_{it} + \alpha_i^2 + \alpha_t^3 + \varepsilon_{it} \quad (2)$$

In this case, there are three data periods: baseline (t = 2018), midline (t = 2020), and endline (t = 2022). ET_{it} is zero for all participants at baseline and one for those in the extension treatment arm in both 2020 and 2022.

Research question 2: Does product understanding impact demand for IBLI?
To assess the impacts of extension treatments, we use administrative data of IBLI purchases in 2018, 2019, 2020, 2021, and 2022.

Research question 2.1: Does product understanding increase demand for IBLI?
We cannot assess the impact of product understanding on demand for IBLI directly for two reasons. The first is that product understanding is unlikely to be exogenous to update. The randomization of the extension video and its prospective impacts on product understanding could make the extension video a useful instrumental variable (IV) for product understanding.

However, there are two drawbacks to using the extension treatment as an IV for product knowledge in this case. The first is logistical: product knowledge was only collected during the main survey years (i.e., 2018, 2020, and 2022) and so any analysis using product knowledge cannot examine uptake in 2019 or 2021. The second is related to timing; the video was first implemented in 2020, which means that it could not explain any changes in product understanding in 2018.

The third is operational; the participants that reported never hearing of IBLI did not perform the quiz on IBLI during the 2018 or 2020 survey rounds; therefore, quiz completion itself may be endogenous to the purchase decision. The result is that there are no IBLI test scores for nearly all households during the baseline. Therefore, using the extension video as an IV for product knowledge restricts the data analysis considerably.

Instead, we focus on the intent-to-treat impacts of the extension video. This allows us to use data from all five project years (2018, 2019, 2020, 2021, and 2022) in the analysis. To estimate the intent-to-treat estimates, we perform the regression described by Equation 3, where $IBLI_{imt}$ is the coverage purchased by individual *i* in *manyatta m* in period *t*, and β^1 is the impact of the extension treatment (ET_{imt}) on purchase behavior. x_{imt} includes a set of household and environmental characteristics that are likely to impact demand for IBLI. *Manyatta*-level fixed effects are used rather than individual fixed effects, because treatment status does not change within individuals between the midline and endline rounds.

$$IBLI_{imt} = \beta^0 + \beta^1 ET_{imt} + \beta^2 x_{imt} + \beta_m^3 + \beta_t^4 + \varepsilon_{imt} \quad (3)$$

The covariates found in x_{imt} include: relative forage index of the previous long-rain, long-dry season (March–October), relative forage index of the current short-rain, short-dry season (October–February), household type (sedentary, partially settled, or nomadic), whether the household herds livestock, whether the household grow crops, the number

of household members, whether the respondent is head of the household, and whether the respondent has completed any education level, participation in the KLIP program, and current reported participation in the REAP graduation program. These same covariates are included in all subsequent analyses.

Research question 2.2: Does product understanding change who purchases IBLI? Recalling that the theory of change hypothesizes that improved product knowledge will help potential clients to more accurately sort themselves into purchasers and non-purchasers according to their expected value of purchasing IBLI, this question focuses on heterogeneity in the effect of extension activities on purchase behavior across household characteristics.

To examine this, we interact the treatment variable with the set of household characteristics listed earlier—any of which could reasonably impact the expected benefits of insurance or the impact of the extension video.[10] We also add the discount coupons to the list of possible interactions, as they would change the expected net value of purchasing IBLI and potential liquidity constraints faced by the household. The estimation is described by Equation 4, where the coefficient estimates found in the vector β^3 describe heterogenous impacts of the extension treatment related to each household characteristic.

$$IBLI_{it} = \beta^0 + \beta^1 ET_{it} + \beta^2 x_{it} + \beta^3 ET_{it} * x_{it} + \beta_i^4 + \beta_t^5 + \varepsilon_{it} \quad (4)$$

Research question 3: What are the impacts of insurance coverage on households? This analysis focuses on indicators of production, income, food security, and health. To identify the causal impacts of IBLI purchases, which are likely to be endogenous to our outcomes of interest, exogenous variation in premium rates faced by participants is used as an IV for purchases. The premium rates were varied exogenously by distributing premium subsidies to a randomly selected subset of participants in the form of discount coupons that could be used to purchase insurance from TIA.

The coupons were distributed in January and February of 2018, 2019, 2020, and 2021. Each coupon could be used once, in either of the two insurance sales windows, in the year they were allocated. The coupons were non-transferable, although respondents without government issued identity cards (required to purchase IBLI coverage) could request that a close relative or family member use the coupon to purchase insurance on their behalf. The exogenous reduction in prices has been shown to increase purchases in other similar environments and can then be used to examine the causal impacts of insurance coverage.

Equation 5 describes the approach used to test the impacts of coverage purchased in the 12 months preceding the survey ($IBLI_{it-1}$) on outcomes at the time the surveys were collected (y_{it}). Discount coupons treatments in the previous period (DCT_{it-1}) are used as

[10] Note that we use the intention-to-treat variable here because it simplifies the analysis, and extremely high compliance rates—enumerators reported 100 percent viewership among those interviewed and in the treatment group, and 0 percent viewership among those interviewed and in the control group—mean that there are few, if any, advantages to the IV estimate. In addition, using the intention-to-treat estimate allows us to use the 2018 round of data, which was collected before the extension treatment took place and therefore creates variation in treatment status among the treated overtime, thereby allowing the use of individual-level fixed effects.

IVs for IBLI coverage in the previous period, as described by Equation 6.[11] Once again, vectors controlling for household and environmental characteristics are included (x_{it-1}), as are individual-level (α_i) and period (α_t) fixed effects to account for variation between individuals or rounds in access to IBLI or in outcomes.

$$y_{it} = \beta^0 + \beta^1 IBLI_{it-1} + \beta^2 x_{it-1} + \beta_i^3 + \beta_t^4 + \varepsilon_{it} \quad (5)$$

$$IBLI_{it-1} = \alpha^0 + \alpha^1 DCT_{it-1} + \alpha^2 x_{it-1} + \alpha_i^3 + \alpha_{t-1}^4 + \gamma_{it-1} \quad (6)$$

Research question 4: Does insurance coverage have greater benefits for those who better understand the product?

To test this question, we reexamine the impacts of insurance, allowing for heterogeneity across those treated with the extension video and those who were not. Explicitly, we hypothesize that the extension video could improve the average outcome of purchasers by changing who purchases IBLI and by changing the behavior of these purchasers.

Similar to the approach used for Research question 3, we instrument for IBLI purchases using the randomized premium discounts (Equations 7.1 and 7.2). Drawing on the methods used in Research question 2.2, we interact the extension treatment with household and environmental characteristics, including with the premium discount coupons, in the first stage of the IV process. These interactions then allow the extension treatment to have heterogenous impacts on the purchases predicted from the first stage of the IV process (i.e., improved sorting). In addition, we add the variable of interest ($IBLI_{it-1} * ET_{it-1}$) to the main estimation equation, which is the interaction between purchases and the extension video, so that the estimated impact of purchases themselves may vary across the extension video (improved impacts, Equation 7.3).

$$y_{it} = \beta^0 + \beta^1 IBLI_{it-1} + \beta^2 IBLI_{it-1} * ET_{it-1} + \beta^3 x_{it-1} + \beta_i^4 + \beta_t^5 + \varepsilon_{it} \quad (7.1)$$

$$IBLI_{it-1} = \alpha^0 + \alpha^1 DCT_{it-1} + \alpha^2 ET_{it-1} * DCT_{it-1} + \alpha^3 x_{it-1} + \alpha^4 x_{it-1} * ET_{it-1} + \alpha_i^5 + \alpha_{t-1}^6 + \mu_{it-1} \quad (7.2)$$

$$IBLI_{it-1} * ET_{it-1} = \delta^0 + \delta^1 DCT_{it-1} + \delta^2 ET_{it-1} * DCT_{it-1} + \delta^3 x_{it-1} + \delta^4 x_{it-1} * ET_{it-1} + \delta_i^5 + \delta_{t-1}^6 + \omega_{it-1} \quad (7.3)$$

3.3 Monitoring plan

The main threats to the research design are: (1) treatments that are not accurately administered; and (2) insurance that is not easily accessible for those that would like to purchase coverage. To address the first threat, we included tracking protocols for the implementation of treatments. These include the following:

- **Accurate treatment**: Extension and discount treatments were integrated into the surveys, so that the survey initiated the treatments. Explicitly, the survey instructed enumerators to distribute appropriate discount coupons for those that were to receive the discount, and the video extension treatment played within the survey software for those that were to receive the extension treatment.

[11] Note that the discount coupons provided a powerful tool for increasing insurance purchases, but the compliance rate is far from perfect (the average rate across the four rounds of coupons is 64 percent). Therefore, the IV approach is more appropriate than the intention to treat, which would generate an estimate of the impacts of being offered discount coupons; this is likely to be considerably lower than the impacts of purchasing insurance.

Enumerators then had to confirm that treatment was provided. In the case of the discount coupons, the survey also prompted enumerators to take a photo of the coupon distributed to that household. Because they were performed within the survey, all these monitoring points are directly linked to the household data.

- **Non-transferability of discount coupons**: Each discount coupon had a unique serial number, and the name and ID number of its recipient printed on it. Insurance agents are trained to only accept discount coupons when they are presented by a person with an ID that matches the name and ID on the coupon.
- **Accountability**: All employed discount coupons and administrative data on IBLI coverage for each sales season are submitted by the insurance company to ILRI for confirmation.

The survey and administrative data were checked and cross-checked as they came in. These activities found several errors, many of which we were able to fix by examining the monitoring data. For example, there are many cases in which respondents misreported their IBLI purchases, which we could identify by examining administrative data from TIA. At the same time, we also identified errors in TIA's administrative data, which we were able to fix using survey data on discount coupons and respondents' ID numbers. Of course, there have also been errors in our implementation of the treatments and surveys. In all cases, we have worked with partners to address these issues in implementation and data.

3.4 Ethics

We have approval from ILRI's ethics board (Ref: ILRI-IREC2019-29) and Kenya's National Commission for Science, Technology and Innovation (Ref No: 190042, License No: NACOSTI/P/21/14599). In addition, all enumerators and participants were provided with the phone number for ILRI's research compliance officer in Nairobi during training and/or the consent process.

3.5 Data collection

A total of 1,875 respondents were interviewed at baseline in January/February 2018. The midline and endline surveys were collected from the same households in January/February 2020 and January/February 2022. There was an attrition rate of less than 8 percent between survey rounds, which was lower than anticipated during the planning stages of the project (Table 2). [12] One main cause of attrition was migration of households outside the study region (e.g., to Marsabit County to attend ceremonies related to the graduation of an age-set). Government eviction of households in a number of survey communities was the second largest cause of long-distance and long-term migration and subsequent attrition.[13]

Links to baseline, midline, and endline survey tools can be found in Appendix B.

[12] Attrition rates of up to 12 percent between survey rounds were planned for because: (1) the survey rounds are two years apart; and (2) pastoral households migrate frequently, making them difficult and costly to track down.

[13] Several study communities were located in the Kirisia Forest and forcibly evicted/voluntarily relocated in late 2019 and early 2020. Upon eviction, many households moved to where they had family, which was often outside of the study region.

4. Findings

4.1 Intervention implementation fidelity

The premium discount coupons were distributed in 2018, 2019, 2020, and 2021. Note that the 2019 and 2021 distributions were not accompanied by a survey of all participants—non-treated participants were not visited—while the 2018, 2020, and 2022 distributions were part of the household surveys. The video was integrated into the midline (2020), endline (2022), and implementation (2021) surveys so that treatments could not be misallocated.[14] Table 2 shows the intent-to-treat distributions across the baseline, midline, and endline surveys.

Table 2: Attrition and adherence to the experimental design

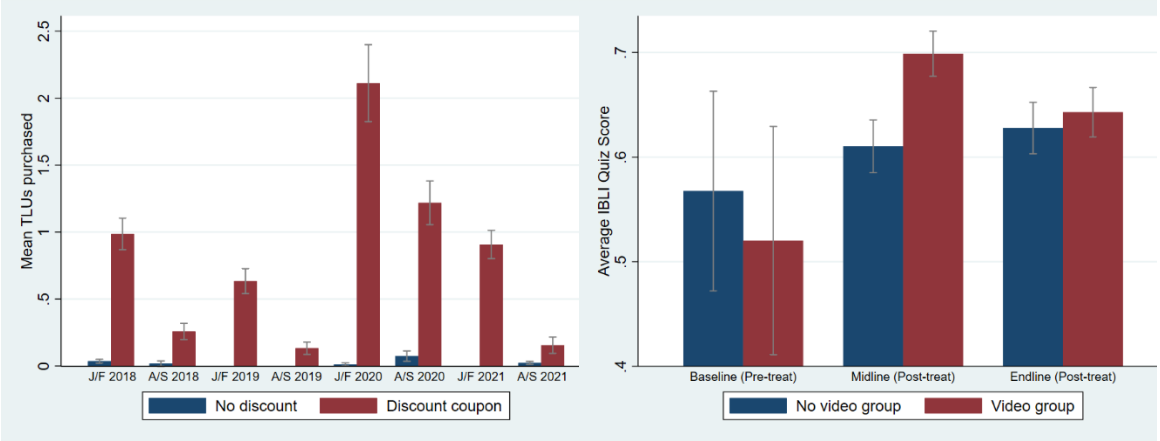
	Intent	Implementation				
	to treat	2018 (baseline)	2019[1] (treat)	2020 (midline)	2021[1] (treat)	2022 (endline)
Experimental treatments						
No discount	943	964		895		1,612
Discount	932	911	882	839	766	
No video	927			850		769
Video	948			884	790	825
Attrition						
- Refused consent				0		0
- Died				11		22
- Medical reasons				15		20
- Moved or migrated outside study region				112		83
Surveyed	1,875	1,875	885	1,734		1,612
Between round sample attrition				7.52%		8.07%
Total attrition				7.52%		14.9%
Coupon treatment accuracy		97.6%		97.1%		100%

Note: [1]The activities in 2019 and 2021 only focused on the treated; therefore, there are no figures on non-treated participants for those activities.

There is strong evidence that the randomized premium discount treatments resulted in their intended immediate effects of increasing uptake, and weaker evidence that the video extension treatments increased product knowledge. Figure 3 illustrates the differences in uptake across premium discounts and sales windows (left panel) and the differences in product knowledge by extension video treatment (right panel).

[14] This is not a statement about potential spillovers, which are a potential issue with the videos. Rather, this statement refers to the fact that if a household was surveyed, either the video played or did not play during that survey.

Figure 3: Demand for IBLI by sales window and premium discount (left panel) and IBLI knowledge across extension treatment group and survey round (right panel)



Note: 95% confidence intervals provided in figures are calculated clustering standard errors at the individual level.

4.2 Impact analysis

4.2.1 Descriptive statistics and balance tables

Table 3 uses baseline data to test for balance in the main variables of interest across treatment arms. A description of the variables' construction is found in Appendix C. Columns 1–4 provide the mean and standard error for each variable of interest at baseline, in each treatment arm. Columns 5–7 show the t-statistic from testing for differences between each treatment group against the control group (Column 1), and Column 8 includes the F-statistic from a universal test for differences between the groups. Note that no household reported having purchased IBLI coverage at baseline, which is expected as it was not yet available in the study region.

In three of the possible 24 cases, there are statistically significant differences (p-value < 0.1) between treatment arms. While this rate is not sufficiently high to question the validity of the randomization, it does point to the importance of controlling for household fixed effects or baseline levels in the analysis, which we do (with the exception of the IBLI purchase variables and the IBLI quiz score, as only a few respondents completed this during baseline).

Table 3: Balance table of primary outcomes in baseline data across treatment arms

	Control	Discount	Video	Both	t-test Difference			F-test[5]
	(1)	(2)	(3)	(4)				
Variable	Mean/SE	Mean/SE	Mean/SE	Mean/SE	(2)-(1)	(3)-(1)	(4)-(1)	
Q1 outcomes								
Has a positive impression of insurance	0.101 [0.013]	0.088 [0.013]	0.078 [0.013]	0.066 [0.012]	-0.013	-0.023	-0.036**	1.400
Aware of IBLI	0.053 [0.010]	0.048 [0.010]	0.029 [0.008]	0.041 [0.009]	-0.004	-0.023*	-0.012	1.203
Score on IBLI quiz[1]	0.523 [0.069]	0.620 [0.069]	0.558 [0.080]	0.493 [0.080]	0.096	0.035	-0.030	0.558

Variable	Control (1) Mean/SE	Discount (2) Mean/SE	Video (3) Mean/SE	Both (4) Mean/SE	t-test Difference (2)-(1)	(3)-(1)	(4)-(1)	F-test[5]
Q2 outcomes								
Any IBLI purchase				N/A				
Total number of polices purchased				N/A				
Q3 and Q4 outcomes								
Earnings IHS[2]	10.714	10.665	10.638	10.719	-0.048	-0.076	0.006	0.270
	[0.071]	[0.082]	[0.070]	[0.074]				
Income from milk IHS[2]	8.353	8.618	8.578	8.955	0.264	0.225	0.602***	2.306
	[0.168]	[0.161]	[0.172]	[0.149]				
Minimum dietary diversity for women[3]	1.984	1.983	1.980	1.923	-0.001	-0.005	-0.062	0.538
	[0.040]	[0.040]	[0.039]	[0.042]				
Food security	1.146	1.135	1.043	1.039	-0.011	-0.104	-0.108	1.328
	[0.049]	[0.050]	[0.051]	[0.051]				
CESD-10[4]	8.542	8.541	8.296	8.041	-0.001	-0.246	-0.501	1.105
	[0.226]	[0.221]	[0.228]	[0.227]				
N	513	475	446	441				

*Note: IHS = inverse hyperbolic sine. [1]N = 81 for this variable at baseline because the quiz was conditional on being aware of IBLI. [2]IHS transformed. [3]10-category dietary diversity score. [4]10-question CESD score. [5] F-test is for joint orthogonality. The values displayed for t-tests are the differences in the means across the groups. *** p < 0.01, ** p < 0.05, * p < 0.1*

4.2.2 Research analyses

The following estimations were performed using STATA®'s *xtreg* and *xttobit* commands (STATA 2017) and user written commands *ivreg2* (Baum, Schaffer, and Stillman 2007) and *xtivreg2* (Schaffer 2010) for STATA®.

Q1: To what extent can a standardized low-burden, client-targeted extension activity improve understanding of the product relative to standard extension activities provided by insurance agents?

The coefficient estimates for Equation 2 are presented in Table 4. There is strong evidence that the extension treatment increased the likelihood that respondents have a positive impression of insurance for pastoralists. The point estimate of that impact indicates a 21 percent increase (t-stat = 3.38) from the pooled control mean in the likelihood that the respondent believes insurance can be helpful to pastoralists.

There is also weak evidence that the extension video increases the likelihood of the respondent having heard of IBLI. The video treatment increased the likelihood that the respondent has heard of IBLI by three percentage points (t-stat = 1.81)—an increase of approximately 8 percent over the control mean. While an increase in awareness of the product is expected, its relatively small impact is unexpected, as respondents in the treated group had watched a video on IBLI about an hour prior to the survey.

We note that during the baseline only 4% of participants were aware that TIA was selling IBLI in their region; by midline, 75% of the video control group was aware that TIA was selling IBLI in their region and this increased to 80% by endline. We interpret these dynamics to mean that TIA's outreach activities, as well as other information channels,

had been mostly successful in creating awareness by the midline, which is when the video extension treatments started. The video treatments further improved awareness, but only marginally.

Further, the fact that between 25 percent (midline) and 18 percent (endline) of participants watched the video about IBLI and its availability through TIA, and then subsequently reported that they had never heard of IBLI being sold by TIA, suggest that either: (1) there was a communication issue related to the IBLI-awareness question (e.g., the enumerator used a different local name for it than that used in the video); or (2) some respondents did not understand the video at all.

The empirical model needed to be adjusted to assess the video's impact on the IBLI quiz score because in both baseline and midline, respondents only completed the quiz if they reported that they had heard of IBLI. Only 81 respondents completed the quiz at baseline, and only 1,291 completed it at midline. The result is that: (1) the completion of the quiz is likely correlated with product knowledge (non-random selection); and (2) we cannot use individual-level fixed effects in any regression that includes IBLI knowledge and the video, as video treatments began at midline and for respondents without a baseline quiz score, treatment is time-invariant.

Therefore, we provide the random effects estimates using data only from the midline and endline rounds and while controlling for round and *manyatta* averages. The point estimate shows a 5-point (t-stat = 4.06) or 8 percent increase in quiz score over the control.

In summary, the extension video improved respondents' perceptions of the potential value of insurance for pastoralists, increased the likelihood that they had heard of IBLI, and increased their understanding of the IBLI policy.

Table 4: Impacts of the extension treatments on perception of insurance and awareness and knowledge of IBLI

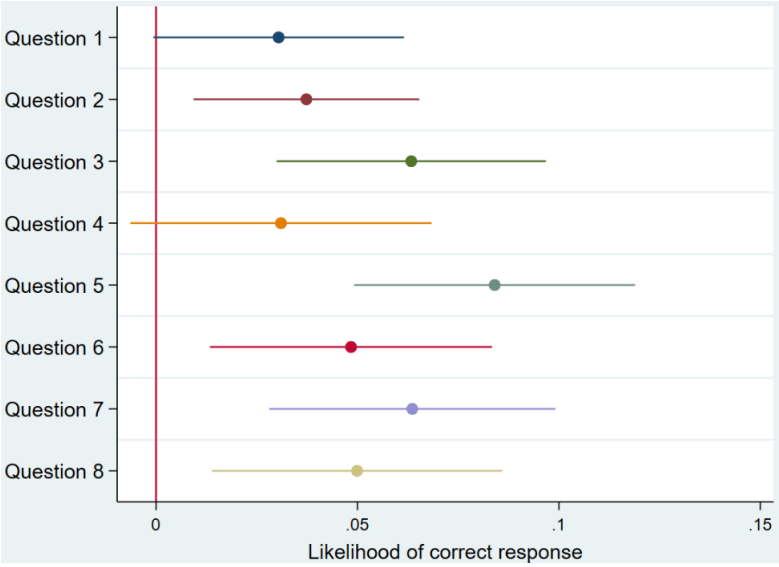
VARIABLES	A positive impression of insurance	Is aware of IBLI	IBLI quiz score
	(1)	(2)	(3)
Video treatment	0.0709***	0.0323*	0.0510***
	(0.0209)	(0.0178)	(0.0125)
Data periods	2018, 2020, 2022	2018, 2020, 2022	2020, 2022
Observations	5,219	5,277	2,901
Number of individuals	1,875	1,875	1,694
R-squared	0.393	0.604	
Time controls	Round	Round	Round
Fixed effects	Participant	Participant	Participant
Pooled control mean	0.336	0.383	0.620

*Note: Standard errors are robust, clustered at household, and presented in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.*

We then disaggregate the IBLI quiz score to determine whether the video impacts knowledge on specific quiz questions more than others. Figure 4 illustrates the estimated impacts and 95% confidence intervals of the extension treatment on the likelihood of

answering each quiz question correctly. Testing for differences in impacts, the impacts on Q5 (t-stat = 2.49), Q3 (t-stat = 1.72) and Q7 (t-stat = 1.67) are all statistically greater than that of Q4; however, there is little other variation observed and (generally speaking) the point estimates across all eight questions are positive, similar in magnitude, and most are statistically different from zero. At the same time, there is not a clear set of questions that appear much more sensitive to the extension treatment than the rest.

Figure 4: Impacts of extension video on individual test questions



Note: The eight questions are as follows: (1) Based on your understanding, what do you think IBLI is? (2) What type of insurance is IBLI/TIA? (3) Which of the following risks related to livestock does IBLI cover? (4) How often does an IBLI client have to pay a premium in order to remain insured? (5) If an IBLI client that purchased insurance does not receive an indemnity payout (compensation) from the livestock insurance by the time the contract ends, would they receive their premium back? (6) Based on your understanding of the livestock insurance, what measurements trigger an insurance indemnity payout? (7) What causes the insurance company to make a payout to a pastoralist with TIA insurance? (8) Based on your understanding of IBLI, what does the index upon which compensation is made, represent?

While the video increases product understanding, it does not do so in a significant way, with an increase in the mean quiz score from 62–67%. The discount coupons themselves, which will be important for our assessment of IBLI's impacts, may also increase product understanding through several channels: (1) by increasing recipients' interest in IBLI and therefore increasing their effort spent in obtaining, processing, and maintaining information on IBLI; (2) by increasing insurance agents' expected value of engaging with coupon recipients due to commission earned (i.e., participants with coupons are more likely to purchase and so insurance agents would be wise to prioritize visiting them); and (3) by increasing clients' IBLI purchases and resultant experience with the product.[15]

Figure 5 illustrates the mean quiz score across waves and by treatment arm. Note that

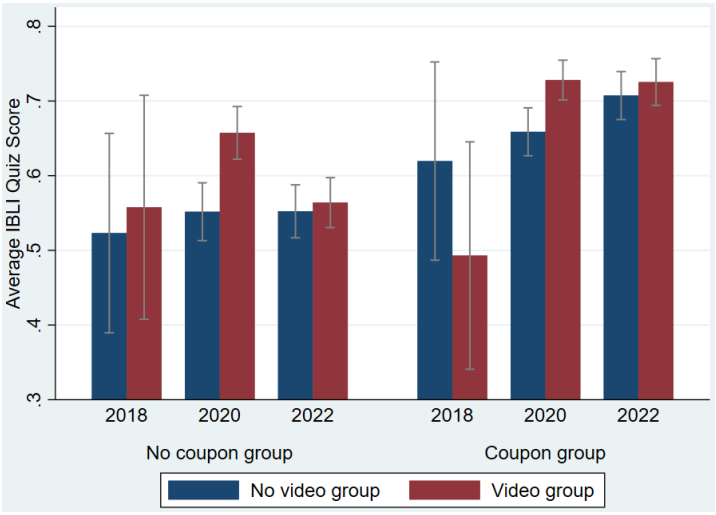
[15] The distribution of discount coupons during the survey took place well after the quiz was completed; therefore, the enumerators could not have impacted quiz scores through coupon distribution and related explanations.

the data from 2018 in this figure only covers the 81 respondents that had heard of IBLI at that point, and that the quiz in 2018 took place before any participants received either of the treatments. Figure 5 illustrates several features of learning in the sample:

1. In the absence of coupons or the extension video, quiz scores improved very little over the four years of the project—less than five percentage points on average—and changes mostly took place between the first and second round.
2. In the absence of discount coupons, the video caused a large improvement in quiz scores when it was first shown at midline (approximately 12 percentage points) but by endline the videos not only failed to further impact quiz scores, the earlier learning from the videos at midline seems to have been lost.
3. The discount coupons had a large and sustained impact on learning, likely through the channels described above.
4. The combination of the video and discount coupons seems to have helped to improve learning from providing coupons alone at midline, but this tapers off by endline.

In summary, the extension video seemed to have had a positive impact when it was first shown, but that impact did not persist, even with additional viewing. However, discount coupons had a strong and persistent impact on quiz scores.

Figure 5: IBLI quiz scores by treatment group before, during, and after treatments



There were also differences in how the three discount coupons work. Coupon A provides a full subsidy for up to four TLUs. Such free insurance may not incentivize a great deal of learning effort by clients (Channel A), but it will certainly incentivize agents to visit those participants (Channel B), and those receiving free insurance are the most likely to have insurance coverage, and, as a result, the most opportunity to learn through experience (Channel C).

To test for the impacts of the discount coupons on IBLI knowledge, and for heterogeneity in impacts across coupon type, we regress the IBLI quiz scores onto variables indicating extension treatment and coupon type. The results indicate that receiving a discount coupon increased knowledge of IBLI considerably: an average of approximately 13 percentage points, which is more than twice the effect size of the extension video (Table 5, Column 1). There is no evidence of heterogeneity in impacts across coupons.

To better understand how the coupons impact learning, we regress the IBLI quiz scores

on the video treatments as well as the coupons, but this time restricting the sample to the subset of 980 participants that never purchased IBLI (in this case, the coupons could not impact product knowledge through the product experience channel) (Table 5, Column 2). For this subsample, the coupons have no impact on product knowledge, while the extension video continues to have an impact similar to that on the full sample. The coupons' loss of impact among the never-purchasers is consistent with product experience (Channel C) playing a large role in product knowledge, as the discount coupons should have been able to impact product knowledge among the never-purchasers through Channels A and B if they had been important.

Table 5: Impacts of the extension video and randomized discount coupons on IBLI quiz scores

	(1)	(2)
VARIABLES	Full sample	Never-purchasers
Video	0.0493***	0.0631***
	(0.0120)	(0.0177)
Coupon A	0.137***	-0.0410
	(0.0163)	(0.0427)
Coupon B	0.135***	0.0368
	(0.0163)	(0.0375)
Coupon C	0.127***	0.0268
	(0.0160)	(0.0327)
Observations	2,901	1,560
R-squared		
Number of ids	1,694	980
Rounds	2020 & 2022	2020 & 2022
Round controls	Yes	Yes
Manyatta controls	Yes	Yes
Participant random effects	Yes	Yes
Control mean of quiz score	0.552	0.537

*Standard errors are robust, clustered at household, and presented in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.*

Q2.1 Does product understanding increase demand for IBLI?

Table 6 shows the estimated direct impacts of viewing the extension video on IBLI purchases, using treatment status and purchase data from all seasons during the study. Both binary and continuous demand variables are tested, first through a probit model of the binary purchase decisions (Table 6, Model 1), then through an OLS regression of the continuous purchase amount including zeros (Table 6, Column 2) and finally through a tobit model on the continuous purchase amount (Table 6, Column 3).

Recall that due to the relationship shown in Q1, these estimates can be interpreted as the intent-to-treat estimates of changes to product knowledge on purchase decisions, as there is no way for the extension video to impact purchases beyond the transfer of knowledge during the video, which was embedded in the survey. There is no indication that the extension video impacted the purchases.

Table 6: Impacts of extension video on IBLI purchases

VARIABLES	Any purchase (Probit) (1)	Coverage purchased (OLS) (2)	Coverage purchased (Tobit) (3)
Extension video	-0.00131	0.00759	-0.0266
	(0.0127)	(0.0667)	[0.0707]
Observations	8,787	8,787	8,787
R-squared	0.069	0.105	
Number of participants	1,875	1,875	1,875
Data periods	'18 '19 '20 '21 & '22	'18 '19 '20 '21 & '22	'18 '19 '20 '21 & '22
Household FEs	Yes	Yes	No
Year FEs	Yes	Yes	Yes

*Note: FE = fixed effect. Regressions also include the following regressors: relative forage index of the previous long-rain, long-dry season (March–October), relative forage index of the current short-rain, short-dry season (October–February), household type (sedentary, partially settled, or nomadic), if the household herds livestock, if the household grow crops, the number of household members, if the respondent is the household head, if the respondent has completed any education level, current participation in the KLIP program, and current participation in the REAP graduation program. Robust and household-clustered standard errors are presented in parentheses. Bootstrapped standard errors are presented in brackets. *** p < 0.01, ** p < 0.05, * p < 0.1*

Q2.2 Does product understanding change who purchases IBLI?

To test whether product understanding impacts who is purchasing IBLI, we rerun the regression that produced Table 6, but this time allow for heterogeneity in the impacts of the video on purchases by interacting it with each of the covariates. Table 7 includes the coefficient estimates of the interactions. Note that all the variables are also included in the regression without interacting them with the extension treatment (these estimates are not included here for the sake of brevity). In this case, the coefficient estimates shown in Table 7 measure and test for differences in how that covariate relates to IBLI purchases between those that watched the video and those that did not. While the controls are not exogenous, the video treatment is, so that the differences are causally attributed to the video.

Consistently across the three models, the extension video positively adds to the (yet unknown) impacts of discount coupons on purchases. The implication of this finding, considering the null results presented in Table 6, is that without the reduced price provided by the discount coupon, learning alone is an insufficient means of increasing demand for insurance; however, in conjunction with a much-reduced price, knowledge gained from the extension video does increase IBLI purchases.

Here, we note that increasing demand for insurance was not the objective of the extension treatment; rather, it was to improve participants' ability to understand whether insurance would be useful to them. Our hypothesis was that the product was more likely to help certain types of individuals, and that we would observe their purchase of more insurance coverage than their peers after viewing the video.

The regression results are consistent with an increase in informed purchases. However, rather than the individuals' characteristics driving the heterogenous impacts of the extension video, it is a variation in the effective insurance premium rate that generates

the largest difference in the likelihood of the product benefitting individuals.[16] The extension treatment increased IBLI purchases among those for whom insurance is highly subsidized (and therefore those most likely to benefit from insurance purchases). There is also evidence that the extension video decreased IBLI purchases among KLIP beneficiaries and respondents that were also household heads.

Table 7: Test for heterogeneity in the impacts of the extension video

VARIABLES	Any purchase (1)	How much purchased (TLUs) (2)	How much purchased (TLUs) (3)
Video (1 = yes)	0.0258	-0.0464	-0.250
	(0.0581)	(0.262)	(0.249)
Video * Received a discount coupon (1 = yes)	0.137***	1.086***	0.915***
	(0.0197)	(0.134)	(0.159)
Video * BOMA (1 = yes)	0.0402	0.161	0.279***
	(0.0256)	(0.118)	(0.129)
Video * Partially sedentary (1 = yes)	-0.00240	0.0683	0.0387
	(0.0196)	(0.126)	(0.101)
Video * Nomadic (1 = yes)	0.0219	0.0578	-0.0112
	(0.0478)	(0.209)	(0.209)
Video * KLIP recipient (1 = yes)	-0.148**	-0.523	-0.470
	(0.0737)	(0.397)	(0.516)
Video * Head of household (1 = yes)	-0.0535***	-0.225**	-0.139
	(0.0201)	(0.0945)	(0.0830)
Video * Respondent attended school (1 = yes)	0.00979	-0.113	-0.117
	(0.0296)	(0.125)	(0.170)
Video * Grows crops (1 = yes)	-0.0202	-0.111	-0.130
	(0.0255)	(0.159)	(0.162)
Video * Herds animals (1 = yes)	0.0348	0.148	0.245
	(0.0312)	(0.132)	(0.104)
Video * Number of household members	-0.00181	-0.0136	-0.0133
	(0.00462)	(0.0189)	(0.0137)
Video * Previous forage index	-0.0676	-0.212	-0.109
	(0.0528)	(0.208)	(0.185)
Video * Current forage index	0.0627	0.0555	-0.0208
	(0.0473)	(0.242)	(0.231)
Observations	8,787	8,787	8,787
R-squared	0.079	0.119	
Number of id	1,875	1,875	1,875
Data periods	'18 '19 '20 '21 & '22	'18 '19 '20 '21 & '22	'18 '19 '20 '21 & '22
FEs	Household & year	Household & year	*Manyatta* & year

Note: FE = fixed effect. Regressions also include the following regressors: relative forage index of the previous long-rain, long-dry season (March–October), relative forage index of the current

[16] For example, with Coupon A, the net cost of purchase comprises only the transaction cost; the expected value of insurance with a premium rate of zero will always be greater than zero.

*short-rain, short-dry season (October–February), household type (sedentary, partially settled, or nomadic), if the household herds livestock, if the household grow crops, number of household members, if the respondent is the household head, if the respondent has completed any education level, current participation in the KLIP program, and current participation in the REAP graduation program. Robust and household clustered standard errors are presented in parentheses. Bootstrapped standard errors are presented in brackets. *** p < 0.01, ** p < 0.05, * p < 0.1*

Q3. What are the impacts of IBLI?

Table 8 provides the individual fixed effects IV estimates of the impacts of IBLI purchases in the previous year on current outcomes. The main variable of interest is a binary indicator that is equal to one if the respondent purchased IBLI coverage during either of the sales seasons in the 12 months before the survey was conducted. There is no indication that IBLI has had any positive impacts on these outcomes. The Kleibergen-Paap rk Wald F statistics are included in the bottom row of the table and indicate that the analysis is unlikely to produce biased estimates due to weak IVs, as they are not weak.

Table 8: IV estimates of the impacts of IBLI coverage

VARIABLES	Livestock expenditures* (1)	Earnings* (2)	Milk income* (3)	MDD-W (4)	Food insecurity (5)	CESD-10 (6)
IBLI coverage (1 = yes)	-0.436 (0.477)	0.298 (0.418)	-0.0617 (0.551)	0.181 (0.151)	-0.212 (0.210)	0.237 (0.846)
Observations	5,071	5,071	5,071	5,071	5,071	5,071
R-squared	0.107	0.058	0.089	0.024	0.039	0.025
Number of ids	1,735	1,735	1,735	1,735	1,735	1,735
Data periods	2018 2020 2022	2018 2020 2022	2018 2020 2022	2018 2020 2022	2018 2020 2022	2018 2020 2022
Individual FEs	Yes	Yes	Yes	Yes	Yes	Yes
Round FEs	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap Wald F-stat	525.4	525.4	525.4	525.4	525.4	525.4

*Note: FE = fixed effect. *Variable is the inverse hyperbolic sine of the KES figure. Regressions also include the following regressors: relative forage index of the previous long-rain, long-dry season (March–October), relative forage index of the current short-rain, short-dry season (October–February), household type (sedentary, partially settled, or nomadic), if the household herds livestock, if the household grow crops, lagged number of household members, if the respondent is the household head, if the respondent has completed any education level, lagged participation in the KLIP program, and lagged participation in the REAP graduation program. Standard errors are robust, clustered at household, and presented in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.*

Q4. Does better understanding of the IBLI product increase the impact of insurance?

To answer this final question, the outcomes in Table 8 are tested once more. The analysis again tests the impact of IBLI coverage in the previous period on outcomes in the survey period. This time, we allow those impacts to vary between those treated with the extension video and those who were not.

Recalling that the theory of change hypothesizes that the main channel through which the extension video could impact individuals is by helping them to make purchase decisions more likely to benefit them, we include the full set of interactions with the video that were tested in Research question 2.2 and used in Table 7 in the first stage for purchases. These interactions then allow the video to have heterogenous impacts on purchase decisions (i.e., in the first stage of the IV process) across household and environmental conditions.

Table 9 provides the results of this analysis. The main finding is that IBLI has no impact on households, irrespective of their knowledge of the product.

Table 9: IV estimates of the impacts of IBLI coverage, allowing for variation across product knowledge

VARIABLES	(1) Livestock expenses*	(2) Earnings*	(3) Milk income*	(4) MDDS	(5) Food security	(6) CESD-10
IBLI coverage (1 = yes)	-0.593	0.597	-0.201	0.191	-0.287	-0.188
	(0.531)	(0.468)	(0.610)	(0.165)	(0.236)	(0.928)
IBLI coverage*Extension	0.679	-0.840	0.121	-0.0513	0.446*	1.486
	(0.515)	(0.561)	(0.603)	(0.170)	(0.252)	(0.962)
Estimated impact of IBLI coverage with extension treatment	0.0820	-0.243	-0.0800	0.140	0.158	1.298
	(0.502)	(0.516)	(0.557)	(0.171)	(0.236)	(0.951)
Observations	5,071	5,071	5,071	5,071	5,071	5,071
R-squared	0.107	0.060	0.090	0.024	0.038	0.026
Number of ids	1,735	1,735	1,735	1,735	1,735	1,735
Data periods		2018	2018	2018	2018	2018
	2018 2020	2020	2020	2020	2020	2020
	2022	2022	2022	2022	2022	2022
Individual FEs	Yes	Yes	Yes	Yes	Yes	Yes
Round controls	Yes	Yes	Yes	Yes	Yes	Yes
Kleibergen-Paap Wald F-stat	34.80	34.80	34.80	34.80	34.80	34.80

*Note: MDDS = minimum dietary diversity score. *Variable is the inverse hyperbolic sine of the KES figure. Analysis includes the full set of controls and interactions listed in Table 7. Standard errors are robust, clustered at household, and presented in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.*

4.2.3 Heterogeneity of impacts

Our main research question is one of heterogeneity; do the impacts of insurance vary across product knowledge? This research question was tested in Section 4.2.2. We reject heterogeneity across product knowledge.

5. Cost analysis

The total cost of the intervention is calculated as the total subsidies used plus the cost of developing the extension videos. Table 10 details the subsidies for each sales window. The total subsidies used by the randomized controlled trial was approximately US$ 70,000.

Table 10: Subsidies by sales window

Year	Sales season	Total subsidy (KES)	Total subsidy (*US$)
2018	Jan/Feb	1,266,096	11,888
2018	Aug/sept	357,317	3,355
2019	Jan/Feb	938,487	8,812
2019	Aug/sept	209,060	1,963
2020	Jan/Feb	1,932,520	18,146
2020	Aug/sept	533,895	5,013
2021	Jan/Feb	1,996,413	18,746
2021	Aug/sept	94,858	891
2022	Jan/Feb	-	-
Total (KES)		7,328,644	68,814

Note: [*]Using the mean exchange rate for 2020, which was US$ 1 = KES 106.5.

The total cost of developing the extension video is calculated as one day of staff time for the Principal Investigator to generate the video content, and one week of staff time for a research associate to generate, translate and record the video content. The total cost for this was less than US$ 2,000. No special software was used.

Distribution of the video during the project did entail considerable expense, but the distribution activities within this intervention are unrepresentative of how the insurance firm would engage with farmers. The main project expenses included training staff and transporting them to remote participants' households. The insurance agents do not normally travel to these remote households; rather, they usually take advantage of natural aggregators such as livestock markets or celebrations, or call interested individuals into a central location for outreach activities.

The purpose of the video was to provide insurance agents with a resource to share with prospective clients, who could easily take away a digital copy on their phones if they had them. Sharing digital files between phones is nearly free of cost, particularly when using Bluetooth-enabled file sharing applications or SD memory cards (methods commonly used in the region).

In this case, the only cost would be a short training for insurance agents on how to use and share the video. Within Samburu County, TIA has about 12 insurance agents and it costs them approximately US$ 30/agent/day and US$ 200/trainer/day for a half day training. Therefore, we estimate the total cost of training TIA's insurance agents in Samburu County to show and distribute the video during their outreach activities to be approximately US$ 560 each year, or US$ 2,240 for the four-year project.[17]

[17] An alternative would be to calculate the cost of the project. This alternative approach would include the costs and time related to hiring enumerators and paying them according to ILRI's policies rather than using TIA's insurance agents and transporting enumerators to participants' households (where surveys were collected and the extension treatments implemented).

Using this approach (which we do not believe is relevant for anything beyond determining the cost of these activities within a specific research design and context), the total cost of disseminating the videos in 2020, 2021, and 2022 was approximately US$ 41,000. This was estimated by first

We therefore estimate that the total four-year cost of the subsidies used by the project, plus the cost of generating the extension video and providing all VIPs in Samburu County with annual training on its use and dissemination, to be approximately US$ 74,355. Given that our analysis did not identify benefits from either intervention, the cost benefit analysis cannot meaningfully progress beyond this point.

6. Discussion

This research agenda aims to test whether low-cost, digitally enabled, and shareable extension treatments could improve understanding of an insurance product and the role that product understanding plays in the impacts of insurance. We hypothesized that improved product knowledge would: (1) change who purchased insurance by helping prospective clients to more accurately assess the value of the product for their particular circumstances; and (2) change how those with insurance coverage responded to it, by improving their ability to accurately integrate their coverage into their production decisions. To do this, we implement a randomized controlled trial, randomizing premium discounts and a low-cost video extension treatment in a factorial design among pastoralists in northern Kenya

We find that watching a short nine-minute video improves product knowledge and perceptions of the potential for insurance to help pastoralists. Further, among those for whom insurance premiums were highly subsidized (which dramatically increases the expected net benefit of purchasing insurance), the extension video increases insurance uptake. These findings support the assertion that the extension video increases the likelihood of purchase by those for whom the benefit is greatest, although not exactly in the way that we had anticipated. There is little evidence of the type of improved sorting among other observable household characteristics or environmental conditions that we had hypothesized.

There is no evidence that IBLI coverage has a positive impact on any of the outcomes that we examine. This null finding holds both for those who received the extension video and those who did not.

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calculating the portion of the implementation costs for the midline in 2020, the coupons distribution in 2021, and the endline in 2022 that were related to the video distribution.

The portion used is calculated as the fraction of total time spent on the extension videos over the total time on activities. For example, the total survey time used to collect the 1,737 midline surveys (which included the extension video) was 144,446 minutes (about 83 minutes per survey).

The extension video was 8 minutes and 53 seconds long. We assume that the video process itself required about one minute to explain, and therefore estimate that, for those that received it, the extension video added about 10 minutes to their survey process. A total of 886 individuals received the extension video, which we estimate added a total of 8,860 minutes to the total survey process and accounted for about 6 percent of the survey time, and is therefore responsible for about 6 percent of the survey costs. This represents an upper bound for the reasons stated above, and because they assume that costs of training enumerators are evenly spread across survey time, which they are not (the training on how to play the video is minimal compared to the training on how to define a household).

6.1 Policy and program relevance: evidence uptake and use

The lack of evidence on IBLI impacts suggests that policymakers and those working to support pastoralists should be cautious in using funds to support improved insurance access. As has been stated many times, insurance itself, and IBLI specifically, are imperfect products that cannot and will not fully protect households from shocks or move them out of poverty on their own. While other research has found that IBLI has positive impacts on households, our findings suggest that IBLI is not effective in all locations or all conditions. Policymakers should be sure to run pilots and effective impact evaluations before making large investments in scaling IBLI into new and unproven locations.

The impact of the extension videos on those with large subsidies highlights a concept that has long been indicated in the information/extension field: simple digital tools can be effective but often are not. In this case, simple digital educational tools were effective in increasing product knowledge, but that knowledge was quickly lost among those that did not also receive high subsidies, presumably because they were less likely to seriously consider the product. The implication is that simple digital tools may be an effective means of information transfer, but this transfer alone is insufficient to instill lasting knowledge.

6.2 Challenges and lessons

There have been several challenges related to operation during the pandemic (SARS-CoV-2). Generally, the policies intended to slow the spread of the virus restricted movement, as well as group sizes and the capacity of buildings and vehicles. These restrictions increased the costs of operations across the board and, importantly, reduced the movement and outreach activities performed by insurance agents.

The 2020–2022 regional drought also created serval challenges. By the 2022 project endline, many households had been facing drought conditions for more than a year, which had several implications for the study. Operationally, it meant tracking down households that had migrated in search of greener pastures or water for their livestock.

More importantly, it pushed many of our participants into tragic circumstances: many households lost livestock; their remaining livestock were too emaciated to produce milk, sell, or slaughter; businesses started with the help of local NGOs failed; informal wage opportunities dried up; and the prices of staple goods increased. The situation seems to have been too dire for products such as IBLI or other livelihood-supporting interventions to protect against it.

Indeed, our analysis of the BOMA Project's graduation program, which made large asset transfers to participants, showed large positive impacts as of 2020; however, those impacts reduced considerably by 2022, even for those who had just completed the two-year program.

7. Conclusions and recommendations

At a high level, our results suggest that IBLI did not produce significant benefits for those with coverage during this project, which is contrary to considerable evidence from other projects that have assessed the impacts of IBLI and other agricultural insurance projects.

This inconsistency could originate from several channels including those related to location, product, clients, and circumstances (e.g., weather).

While we cannot know which channel is driving this discrepancy, it highlights the fact that evidence building and learning are cumulative processes, and that there are likely many differences in important and unobserved characteristics between studies. It also suggests that caution should be taken when assuming that programs will have similar impacts across space and time, even when there is considerable evidence to draw from.

At the IBLI level, our findings have two main implications. The first is that while IBLI has been shown to benefit clients in several circumstances, it did not have a positive impact between 2018–2022 in Samburu—a time that encompassed not only severe drought conditions in 2020 and 2021, but also COVID and conflict, all of which resulted in critical conditions across our study sample in 2020 and 2022.

The implication is that for the participants of this study, an intervention providing only IBLI was clearly unable to address or even mitigate the impacts of the onslaught of shocks they were facing. Clearly, study households and the general population in this region needed much greater support than IBLI, or other interventions operating in the region, were able to provide.

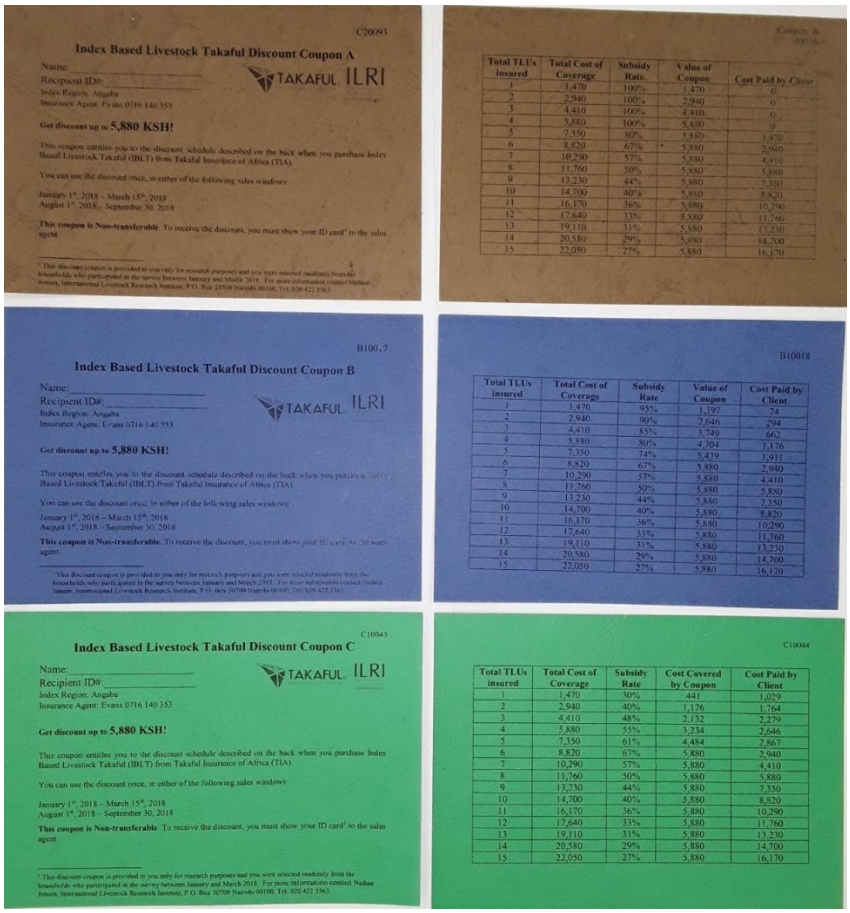
Appendix A: Extensions videos and discount coupons

We implemented two versions of the extension videos. The first video used the standard livestock framing that focuses on the impacts of drought on forage, which puts livestock at risk. The second uses household framing and focuses on the impacts of drought on income and therefore resources available for households to meet their basic needs during drought. Testing for variation in outcomes between the two videos falls outside the scope of the research presented here. The videos can be found at the following links.

- Livestock framing: https://drive.google.com/file/d/1bC6AXw8QzwyCCQYoqubwgtAlXc5yAcY0/view
- Household framing: https://drive.google.com/file/d/1frjgdArdEPxS3jObTYfuyf6VDtdtQk7m/view

Three different types of discount coupons were distributed. Coupon A provides a flat 100% discount for the first four TLUs insured. Coupon B provides a 95% discount on the first TLU and decreasing discounts on the subsequent five TLUs, so that the client pays full cost for the seventh TLU. Coupon C provides a 30% discount on the first TLU insured and then the discount increases gradually to 95% for the sixth TLU, then falling to zero. Households could use the coupons to purchase as much or as little insurance as they liked. Figure A1 includes images of the three types of discount coupons. Testing for variation in outcomes between the three coupons falls outside the scope of the research presented here.

Figure A1: Images of the discount coupons



Appendix B: Survey instruments and other evaluation tools

The baseline survey tool is available at the following location: https://drive.google.com/file/d/1iU5HDY5aF2GmHD3PtkswcA6vZzOcbPer/view?usp=sharing

The midline survey tool is available at the following location: https://drive.google.com/file/d/1NK0FYEoicvj6cgjeA2DocINQTgPHbGkE/view?usp=sharing

The endline survey tool is available at the following location:

https://drive.google.com/file/d/1RHiJ762uf6fGnPifnBeAyWAkDVmifcm1/view?usp=sharing

Appendix C: Variable construction

Question 1 outcomes	
Has a positive impression of insurance	A binary variable that is equal to one if the respondent responded to the question “Do you think that insurance can be helpful?” with “Good and relevant: It can be helpful to pastoralists like me,” or “Good and somewhat relevant: It can be helpful to other pastoralists, but not me.” The other options are “Good and irrelevant: It can be helpful to other people but not for pastoralists,” “Bad and irrelevant: It is not helpful for normal people,” and “Very bad: It is not helpful for anyone except the insurance companies.”
Aware of IBLI	A binary variable that is equal to one if the respondent had heard of IBLI.
Score on IBLI quiz	The respondents’ ratio [0,1] of correct responses to an eight-question quiz on IBLI policies and how they function.
Question 2 outcomes	
Any IBLI purchase	A binary variable that is equal to one if the respondent purchased insurance in the 12 months following the survey or treatment.
Coverage purchased	The amount of insurance purchased in TLUs (the units in which IBLI is sold). The units are calculated as follows: 1 camel = 1.4 TLU, 1 cow = 1 TLU, 1 sheep = 0.1 TLU, and 1 goat = 0.1 TLU.
Questions 3 & 4 outcomes	
Livestock expenses (inverse hyperbolic sine [IHS])	The total amount spent by the household on livestock inputs over the previous 12 months. Expenses include animal fodder, supplementary feeds for livestock, water for livestock, vaccinations and medications for livestock, herders’ wages, transportation for livestock, and “other.” Livestock expenses are transformed using the IHS to reduce the impacts of extremely high values on regression estimates.
Earnings (IHS)	Total annual earnings as reported by season (long-rain, short-rain, long-dry, short-dry) and livelihood activity (including cash and in-kind earnings). The earnings are transformed using the IHS to reduce the impacts of extremely high values on regression estimates.
Income from milk (IHS)	The total annual income from milk was constructed by multiplying household-reported milk productivity by the number of animals lactating by average ward milk prices. Productivity and number of lactating animals are reported by animal type and season (long-rain, short-rain, long-dry, short-dry). Milk income is transformed using the IHS to reduce the impacts of extremely high values on regression estimates.

Minimum dietary diversity for women	Respondents are asked if they had eaten anything from each of the WHO 16-food-group categories in the last 24 hours. These 16 food groups are then transformed into 10, as described by FAO and FHI 360 (2016).
Food security	Response to the question “In the LAST 7 DAYS, how many days did YOU go to sleep at night without an evening meal because there was not enough food?”
CESD-10	The sum of scores generated from a set of 10 questions. The domain of CESD-10 is [0,30]. A score of 10 or higher has been used to indicate likely depression.
Randomized treatment variables	
Premium discount	A binary variable that is equal to one if the household received the premium discount. Note that the randomization for discounts was performed only once at the beginning of the study, and discounts were distributed once each calendar year.
Extension video	A binary variable that is equal to one if the household received the extension video. Those selected to receive the extension video were treated in 2020, 2021, and 2022.

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