Long-run Effects of Catastrophic Drought Insurance*

Christopher B. Barrett[†] Nathan Jensen[‡] Karlijn Morsink[§]
Yuma Noritomo[¶] Hyuk Harry Son[‖] Rupsha Banerjee[**] Nils Teufel[††]

October 2024

For the latest draft, *please click here*

Abstract

Aggregate shocks such as droughts can have negative long-run impacts on various well-being indicators. Formal insurance against such covariate shocks can mitigate these adverse consequences. We study the long-run impacts of catastrophic drought insurance on pastoralists in Kenya and Ethiopia. We leverage randomized premium discounts distributed when insurance was first introduced to estimate the impact of insurance on outcomes 10 ten years later. Insurance induced a shift in production strategies: an increased herd share of larger animals, such as camels and cattle, and a sharp decrease in smallstock like goats. The share of household members who completed age-appropriate education increased significantly. This seems to result from both reduced demand for child labor due to herd composition shifts – the marginal productivity of child labor is lower herding large rather than small livestock – and positive income effects. The long-run effects we observe arise entirely from reduced *ex ante* risk exposure and the behavioral change it induces, not from the cash indemnity payments insured households receive *ex post* of drought. The results are robust to a range of prospective confounders, including controlling for prospective spillover effects among households.

Keywords: Africa, human capital, index insurance, livestock, pastoralists, production strategies

[†]Charles H. Dyson School of Applied Economics and Management and Jeb E. Brooks School of Public Policy, Cornell University, USA. Email: cbb2@cornell.edu

[‡]Global Academy of Agriculture and Food Systems, University of Edinburgh, UK. Email: njensen@ed.ac.uk

[§]Utrecht University School of Economics, The Netherlands. Morsink is also affiliated with the Development Economics Group at Wageningen University, The Netherlands, the Charles H. Dyson School of Applied Economics and Management, Cornell University, USA, and the Center for the Economics Analysis of Risk at Georgia State University, USA. Email: k.morsink@uu.nl

[¶]Charles H. Dyson School of Applied Economics and Management, Cornell University, USA. Email: yn266@cornell.edu

[‖]Utrecht University School of Economics, The Netherlands. Email: h.son@uu.nl

[**]International Livestock Research Institute, Nairobi, Kenya. Email: R.Banerjee@cgiar.org

[††]International Livestock Research Institute, Nairobi, Kenya. Email: N.Teufel@cgiar.org

[0]*Data were collected by a consortium of the International Livestock Research Institute (ILRI), Cornell Uni-

1 Introduction

Uninsured exposure to catastrophic aggregate shocks such as droughts and other natural disasters have negative long-run impacts on lifetime well-being, as reflected in education, health, assets, and labor-market outcomes (Maccini and Yang, 2009; Dinkelman, 2017; Shah and Steinberg, 2017; Carrillo, 2020). Multiple mechanims may explain these adverse effects. When shocks occur, people suddenly facing severe financial liquidity constraints may liquidate productive assets and reduce human capital investment, with especially detrimental effects when it happens early in life (Jensen, 2000; Alderman, Hoddinott, and Kinsey, 2006). Uninsured exposure to disaster risk may also induce risk averting behaviors, discouraging investment in strategies that promote growth (Boucher, Carter, and Guirkinger, 2008; Karlan et al., 2014; Emerick et al., 2016). In the presence of multiple equilibrium poverty traps there might not be any recovery if a disaster pushes the household into a low-level, poor equilibrium (Lybbert et al., 2004; Kraay and McKenzie, 2014; Barrett, Carter, and Chavas, 2019). While the literature points to insurance market failures as an important cause of disaters' adverse effects (Lybbert et al., 2004; Karlan et al., 2014; Barrett, Carter, and Chavas, 2019) evidence on the longer-run impacts of insurance remains lacking.

We present evidence on the 10-year, long-run effects on income, assets, production strategies, and human capital accumulation of an insurance product against catastrophic droughts, offered to pastoral households in the arid and semi-arid lands (ASAL) of northern Kenya and southern Ethiopia. We find that insurance uptake changes production strategies, inducing an increase in the herd share of large animals – camels and cattle – at the expense of small animals, particularly goats. These herd shifts result in sizeable, significant increases in crop income and large, albeit impre-

versity, Syracuse University, the University of California at Davis, the University of Sydney, and the Institute of Developing Economies-JETRO, supported financially by the US Agency for International Development (USAID) Agreement No. LAG-A-00-96-90016-00 through Broadening Access and Strengthening Input Market Systems Collaborative Research Support Program (BASIS AMA CRSP), the Australian Department of Foreign Affairs and Trade through the Australia Development Research Awards Scheme award "The human and environmental impacts of migratory pastoralism in arid and semi-arid East Africa", JSPS Grant-in-Aid for Scientific Research (B)-26301021, the UK Department for International Development(DfID) through FSD Trust Grant SWD/Weather/43/2009, the Agriculture and Rural Development Sector of the European Union through Grant agreement No: 202619-101, USAID Grant No: EDH-A-00-06-0003-00, the World Bank's Trust Fund for Environmentally and Socially Sustainable Development (Grant No: 7156906), the CGIAR Research Programs on Climate Change, Agriculture and Food Security and Dryland Systems, the CGIAR Standing Panel on Impact Assessment, the CGIAR Research Program on Livestock, and the Foreign, Commonwealth & Development Office Project "Extreme Poverty - Building Evidence for Effective Action" through Oxford Policy Management Limited (Award Number: POR008864). This research was approved by Institutional Review Boards at Cornell University (Protocol ID No 0907000655, 1203002881, 2008009760) ILRI (IRB approval number: ILRI-IREC2015, ILRI-IREC2020-53), and NACOSTI(NACOSTI/P/20/7050). We thank seminar audiences at Colgate, Cornell, European University Institute, Centre for the Study of African Economies 2024, International Conference of Agricultural Economists 2024, Japanese Association of Development Economics, Oxford Policy Management, UC Davis, Utrecht, Wageningen, and Yonsei, Michael Carter, John McPeak, and Dean Yang for helpful comments.

cisely estimated, increases in livestock income despite no significant change in the total value of productive assets 10 years later. This implies a boost to household herd productivity. The change in herd composition is accompanied by a substantial and significant increase in household members' educational attainment. The herd composition and education impacts appear closely linked. Children are far more likely to herd small animals than large animals, so the induced change in herd composition reduces the marginal productivity of child herding labor. This creates incentives to send children to school – similar to mechanisms described in Shah and Steinberg (2017) and Bau et al. (2024) – potentially magnified by positive income effects. Indeed, we show that the herd composition effects temporally precede the educational impacts, reinforcing the hypothesis that induced changes in household derived demand for child labor plays a role in observed educational gains. Moreover, we find that insurance coverage that reduces *ex ante* risk exposure fully explains these behavioral changes and long-run well-being effects, not the cash transfers resulting *ex post* of drought onset after the insurance triggers indemnity payments.

Investigating the long-run effects of insurance against aggregate shocks has to date been impeded by the fact that most catastrophic insurance programs in low-income rural communities have proved short-lived. Agricultural indemnity insurance is often fraught with moral hazard, adverse selection and high transaction costs, while index insurance products have typically remained at pilot scale due to low product quality and implementation challenges (Binswanger-Mkhize, 2012; Mobarak and Rosenzweig, 2013; Carter et al., 2017; Jensen and Barrett, 2017; Hill et al., 2019). A notable exception is the Index-Based Livestock Insurance (IBLI) program.[1] IBLI relies on a satellite-based Normalized Difference Vegetation Index (NDVI) indicator of relative forage scarcity – specifically designed to minimize basis risk in this system – to insure herders against catastrophic herd losses associated with droughts (Chantarat et al., 2013). Since piloting in northern Kenya in 2010, IBLI has gradually expanded. As of December 2022, over 560,000 households in three countries (Ethiopia, Kenya, and Zambia) have been individually insured through IBLI (Jensen et al., 2024b). Recent initiatives by the governments of Kenya, Ethiopia, Djibouti and Somalia, supported by the World Bank, aim to scale IBLI further to reach 1.6 million pastoralists by 2025 (The World Bank, 2022).[2] Given that the program has run for many years, and was originally introduced through an experiment with a panel household survey, IBLI uniquely allows for investigation of the long-run impacts among a low-income population of insurance against catastrophic droughts.

To investigate these long-run impacts, we conduct a 10-year follow-up panel survey with the

[1]Unlike most agricultural index insurance products, which insure against low annual crop yield realizations, IBLI insures against the loss of durable assets, in this case livestock, like most commercial insurance products worldwide.

[2]Beyond those five countries, IBLI is also employed for macro-scale sovereign drought insurance in Kenya and Mauritania. For more background details on IBLI, see Jensen et al. (2024b).

original baseline sample of pastoral households from Kenya (in 2009) and Ethiopia (in 2012). We leverage the individual-level randomized distribution of insurance premium subsidies – that happened immediately after the baseline – to 1,439 pastoralists from 33 locations. In each location, a random sample of individuals was randomly assigned to receive premium discount coupons that were distributed just prior to each of six sales seasons between 2010 and 2015. The coupons were non-transferable, expired at the end of the sales season, and were re-randomized each sales season. The coupons provided households with a discount on the insurance premium for a maximum of 15 Tropical Livestock Units (TLUs).[3] The same households were then surveyed annually for three rounds in Ethiopia and four rounds in Kenya, up to 2015. During the period 2009-2015, low NDVI readings triggered the drought index four times in Kenya and one time in Ethiopia, resulting in indemnity payments to policyholders. No randomized premium discounts were provided nor any surveys were conducted after 2015, until we conducted the 10-year follow-up survey with original panel households in 2020 in Kenya and in 2022 in Ethiopia. Last mile supply constraints among the insurance companies limited the take-up of the insurance in our study communities after the experimental period (Jensen et al., 2024a).[4]

We leverage randomized insurance premium discounts distributed during the initial years of IBLI to estimate the Local Average Treatment Effect (LATE) of insurance purchase on our pre-specified outcomes.[5] We causally identify the long-run impacts of any insurance purchase, instrumenting insurance purchase in the first three sales seasons by the number of discount coupons received during that initial exposure period.[6] Our pre-specified primary outcomes are assets (i.e., herd size), total cash income, production strategies (i.e., herd composition), and human capital accumulation (i.e., maximum education level of household members), chosen because aggregate shocks have been demonstrated previously to negatively affect these outcomes. Our pre-specified secondary outcomes are recent insurance uptake and short-run impacts observed during or immediately after the experiment period: herd management expenditures, annual milk income (cash only), livestock loss, distress sale of livestock, and the share of children working.

[3]Tropical Livestock Unit (TLU) is an integrated unit for aggregating cattle, camel, sheep, and goats by typical live body weight and basal metabolic nutrient intake requirements.1 TLU = 0.7 Camel = 1 Cattle = 10 Sheep/goats

[4]The research team had provided the last mile marketing and outreach for the commercial underwriters during the 2010-15 period, including providing transport to the 33 study locations for insurance sales agents. When the field research ended after the 2015 survey rounds, the insurers did not reliably offer IBLI in our study villages, even as they continued to sell IBLI in other villages where the insurers had arranged and financed the last mile sales and outreach from 2010-15. Thus while IBLI has continued, even expanded overall outside our study villages since the study period, in our study villages sales paused. IBLI was effectively a temporary intervention in these 33 villages prior to our 10-year follow up visits.

[5]See AEARCTR-0011184 at https://www.socialscienceregistry.org/trials/11184.

[6]As discussed below, this provides the strongest instrument while maintaining monotonicity of the relationship between the instrument and the endogenous regressor.

The long-run effects of catastrophic drought insurance are striking. We observe a sharp shift in herd composition – a 72 percent reduction in the share of goats herded and a corresponding increase in larger animals herded, significant at the five percent level. For households with less than 20 TLUs at baseline,[7] the herd composition change corresponds to a reduction of 18 goats herded. While the average household herd size and total cash income do not change significantly, we find large but imprecisely estimated increases in in-kind livestock income and significant increases in in-kind crop income, suggesting improvements in productivity. Furthermore, we find substantial and significant increases of 40%-55% – relative to the control group – in the maximum, total and average education of household members who were school-aged (5-17 years old) during the experiment, significant at the ten, five and five percent level, respectively. We also observe a more-than-doubling of the share of current children studying full time, from about 16 percent in the control group to 42 percent for households with insurance, significant at the ten percent level.

Several of IBLI's short-run effects that were identified during and immediately after the experiment period (Jensen, Barrett, and Mude, 2017; Janzen and Carter, 2019; Matsuda, Takahashi, and Ikegami, 2019; Noritomo and Takahashi, 2020) – on total herd size, herd management expenditures, livestock loss, and distress sales of livestock – disappear at this longer-run horizon. We also do not find a significant long-run effect of insurance on recent insurance uptake in the 12 months before the endline, consistent with the observation that the commercial insurers ceased supplying our study villages when the research team stopped providing supporting last mile logistics (Jensen et al., 2024a).

Beyond a range of standard robustness checks to alternative controls and outcome variables, we also investigate the robustness of our results to potential interpersonal spillovers. In the original experiment, households within communities were randomized to either receive discount coupons or not. Spillovers in the first- and second-stage of our IV strategy – for example through informal risk-sharing arrangements between treated and untreated individuals that might affect and be affected by insurance uptake – may violate the Stable Unit Treatment Values Assumption (SUTVA) and/or the exclusion restriction on which our causal identification relies. Therefore we leverage exogenous variation across communities in discount coupons received by peers, to estimate potential spillovers in our first- and second-stage IV estimation as a robustness check on our core results. We find that our second-stage outcomes on education, herd composition and whether or not children study full-time remain robust to controlling for potential interpersonal spillovers, although they become less precisely estimated in some specifications.

We explore candidate mechanisms that might explain these long-run outcomes. We rule out the

[7] This represents the lowest 67% of the baseline herd size distribution

possibility that initial insurance uptake during the experiment induced IBLI coverage at endline. Indeed, because insurance company supply constraints in our study villages effectively precluded insurance purchase after the experimental period, the observed effects must arise from transitory insurance exposure. In a context previously shown to exhibit multiple equilibrium poverty traps (Lybbert et al., 2004; Barrett et al., 2006; Santos and Barrett, 2011; Santos and Barrett, 2019), one might expect transitory exposure to have permanent effects that should not exist in the absence of multiple equilibria. We therefore investigate the evolution of estimated effects over time, starting immediately after the third sales season ($\sim$ 1.5 years) through the end of the experiment, after the sixth sales season ($\sim$ 3 years). Herd composition changes began right away after IBLI's launch, 1.5 years after the introduction of insurance, and became significant after three years and continuing to grow thereafter. The production strategies effects preceded the effects on education, which appear only appear at the long-run follow-up. Temporary insurance coverage seems to have induced investment behavior change, resulting in rebalanced herd portfolios and greater investment in children's education.

Transitory IBLI uptake seems to have stimulated a behavioral shift, the effects of which unfolded over time, even after households dropped insurance coverage. These results are consistent with the observation that one incurs heightened, temporary risk in transitioning production strategies from a more liquid, lower-productivity portfolio to a higher-productivity but less liquid one. If drought strikes after one has liquidated goats and before the larger species has had time to calve, lactate and safely establish their young, one runs a heightened risk of catastrophic loss because calves and pregnant or lactating cows are especially vulnerable to dehydration, disease, and malnutrition (Shrum et al., 2018). IBLI seems effective in facilitating pastoralists' transition to a more desirable herd portfolio.

The effects on herd composition and educational attainment are substantively linked. Larger animals like camels and cattle are more productive per TLU – through milk and calves produced – and thus generate more income, for which we provide suggestive evidence. While this positive income effect may have helped incentivize educational investment, the herd composition change and reduction in the number of goats herded also reduced household derived demand for child labor. Children are far less likely to herd large animals like camels or cattle, than they are goats or sheep. This would have increased incentives to educate children, and especially boys, who are most commonly involved in herding small animals. The significant educational impacts are indeed concentrated among boys, not girls.

IBLI's effect on herd composition may have arisen due to a reduced need for risk-averting behaviors such as holding goats as precautionary savings[8] or investing in lower-risk but lower-return assets such as goats instead of camels or cattle.[9] We find that long-run effects are attributable entirely to *ex ante* behavioral effects induced by insurance coverage that reduces catastrophic risk exposure, and not at all from the *ex post* impacts of the large cash transfers insured households received as indemnity payments triggered by (exogenous) low NDVI readings during droughts. This is consistent with prior findings of subjective well-being gains from insurance coverage even in the absence of payouts (Tafere, Barrett, and Lentz, 2019), as well as *ex ante* effects of insurance on increases in productive investments, irrespective of indemnity payments (Karlan et al., 2014; Cole and Xiong, 2017; Jensen, Barrett, and Mude, 2017; Hill et al., 2019; Matsuda, Takahashi, and Ikegami, 2019; Boucher et al., 2021; Stoeffler et al., 2022).

Multiple data sets from this context have previously found multiple equilibrium poverty traps reflected in threshold herd sizes below which self-reinforcing path dynamics trap households in poverty in expectation (Lybbert et al., 2004; Barrett et al., 2006; Santos and Barrett, 2011; Santos and Barrett, 2019).[10] During those earlier periods, negligible opportunities existed for non-poor livelihoods without livestock (Little et al., 2008; McPeak, Little, and Doss, 2011). Our finding that insured households - especially those with smaller herds - change herd composition away from goats and invest in children's education is consistent with the hypothesis that households have begun to perceive labor markets for educated workers as an alternate escape route from poverty traps in this region. As such we connect suggestively to the literature on multiple equilibrium poverty traps (Lybbert et al., 2004; Kraay and McKenzie, 2014; Barrett, Garg, and McBride, 2016; Banerjee et al., 2019; Barrett, Carter, and Chavas, 2019; Balboni et al., 2022). Uninsured catastrophic risk esposure can be a key driver of those poverty traps (Barrett, Carter, and Chavas, 2019; Santos and Barrett, 2019). We demonstrate that the behavioral change induced by even transitory insurance coverage yields durable, growing gains over time, consistent with multiple equilibrium poverty traps theory.

We also build on the literature on the long-run impacts of uninsured exposure to covariate extreme weather shocks, which routinely finds negative effects on education (Maccini and Yang, 2009; Shah and Steinberg, 2017; Carrillo, 2020; Bau et al., 2024), health (Maccini and Yang, 2009; Dinkelman, 2017; Carrillo, 2020), assets (Maccini and Yang, 2009), and labor market out-

[8]Goats are often referred to as "cash with four legs," a highly liquid, non-lumpy asset, with an average value of roughly USD 10, commonly sold to cover modest expenses (McPeak, Little, and Doss, 2011). Given average IBLI purchase volumes, we can rule out that the sale of goats to pay for insurance premiums explains any more than a very small – i.e., $\sim 10\%$ – of the estimated effect.

[9]Camel and cattle are lumpy – at USD 120-250 per head average asset value – implying an order of magnitude larger absolute loss in case of catastrophic weather shocks.

[10]Balboni et al. (2022) similarly find that transfers of lumpy productive assets such as cattle can help households escape poverty traps in Bangladesh.

comes (Carrillo, 2020). We demonstrate that insurance against catastrophic weather shocks has a positive effect on similar long-run outcomes through its *ex ante* effect on behaviors. Our results are most consistent with an interpretation akin to Shah and Steinberg (2017) and Bau et al. (2024), where insurance against catastrophic shocks, by changing production strategies, has an indirect effect on the marginal productivity of child labor, changing incentives for children to remain in school. That effect is reinforced by the positive income effects arising from induced changed production strategies.

We also connect to a nascent literature on the long-run impacts of development interventions (see Bouguen et al. (2019) for a review). Most evidence comes from either studies of human capital interventions or unconditional cash transfers and grant assistance. Human capital interventions[11] appear particularly effective at boosting long-run economic outcomes (Hoddinott et al., 2008; Banerjee, Duflo, and Kremer, 2016; Baird et al., 2016; Bandiera et al., 2017; Charpak et al., 2017; Barham, Macours, and Maluccio, 2017; Bettinger et al., 2018; Blattman, Fiala, and Martinez, 2020; Gray-Lobe, Pathak, and Walters, 2023). Studies of unconditional cash transfers and grant assistance consistently find large short-run effects, particularly on accumulation of assets, that dissipate over time, fading out in the long-run, much as our herd size effects do (Araujo, Bosch, and Schady, 2017; Baird, McIntosh, and Özler, 2019; Blattman, Dercon, and Franklin, 2022; Blattman, Fiala, and Martinez, 2020). We bridge these two literatures by exploring the long-run impacts of an intervention to insure against catastrophic covariate shocks, demonstrating the long-run importance of risk mitigation for human capital formation in particular.

We also build on a literature on the impacts of index insurance against aggregate weather shocks, which has so far focused on short-run impacts. Multiple studies find *ex ante* behavioral changes manifest as increases in productive investments (Karlan et al., 2014; Jensen, Barrett, and Mude, 2017; Cole and Xiong, 2017; Matsuda, Takahashi, and Ikegami, 2019; Hill et al., 2019; Belissa, Lensink, and van Asseldonk, 2020; Mishra et al., 2021; Stoeffler et al., 2022; Son, 2023). Prior studies also found that IBLI boosts income and smooths consumption *ex post* of drought shocks (Jensen, Barrett, and Mude, 2017; Janzen and Carter, 2019; Matsuda, Takahashi, and Ikegami, 2019; Noritomo and Takahashi, 2020). We contribute to this literature by demonstrating that long-run impacts also exist, but seem to arise entirely due to *ex ante* behavioral responses.

[11] Interventions that focus on de-worming, nutritional supplementation or prenatal interventions, sometimes combined with asset transfers, skills training or other economic interventions.

2 Context and Index-Based Livestock Insurance

The population in the northern Kenya and southern Ethiopia ASAL heavily depends on extensive livestock grazing - pastoralism - as the most productive livelihood strategy on infertile drylands (Little et al., 2008; McPeak, Little, and Doss, 2011; Jensen et al., 2024b). Households herd camels, cattle, goats, and sheep, and herd composition varies with the aridity of the location. The average baseline herd size was equivalent to 23 cattle.[12] On average, herds consist of 43% cattle, 33% goats or sheep and 23% camels. These animals play different roles in the productive strategies of households. Larger animals like camels and cattle are lumpy assets with values of USD 120-250 each. They are typically seen as investments yielding higher lactation rates, more valuable offspring, and greater social status. Goats are sometimes referred to as "cash with four legs," a highly liquid, non-lumpy asset, with an average value of roughly USD 10, commonly sold to cover modest expenses (McPeak, Little, and Doss, 2011).

The annual household-level nominal cash income of our survey households is similar at baseline and endline, roughly USD 1.3-1.5 per day, implying a substantial reduction in real cash income between our baseline and endline.[13] Over time, households substantially increase the share of cash income invested in herd management, specifically fodder, water, and veterinary expenditures, from about 10% at baseline to 25% at endline. Investing in veterinary services is a particularly effective strategy for reducing livestock mortality and for maintaining herd lactation rates, especially for large animals (Admassu et al., 2005; Homewood et al., 2006; Sieff, 1999; Santos and Barrett, 2011).

Only 10% to 15% of household heads ever went to school; the average completed education is approximately one year. Investments in education have, however, increased substantially over time. At baseline, the share of children aged 5-17 enrolled in school was 48.7 percent, but had increased to 61.3 percent at endline. Education outcomes are closely linked to the productive strategies of

[12] Hereafter we use cattle market value equivalents (CMVE) instead of TLU measures.Because our interest is in total herd size or herd size composition as a productive asset or as a store of wealth, we favor aggregation based on market value rather than on biophysical (i.e., nutrient intake) requirements, which is the basis for TLU. CMVE uses average sales prices by species in the 2010-22 survey data to establish species' relative average value.The average market values from our sales and purchases data are presented in Online Appendix Table G1. In Kenya, 1 CMVE= 0.625 camel=1 cattle=10 goats/sheep, and in Ethiopia, 1 CMVE=4 camel=1 cattle=6.25 goats/sheep. CMVE is strongly, positively correlated with TLU; the two measures just aggregate across species using different weighting schemes. All of our results are qualitatively invariant to the use of CMVE versus TLU.

[13] The endline-to-baseline cash income ratio is 531.70/498.44 = 1.07, the endline-to-baseline total income rate is 1114.45/1299.74 = 0.857, while the endline-to-baseline CPI ratio is 2.08 in Kenya and 2.99 in Ethiopia. Total income includes the value of in-kind livestock and in-kind crop income, which is more than double of cash income in these settings, as shown in Online Appendix Tables F1 and F2. Our total income estimates ignore prospective growth in the metabolic mass of livestock, which might occur with changing herd demographic profiles if distress sales fall (Janzen and Carter, 2019), although we suspect such effects, if any, are small.

these households. Children aged 5-17, especially boys, commonly help with herding, especially of goats and sheep. When children aren't studying full-time, a large share of them work. At baseline, 40 percent of school-aged children worked full-time, 28 percent part-time. At endline in Ethiopia, the share of children working full-time fell approximately 40 percent, from 47 to 28 percent, and the share of part-time working children decreased by 31 percent, from 26 to 18 percent.[14]

The pastoral households in our sample are vulnerable to catastrophic drought shocks. Drought-related starvation, dehydration and disease account for 47 percent of the livestock losses in the region (Jensen, Barrett, and Mude, 2016). Following droughts, pastoralists rebuild herds slowly, relying largely on biological reproduction supported by complex systems of inter-household livestock gifts and loans (McPeak and Barrett, 2001; Lybbert et al., 2004; Little et al., 2008; McPeak, Little, and Doss, 2011; Takahashi, Barrett, and Ikegami, 2019).

Informal insurance networks have been fraying in the region, however, in part because of seemingly more frequent and severe droughts that tax all households at the same time (McPeak, Little, and Doss, 2011; Huysentruyt, Barrett, and McPeak, 2009). The aggregate nature of droughts implies that many herders simultaneously seek to sell livestock with declining weight and productivity – which affect animals' value – thus livestock markets offer little income or wealth stabilization against drought shocks (Barrett et al., 2003). And prior to IBLI, financial services were largely unavailable in these areas (McPeak, Little, and Doss, 2011). As a result, herd accumulation has long been the key risk management strategy for ensuring that households can rebuild assets after catastrophic shocks, for the simple reason that greater pre-drought herd size is strongly associated with increased post-drought herd size (Lybbert et al., 2004; McPeak, 2005; Barrett and Swallow, 2006; Cissé and Barrett, 2018).

IBLI offers a novel means to manage catastrophic drought risk. Forage availability offers a key signal of drought in rangelands. So IBLI was designed around near-real-time measures of the Normalized Difference Vegetation Index (NDVI), a reliable signal of forage availability (Meroni et al., 2014; Prince, 1991; Tucker et al., 1985) and shown to be strongly correlated with livestock mortality in this region (Chantarat et al., 2013). NDVI is generated and provided freely every ten days by the United States Geological Survey (USGS) from global satellite data. IBLI uses an index that aggregates NDVI data within geographically defined index units in each of two annual seasons that characterize the region's bimodal annual rainfall pattern. Historic NDVI data for each insurance unit were used to develop a statistical distribution of drought outcomes. Insurers and reinsurers used those estimates to negotiate a strike level below which indemnity payments would be made (Chantarat et al., 2013; Vrieling et al., 2016). While the specifics of the IBLI policy

[14] Comparable enrollment data were not collected at endline in Kenya.

and the index that underpins it have evolved somewhat over time and differ slightly between the Ethiopia and Kenya sites, the core is uniform.[15]

IBLI piloted in Marsabit County, in northern Kenya, in January 2010 as a purely commercial index insurance product sold directly to individual pastoral households. A similar product was introduced in the neighboring Borana region of southern Ethiopia in August 2012. Immediately following our experiment, in 2015, the Government of Kenya added IBLI to its social protection programming by launching the Kenya Livestock Insurance Program (KLIP), which used public resources to purchase individual IBLI policies on behalf of vulnerable pastoralists. Households were, however, generally unaware of their status of coverage, and commercial IBLI was no longer sold in our study areas in Marsabit. In Borana, commercial sales were sustained at the same or higher volumes after the original pilot ended, but supply in our specific study locations nearly vanished. Effectively, once the initial IBLI experiment ended in 2015, the insurance companies underwriting IBLI ceased offering it for sale in our study sites.

3 Study design and econometric strategy

To study the long-run effects of catastrophic drought insurance, we leverage the original experimental design of seasonally randomized insurance premium discount coupons to 1,439 pastoralists from 17 locations in Borana Zone in Ethiopia and 16 locations in Marsabit County in Kenya. The 33 study locations were selected strategically to ensure representation across environmental conditions and remoteness. Household selection within those locations was random within baseline herd size strata, which is one of the most important predictors of resilience against shocks. These strata were obtained using household rosters from government administrative offices and stratifying these households into three categories according to household herd size. The sample size in each site was proportional to its total population, resulting in 924 households sampled in Kenya, and 515 households in Ethiopia.

Baseline household surveys took place in Kenya in the fourth quarter of 2009 and in Ethiopia in the first quarter of 2012, before IBLI's launch was announced in either country. The surveys captured a range of household demographic and economic data.[16] IBLI launched with the first follow-up survey round after the baseline in each location. Panel surveys of the same households

[15]See Jensen et al. (2024b) for richer details on the background, history and impacts of IBLI, including the evolution of contract design details.

[16]Additional details on the original research design, sample, survey tools and discount coupons can be found at ILRI's data portal: https://data.ilri.org/portal/dataset/ibli-marsabit-r1 and https://data.mel.cgiar.org/dataset.xhtml?persistentId=hdl:20.500.11766.1/FK2/S19DC6 for Kenya and https://data.ilri.org/portal/dataset/ibli-borena-r1 for Ethiopia.

were then conducted annually for three rounds in Ethiopia and four rounds in Kenya, up to 2015. Individuals in the sample were randomly assigned to receive premium subsidies through discount coupons that were distributed just prior to a sales season. These randomized discount coupons were non-transferable, expired at the end of the sales season, and were re-randomized in each of six sales seasons between 2010 and 2015. The coupons provided households with a discount on the insurance premium for a maximum of 15 TLUs. In each location in each round, 60 percent of the sample households randomly received a discount coupon providing a premium discount of 10-60 percent, at 10 percent intervals. During the experiment, low NDVI readings arising from drought triggered the index four times in Kenya and one time in Ethiopia, resulting in indemnity payments. We correct for measurement error in self-reported insurance purchase survey data using the insurers' administrative records.

No surveys nor experiments were conducted in these sites after 2015 until we conducted follow-up surveys in both countries with original panel households in 2020 in Kenya and in 2022 in Ethiopia to investigate the long-run impacts ten years after the original baseline. Figure 1 shows the timeline of the original pilots, discount coupon treatments, as well as the timing of the latest rounds of surveys in each country. Of the original 1,439 baseline pastoralists, we managed to re-survey 82 percent ten years later, a high retention rate given average annual attrition rates of 7.5 percent in panel surveys (Molina Millán and Macours, 2017)

Equation (1) offers a general Analysis of Covariance (ANCOVA) representation of how we model the long-run impacts of past and current insurance purchases, where y_{ijt} is outcome y for individual i, who lives in location j.[17] $t = 0$ refers to the baseline period, before any insurance was sold in location j, $t = 1$ refers to the first period when insurance was sold in location j, and $t = T$ is the final survey period, ten years after baseline. I_{ij1} refers to insurance purchase by individual i in the first sales period. X_{ij0} reflects a vector of household characteristics at baseline, and D_{ij} is a vector of the number of sales seasons during which the household received randomized insurance premium discount coupons.

$$y_{ijT} = f(I_{ij1}, ..., I_{ijT}, y_{ij0}, X_{ij0}, D_{ij}) \tag{1}$$

To causally identify the long-run impacts of insurance, we estimate the LATE of insurance purchase for our pre-specified outcomes, instrumenting for insurance purchase by the number of seasons in which the pastoralist received a discount coupon. As pre-specified, we restrict the analysis to discount coupons and insurance purchases in the first three sales seasons, as this provides

[17]Location refers to 16 sublocations in Kenya and 17 kebeles in Ethiopia. Locations are nested within distinct index insurance units within which the NDVI-based index determines whether an indemnity payment occurs.

Figure 1: Panel Timeline

Year/Season	Kenya Survey	Kenya Policy	Ethiopia Survey	Ethiopia Policy
2009 Oct-Nov	R1			
2010 Jan-Feb		Coupon1		
2010 Oct-Nov	R2			
2011 Jan-Feb		Coupon2		
2011 Aug-Sep		Coupon3		
2011 Oct-Nov	R3	Payout1		
2012 Mar-Apr		Payout2	R1	
2012 Aug-Sep		Coupon4		Coupon1
2012 Oct-Nov	R4			
2013 Jan-Feb		Coupon5		Coupon2
2013 Apr		Payout3	R2	
2013 Aug-Sep		Coupon6		Coupon3
2013 Oct-Nov	R5			
2014 Jan-Feb				Coupon4
2014 Mar-Apr		Payout4	R3	
2014 Aug-Sep				Coupon5
2014 Oct-Nov				Payout1
2015 Jan-Feb		Contract Shift		Coupon 6
2015 Mar			R4	
2015 Aug				Contract Shift
2015 Oct-Nov	R6			
2020 Aug-Sep	R7			
2022 Jan-Feb			R5	

Notes: The figure presents the timeline of the experiment in Kenya and Ethiopia. R1-R7 refers to the rounds of the panel survey. Coupon1-Coupon6 refers to the rounds where discount coupons were randomly assigned to recipients, and re-randomized every round. The discount coupons provided discounts on the insurance premium for purchase of coverage over a period of 12 months. Payout1-Payout4 refers to indemnity payments made to (some) recipients because the index was triggered in that season although payout 3 and payout 4 were triggered only outside of our study sub-locations. Contract shift refers to the moment when the IBLI contract underwent changes from asset replacement to asset protection.

a strong instrument (see Section 5). This approach does not, therefore, identify the effect of any changes in behavior during the period with randomized discount coupons in sales seasons 4 to 6, for which we control. We discuss these dynamics and potential mechanisms driving long-run impacts in Section 7.

Equations (2) to (5) describe the outcome and IV equations. We use an ANCOVA specification to estimate the LATE of insurance purchase on long-run outcome y in Equation (2), instrumenting for any insurance purchase using the number of discount coupons received by households in each of the first three sales seasons, from Equation (3). Equation (4) generates a binary variable that takes the value one if individual i purchased insurance during any of the first three sales seasons. Equation (5) aggregates the number of discount coupons received (Z) by an individual household i in location j in sales period t over the first three seasons ($t = 1,2,3$), yielding our instrument (D_{ij}). We control for the number of discount coupons received in sales seasons 4, 5, and 6 ($I_{ij4}^{t=6}$).

$$y_{ijT} = \beta_0 + \beta_{LATE}\widehat{I_{ij}} + \beta_1 y_{ij0} + \beta_2 X_{ij0} + \beta_3 D_{ij4}^{t=6} + \rho_j + \varepsilon_{ijT} \tag{2}$$

$$I_{ij} = \alpha_0 + \alpha_1 D_{ij} + \alpha_2 y_{ij0} + \alpha_3 X_{ij0} + \alpha_4 D_{ij4}^{t=6} + \rho_j + \mu_{ij} \tag{3}$$

$$I_{ij} = \begin{cases} 1 \text{ if there exists } t \in \{1,2,3\} \text{ such that } I_{ijt} > 0 \\ 0 \text{ otherwise} \end{cases} \tag{4}$$

$$D_{ij} = \sum_{t=1}^{t=3} Z_{ijt}^{D} \text{ where } Z_{ijt}^{D} = 1 \text{ if } D_{ijt} > 0 \tag{5}$$

We include location fixed effects to control for time-invariant, location-level unobservables. Because households rarely migrate on their own but rather travel together with their community members from the same location, location fixed effects effectively control for effects at broader grazing ranges that are episodically used by the households in each community j (McPeak, Little, and Doss, 2011; Huysentruyt, Barrett, and McPeak, 2009). Robust standard errors are used following Abadie et al. (2022) and De Chaisemartin and Ramirez-Cuellar (2024).

4 Balance and Attrition

Table 1 presents the mean and standard deviation of pre-specified balance variables, and baseline values of our pre-specified primary and secondary outcomes in each country and pooled, for the non-attrited sample of households (see below for attrition analysis).[18] We test for balance for each of our pre-specified balance variables, by whether or not a household received a discount coupon in each round in Appendix Table A1. We do not observe any significant differences per round, and normalized differences are below the threshold of 0.25 in 46 out of 48 tests. F-statistics for joint significance of all variables per round are insignificant, and so are F-statistics for joint significant of one variable across all rounds.

The right panel of Figure 2 shows that, on average, respondents purchased insurance 0.82 times. During the period of the experiment, coupons were offered six times, once or twice per year. Given that the product provides coverage for one year, the equivalent of full insurance coverage during the experimental period in Kenya would have been purchase of insurance three times, while in Ethiopia the equivalent of full insurance coverage during the experimental period would have been purchase of insurance 2.5 times. Just over half of the sample ever bought IBLI. The right panel of Figure 2 shows that 29% of respondents purchased insurance once, 14% twice, and 7% more than twice. The left panel of Figure 2 shows the distribution of the number of sales seasons in which pastoralists received discount coupons. On average, they received coupons 4.07 times. However, 52 percent of ever-purchased households purchased in the first sales season, 19 percent in the second sales season, and 11 percent in the third sales season. In total 83 percent of the ever-purchased households took up the insurance within the initial three sales seasons. We would exploit less variation if we use the full six sales seasons instead of the initial three sales seasons during which most purchases occurred. Therefore, we use the three initial sales seasons of insurance uptake and discount coupon receipts to identify the causal effects of insurance on our pre-specified primary and secondary outcomes.[19]

At the 10-year follow-up, we successfully re-interviewed 82 percent of the baseline households (1,179 out of 1,439 – Appendix Table A2). Attrition is not differential by our instrument, the number of coupons received during the initial three seasons, as shown in Appendix Table A3. Overall, households that are not male-headed, that have fewer adults, and that do not own agricultural land were more likely to attrit from the sample (see Appendix Table A4).[20]

[18] Appendix Table C1 presents the endline values of our pre-specified primary and secondary outcomes.

[19] 50 households (4.2 percent of the sample) purchased insurance before they received any discount coupons. Out of those 50 households, 14 purchased without receiving any coupons in any season, while 23 purchased in the very first sales season without receiving any coupons. Our results are robust to the exclusion of these 50 observations.

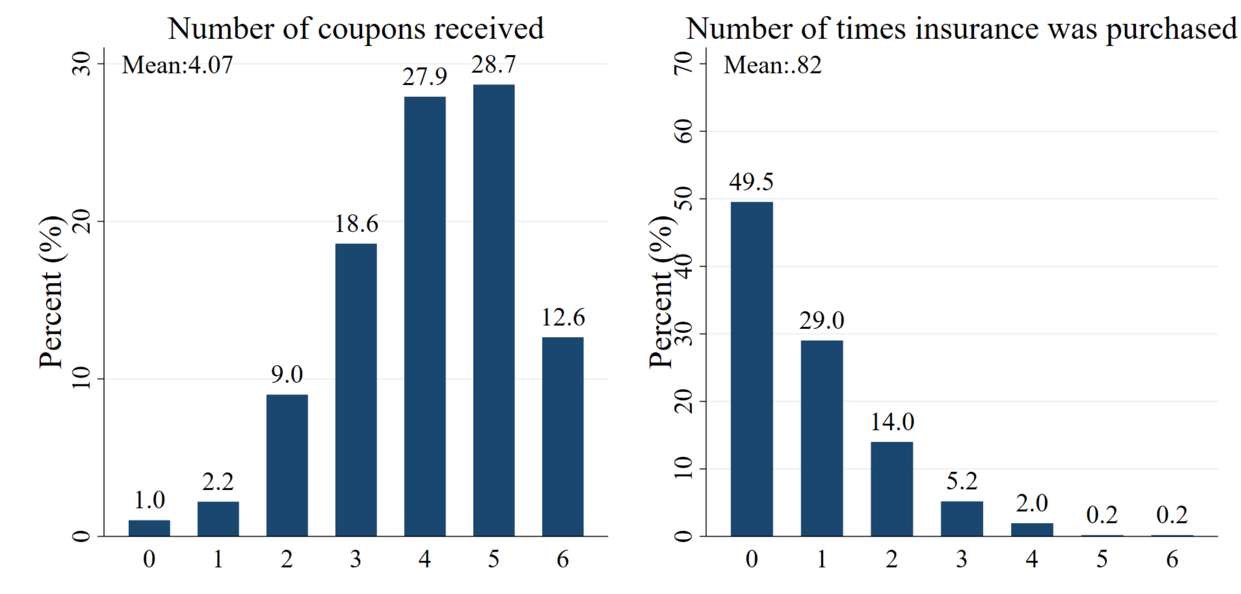
[20] We pre-specified two additional attrition tests. First, a joint test of selective attrition, which shows that only the

Table 1: Summary statistics of the baseline characteristics

	Kenya				Ethiopia				Pooled			
	Mean/SD	Min	Max	Obs	Mean/SD	Min	Max	Obs	Mean/SD	Min	Max	Obs
Prespecified household characteristics												
Age of household head in years	48.08	18.00	98.00	781	50.23	20.00	100.00	398	48.81	18.00	100.00	1179
	[18.35]				[18.30]				[18.35]			
Male headed household	0.63	0.00	1.00	781	0.79	0.00	1.00	398	0.68	0.00	1.00	1179
	[0.48]				[0.41]				[0.47]			
Household head's years of education	1.05	0.00	16.00	771	0.54	0.00	13.00	397	0.87	0.00	16.00	1168
	[3.07]				[1.84]				[2.72]			
Adult equivalent	4.68	0.70	12.90	781	4.94	1.40	14.90	398	4.77	0.70	14.90	1179
	[1.95]				[2.01]				[1.97]			
Dependency ratio	0.50	0.00	1.00	781	0.54	0.00	1.00	398	0.51	0.00	1.00	1179
	[0.21]				[0.19]				[0.20]			
Herd size (CMVE)	25.48	0.00	416.95	781	17.01	0.00	277.38	398	22.62	0.00	416.95	1179
	[35.98]				[23.90]				[32.64]			
Annual income per adult equivalent (USD)	121.45	0.00	1617.14	781	102.79	0.00	1639.55	398	115.15	0.00	1639.55	1179
	[198.01]				[159.19]				[185.95]			
Own or farm agricultural land	0.18	0.00	1.00	781	0.65	0.00	1.00	398	0.34	0.00	1.00	1179
	[0.38]				[0.48]				[0.47]			
Fully settled	0.23	0.00	1.00	781	0.76	0.00	1.00	398	0.41	0.00	1.00	1179
	[0.42]				[0.43]				[0.49]			
Baseline prespecified primary outcomes												
Share of camels in herd (CMVE)	0.30	0.00	1.00	730	0.12	0.00	0.98	395	0.23	0.00	1.00	1125
	[0.31]				[0.21]				[0.29]			
Share of cattle in herd (CMVE)	0.30	0.00	1.00	730	0.67	0.00	1.00	395	0.43	0.00	1.00	1125
	[0.36]				[0.25]				[0.37]			
Share of goats in herd (CMVE)	0.25	0.00	1.00	730	0.17	0.00	1.00	395	0.22	0.00	1.00	1125
	[0.26]				[0.18]				[0.24]			
Share of sheep in herd (CMVE)	0.14	0.00	1.00	730	0.05	0.00	1.00	395	0.11	0.00	1.00	1125
	[0.17]				[0.08]				[0.15]			
Annual total household cash earning (USD)	516.55	0.00	6877.83	781	462.92	0.00	5423.73	398	498.44	0.00	6877.83	1179
	[828.25]				[594.14]				[757.52]			
Maximum years of education	3.54	0.00	12.00	641	2.92	0.00	10.00	333	3.33	0.00	12.00	974
	[3.30]				[2.55]				[3.08]			
Baseline prespecified secondary outcomes												
Herd management expenditure (USD)	48.79	0.00	2395.60	781	41.00	0.00	2146.89	398	46.16	0.00	2395.60	1179
	[153.93]				[129.63]				[146.17]			
Annual milk income (USD)	886.09	0.00	12192.44	781	161.81	0.00	2496.61	398	641.59	0.00	12192.44	1179
	[1668.25]				[265.31]				[1408.51]			
Livestock lost in the past 12 months (CMVE)	11.05	0.00	116.90	781	9.20	0.16	200.60	343	10.49	0.00	200.60	1124
	[15.22]				[16.96]				[15.79]			
Number of camel lost in the past 12 months (CMVE)	1.15	0.00	61.00	728	0.28	0.00	6.00	343	0.87	0.00	61.00	1071
	[3.56]				[0.81]				[3.00]			
Number of cattle lost in the past 12 months (CMVE)	5.13	0.00	96.00	728	7.58	0.00	199.00	343	5.92	0.00	199.00	1071
	[11.40]				[16.04]				[13.11]			
Number of goats/sheep lost in the past 12 months (CMVE)	32.52	0.00	607.00	728	5.69	0.00	66.00	343	23.93	0.00	607.00	1071
	[55.13]				[8.67]				[47.39]			
Distress sale in the past 12 months (CMVE)	0.77	0.00	27.10	781	7.72	0.00	206.75	398	3.12	0.00	206.75	1179
	[2.03]				[19.66]				[11.99]			
Share of children working full-time	0.36	0.00	1.00	644	0.47	0.00	1.00	350	0.40	0.00	1.00	994
	[0.38]				[0.34]				[0.37]			
Share of children working part-time	0.29	0.00	1.00	644	0.26	0.00	1.00	350	0.28	0.00	1.00	994
	[0.39]				[0.32]				[0.37]			
Share of children studying full-time	0.22	0.00	1.00	644	0.12	0.00	1.00	350	0.18	0.00	1.00	994
	[0.36]				[0.23]				[0.32]			
Observations	781				398				1179			

Notes: The table presents the summary statistics – mean, standard deviation (in square brackets), minimum value, maximum value, and the number of observations of each variable – of the study sample, by country, and in total. Adult equivalent is the weighted sum of the household members as their adult equivalent, based on the following age-specific weights: A household member between 16 to 65 (AE=1), a child under 5 (AE=0.5), a child between 5 to 15 (AE=0.7), a household member above 65 (AE=0.7). The dependency ratio is calculated by dividing the number of dependents (household members younger than 15 years old and older than 65 years old) by the number of household members in adult equivalents. Herd size is the sum of the animals herded by the household, aggregated using cattle market-value equivalent. Cattle market-value equivalent (CMVE) is a unit to aggregate the animals across different animal types based on their market values using panel survey data. In Kenya, 1 CMVE= 0.625 camel=1 cattle=10 goats/sheep, and in Ethiopia, 1 CMVE=0.4 camel=1 cattle=6.25 goats/sheep. Annual total household cash earning is the sum of cash income from the following categories: sale of livestock, sale of livestock products, crop cultivation, salaried employment, casual labor, business and petty trading, and other major sources of income excluding gifts and remittances in the past 12 months. Herd management expenditure is the sum of the expenditure on water, fodder, supplementary feeding, and veterinary expenses.

Figure 2: Number of coupons received and the number of seasons with any insurance purchase



Notes: The left panel x-axis presents the number of coupons that respondents received during the six sales seasons in the experiment. The y-axis shows the percent of respondents who received 0, 1, 2, 3, 4, 5, or 6 discount coupons during these six sales seasons. The right panel x-axis presents the number of seasons that respondents purchased insurance. The y-axis shows the percent of respondents who purchased insurance 0, 1, 2, 3, 4, 5, or 6 times during these six sales seasons.

5 Results

We first examine the effect of randomized discount coupons on insurance purchase, the first stage of our IV strategy. Figure 3 presents the correlation between the number of times that a pastoral household received coupons during the six experimental rounds and the average number of seasons they purchased insurance. A strong, positive correlation exists (p-value$<$0.001). Table 2 presents the first stage estimation results of Equation (3). Columns 2 to 7 present the estimated effect of receiving a discount coupon on insurance purchase in each round. Columns 1 to 4 show that coupon receipt significantly predicts any insurance uptake during the first three seasons at the one percent level (Column 1), during the first season at the one percent level (Column 2), during the second season at the five percent level (Column 3), and during the third season at the five percent level (Column 4). There is no significant effect of the discount coupon on insurance purchase in any of the latter three seasons. We therefore choose the number of coupons that a respondent received during the first three seasons as our instrument.

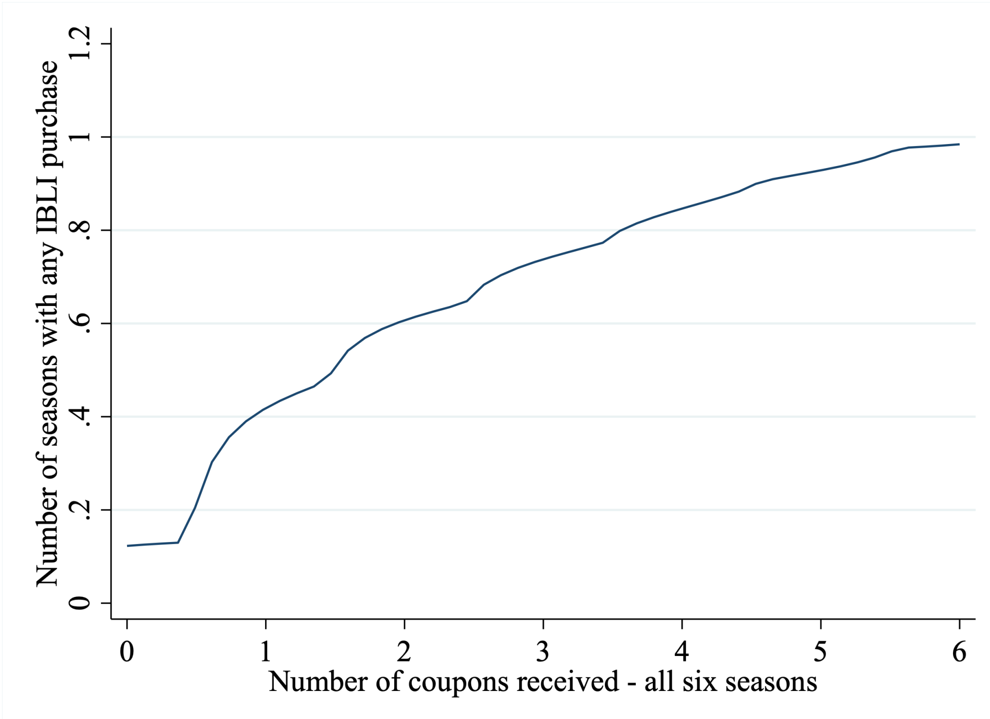
number of adults in the household significantly predicts attrition (Appendix Table A5). Second, a test for differential attrition per survey round shows that respondents that received a discount coupon are 5 percentage points less likely to attrit in sales season 3 (Appendix Table A6).

Table 2: First stage regression results

	Any insurance purchased – first three seasons	Any insurance purchased – per season					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
No. of coupons received – first three seasons	0.124^{***} (0.016)						
Coupon Receipt – first season		0.236^{***} (0.023)					
Coupon Receipt – second season			0.078^{***} (0.022)				
Coupon Receipt – third season				0.127^{***} (0.017)			
Coupon Receipt – fouth season					0.066^{***} (0.017)		
Coupon Receipt – fifth season						0.070^{***} (0.016)	
Coupon Receipt – sixth season							0.058^{***} (0.013)
Controls	✓	✓	✓	✓	✓	✓	✓
Effective F-statistics	57.374	106.329	12.878	55.462	15.587	19.502	19.669
10% Critical Value	23.109	16.380	16.380	16.380	16.380	16.380	16.380
N	1179	1168	1168	1176	1175	1173	1171

Notes: The table presents the coefficients from the first stage linear probability model regressions in columns, with robust standard errors in parentheses, as the individual-level was the level of randomization. Column 1 presents the estimated effect of the number of discount coupons received in the first three seasons on whether the respondent purchased any insurance in the first three seasons. The subsequent columns (Columns 2-7) present the estimated effect in each season of whether the respondent received a discount coupon on whether the respondent purchased any insurance. Community fixed effects are included as randomization was stratified at the community level. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. Row "Effective F-statistics" and "10% Critical Value" reports effective F-statistics and 10 percent critical values from Olea and Pflueger (2013) to test for weak instruments. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Figure 3: Correlation - IBLI purchase and coupon receipt



Notes: The x-axis shows the number of discount coupons the respondent received. The y-axis shows the estimated conditional likelihood that the respondent purchased insurance during any of the six seasons. The black line depicts the estimated relationship.

Column 1 of Table 2 presents the results of Equation (3), where we estimate the effect of the number of coupons received in the first three seasons on whether or not a respondent purchased any insurance during the first three seasons.[21,22] An increase in one additional coupon received in these first three seasons, significantly increases the likelihood that a respondent purchased insurance by 12.3 percentage points, which is significant at the one percent level. The effective F-statistics of Olea and Pflueger (2013) are greater than the critical value at the 10 percent level, providing support for the strength of our instrument.[23]

[21] In the pre-analysis plan we pre-specified the endogenous variable as the cumulative insurance purchase {0,1,2,3} in the first three seasons. However, this specification violates the monotonicity assumption that is required for valid instruments, because the number of times insurance is purchased does not increase monotonically with the number of discount coupons received (Appendix Table C2). When instead, we create a binary variable of whether or not the respondent purchased any insurance in the first three seasons, insurance purchase does monotonically increase with the number of discount coupons received, and we therefore use this endogenous variable.

[22] We do not include any analysis using the intensive margin of IBLI uptake – the CMVE of animals insured because the number of coupons received by respondents is not a significant predictor of this intensive margin uptake.

[23] Figure G1 in the Online Appendix shows a steady cumulative increase in uptake over the pilot sales seasons.

5.1 Long-run Effects of Catastrophic Drought Insurance

We report the results for all our pre-specified primary and secondary outcomes in Tables D1, D2, D3 and D4 in the Appendix. We observe no statistically significant effects of insurance on herd size,[24] annual household cash earnings, herd management expenditure, livestock loss, distress sales, livestock sales, full-time work and part-time work of children, nor the likelihood of purchasing insurance in the 12 months prior to the endline. The impacts on total milk income are statistically insignificant but quite large in magnitude. We observe significant changes in our pre-specified measures of herd composition, education, and the likelihood that children study full-time, which we discuss in detail now.

Table 3 shows a substantial change in insured households' production strategies, through changes in herd composition. Column 3 shows a decrease of 21.1 percentage points in the share of goats herded, significant at the five percent level, relative to a control mean share of 29.3, implying a 72 percent decrease. There are no changes in the share of sheep herded, so by construction the herd share of camels and cattle increased (Columns 1 and 2). Point estimates for camels and cattle are positive but noisy (p-value=0.236 and 0.197, respectively) when estimated independently, but significant when we aggregate large animals (cattle and camel) versus small animals (goats and sheep), as shown in Columns 5 and 6.[25]

While we observe no effects on mean total herd size (Table D1), mean herd size per species or the mean number of animals (Table C3), the herd size distributions are extremely skewed, with a small number of households owning a very large number of animals. Therefore Table C4 considers effects by baseline herd terciles. Panel A shows effects for households with herd sizes below 20 TLU at baseline (the lowest 67% of the baseline herd size distribution); panel B shows effects for herd sizes above 20 TLU. For households with herd sizes below 20 TLU that purchased insurance, we observe substantial reductions in the value of goats herded by 1.88 CMVE, and in the raw number of goats herded by 18, both significant at the ten percent level. The herd composition effects do not appear among those with the most livestock, whose herd size largely protects them from catastrophic drought driving them into a poverty trap (Lybbert et al., 2004; Barrett et al., 2006; Santos and Barrett, 2019). Rather induced change in production strategies occurs among the mass of poorer pastoralists whose modest-to-moderate herd sizes leave them extremely vulnerable.

[24] Appendix Tables E1 show that results are robust to using CMVE or TLU.

[25] We report the same outcomes, but using coupon receipt from all six sales seasons as the IV instead, in Appendix Table G2. The effects are similar.

Table 3: Long-run effects of catastrophic drought insurance on herd composition

	Outcome: N of animal type in CMVE / Total N of animals in CMVE					
	Camel	Cattle	Goats	Sheep	Camels & cattle	Goats & sheep
	(1)	(2)	(3)	(4)	(5)	(6)
Any insurance purchased	0.104	0.106	-0.211**	0.005	0.209*	-0.209*
	(0.088)	(0.081)	(0.094)	(0.050)	(0.112)	(0.112)
Controls	✓	✓	✓	✓	✓	✓
Control mean	0.255	0.311	0.293	0.141	0.566	0.434
Observations	987	987	987	987	987	987

Notes: The table presents estimated Local Average Treatment Effects of any insurance purchase in the first three seasons, instrumented by the number of discount coupons received in the first three seasons on herd composition. The dependent variable "herd composition" is measured as the number of animals of each animal type that the household herds expressed in CMVE divided by the total number of animals that the household herds expressed in CMVE. Community fixed effects are included as randomization was stratified at community level. Standard errors are clustered at the household-level, as this was the level of randomization. Data includes 987 of the 1179 households excluding households that are not currently herding any livestock. The row "Control Mean" indicates the average outcomes for those who did not receive any coupons in the first three seasons. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

We do not observe increases in annual household cash earnings nor in cash milk income, our pre-specified income measures. However, increases in productivity, and thus income, would logically be expected to accompany a transition from small to large animals, even with no change in total herd size. Therefore, and because the east African pastoral economy relies heavily on in-kind earnings, we do a thorough analysis of dis-aggregated in-kind and cash income.[26] While many of income component measures are noisily estimated, several estimates suggest substantial increases in livestock income, consistent with induced productivity gains (Appendix Tables C5 and C6). In-kind milk income triples among insured households, relative to households in the control group, while in-kind earnings from slaughtered animals double, and in-kind crop income increased manifold; the crop income effects are even significant at the one percent level.

Columns 1-3 of Table 4 present IBLI's effects on education, as measured by the maximum, total and average years of education of household members who were school-aged during the experiment.[27] We observe a substantial increase of 2.9 years in the maximum years of education, compared to 7.3 years in the control group, a 40% increase, significant at the ten percent level.[28] Column 2 shows that insured households have 7.3 years more total education among school-aged household members than households in the control group, who have 13.3 total years of education, a 55% increase, significant at the five percent level. Finally, Column 3 shows a 2.5 year increase in the average education of school-aged household members in insured households, significant at the ten percent level, compared to 5.3 years in the control group, a 48% increase.[29]

Consistent with results on education, we also observe a substantial increase of 42.3 percentage points in the likelihood that children study full-time, relative to a control mean of 15.9 percent (*p*-value 0.092). We also observe large and noisy but insignificant negative point estimates for children working. Children's full-time and part-time work fall by an estimated 36.3 and 20.2 percentage points, respectively, relative to a control mean of 34.5 and 20.8. These latter results are consistent with the increase in full-time study and education.[30]

[26]Appendix Tables F1 and F2 detail the construction of in-kind income variables.

[27]Our pre-specified measure of education is the maximum years of education of all household members, which significantly increases by 1.6 years, from a control mean of 6.61, a 24.2 percent increase, which is marginally insignificant with a *p*-value of 0.135. Of course the years of education for household members that were above school-age during the experiment cannot increase, and this adds noise to our estimates of the effect of insurance. Therefore we construct education measures for household members who were school-aged during the experiment.

[28]The sample size drops to 742, the number of endline households with school-aged members at some point during the experiment.

[29]We report the same outcomes, but using coupon receipt from all six sales seasons as the IV instead. The results in Table G4 shows that the effects are similar.

[30]Figure C1 shows that child time use appears related to the number of goats, positively correlated with the share of children working full-time, and negatively correlation with the share studying full-time.

Table 4: Long-run effects of catastrophic drought insurance on education

	Of household members who were school-aged at any point during initial three periods of experiments			Share of children in the household		
	Maximum years of education	Total years of education	Average years of education	Working full-time	Working part-time	Studying full-time
	(1)	(2)	(3)	(4)	(5)	(6)
Any insurance purchased	2.906*	7.314**	2.520**	-0.363	-0.202	0.423*
	(1.544)	(3.704)	(1.276)	(0.274)	(0.231)	(0.251)
	[0.074]	[0.074]	[0.074]			
Controls	✓	✓	✓	✓	✓	✓
Control mean	7.255	13.275	5.296	0.345	0.208	0.159
Observations	742	742	742	376	376	376

Notes: This table presents the estimated Local Average Treatment Effect (LATE) of any insurance purchase in the first three seasons, instrumented by the number of discount coupons received in the first three seasons, on education outcomes. The dependent variables "Maximum years of education", "Total years of education", and "Average years of education" (Column 1-3) are measured among household members who were school-aged at any point during the initial three periods of experiments, i.e., household members who are currently 15-29 years old in Kenya and 15-17 years old in Ethiopia (data in Ethiopia is limited to those up to 17 years old). The dependent variables "Working full-time", "Working part-time", and "Studying full-time" (Column 4-6) were only measured at endline in Ethiopia, and represent the share of current children aged 5-17 in the household that are reported to be engaged in each activity. Data includes 742 of the 1179 households for Columns 1-3, excluding households without household members who were school-aged during the experiment. Community fixed effects are included as randomization was stratified at community level. Standard errors are clustered at the household-level, as this was the level of randomization. The FDR adjusted p-values (q-values) are reported in square brackets for outcomes that were not pre-specified, calculated according to the sharpened process Anderson (2008). The row "Control Mean" indicates the average outcomes for those who did not receive any coupons in the first three seasons. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

If the shift in production strategies - in particular, away from herding small animals – drove the education results, the effects should arise predominantly among boys, who most commonly herd goats and sheep. Table C7 presents the disaggregated results for males in Panel A and females in Panel B. The effects we find indeed appear driven by boys.[31,32,33]

6 Robustness to interpersonal spillovers

Beyond the various robustness checks already mentioned, we consider the possibility that interpersonal spillovers might violate the assumptions on which causal identification depends in our design. Given that randomization occurred at the individual-level, within communities, we want to test for robustness to intra-community spillovers. Such spillovers could imply that the take-up or outcomes measured in control households are influenced by the discount coupons received, insurance take-up or outcomes of treated households. The presence of informal risk-sharing and informal transfers makes such spillovers plausible (Mobarak and Rosenzweig (2013), Riley (2018), Anderberg and Morsink (2020), Takahashi, Barrett, and Ikegami (2019), Berg, Blake, and Morsink (2022)).

The original experiment randomized households within communities, each season, to either receive discount coupons or not. At the level of individuals in one community this thus creates random variation in the intensity of encouragement received by peers. If we pool individuals in the sample across communities, across-community variation in the intensity of the instrument of both the recipient and their peers can be leveraged to investigate spillovers. We investigate these potential spillovers in the first stage – so from peers' discount coupons received on recipients' insurance purchase and vice versa – and in the second stage – from recipients' insurance purchase on peers' outcomes and vice versa. One challenge, given that our research was not designed to measure spillovers, is that the randomization within communities implies that coupon receipt by the recipient and their peers' are mechanically negatively correlated. Given the fixed pool of coupons within a community, if one respondent received a coupon, their peers were (slightly) less

[31] We also test for effects on household members who were too young to be in school during the experiment, but who were school age by endline. Table C8 shows positive but insignificant effects on the maximum, total, and average years of schooling for this younger cohort, with p-values of 0.146, 0.824, and 0.229, respectively.

[32] To determine whether the educational effect is influenced by changes in household composition, Columns 1 and 2 in Table C9 show no effect of insurance purchase on the number of young adults in the household. So as to test for selective out-migration of higher educated young adults, we estimate correlations between the baseline average education of young adults, and the share of young adults at endline. The positive and significant estimates on baseline average education of young adults suggest that no such selection occurred; if anything, endogenous household composition would bias our estimated educational effects downwards.

[33] Table C11 shows the estimated relationship between children's educational attainment and the endline-baseline change in small ruminant holdings. The increase in education is greatest for those who lost the most goats or sheep.

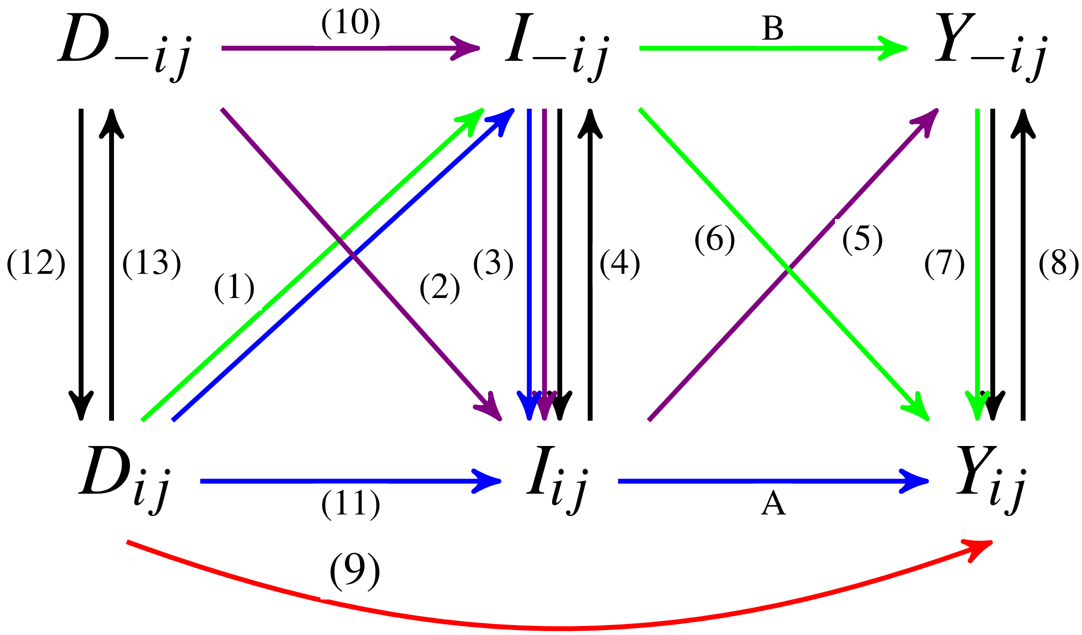
likely to receive one. This also implies that the value of "discount coupon received" for households who are recipients of coupons will always be mechanically larger than the "discount coupons received" by their peers (Guryan, Kroft, and Notowidigdo, 2009; Caeyers and Fafchamps, 2020). This implies that we cannot include community fixed effects (Fruehwirth, Iyer, and Zhang, 2019; Rahman, 2023). We can only check if our main results are robust to potential spillovers, but we cannot quantify or even reliably sign the direction of spillovers, given that they are not separately identifiable from the mechanical correlation.

To explore the possibility of confounding due to spillovers, we first identify the potential spillover pathways. These are graphically represented by Figure 4. Let D_{ij} denote discount coupon receipt by herder i residing in community j, I_{ij} represent insurance purchase, and Y_{ij} denote the long-run outcome of this herder. Note that there exists a group of other herders, $-i$, whom we refer to as "peers," also from community j. We define D_{-ij} as the peers' discount coupon receipt, I_{-ij} as the peers' decision of whether or not to buy insurance, and Y_{-ij} as the peers' long-run outcome. For this analysis, we assume that no inter-community spillovers exist. The blue line A represents the main causal effect of interest, namely the impact of i's IBLI purchase on $i's$ long-run outcomes. Since insurance purchase is endogenous, we use exogenous variation created by the randomized discount coupons D_{ij} as an instrument (pathway (11)) to estimate the LATE. The red arrow presents a direct violation of the exclusion restriction, the green and purple arrows present spillovers in the first and second stage, out of which the green ones can lead to violations of the exclusion restriction. Black arrows present mechanical correlations generated by our experimental design. For a detailed description of all the spillover pathways, including examples, please see Appendix B.

To control for the potential confounding of spillovers empirically, we construct proxies for D_{-ij} and I_{-ij} for each respondent i.[34] We do so by taking the mean of the number of coupons received and the mean of insurance purchase by all peers in the community. Following the same logic we also create a vector of control covariates for all peers in the community. Table B2 shows the results of the first-stage estimates. Columns 1 and 2 show that there is indeed the expected negative correlation between discount coupons received by the recipient and their peers. Columns 3-5 show, however, that the effect of the number of discount coupons received by the recipient on their insurance purchase is unaffected in magnitude and significance by inclusion of the peers' discount coupons' receipt. Columns 6-8 show that the effect of the number of discount coupons received by peers on peers' insurance purchase is unaffected in sign and significance by the discount coupons received by the recipient. Together, these findings suggest that spillovers from discount coupons received by peers, if they exist, do not meaningfully affect coupon recipients' insurance purchase, and vice versa. This also implies that we can control for spillovers in the second-stage using the

[34] Appendix Table B1 presents the summary statistics of spillover variables.

Figure 4: DAG: potential spillover interaction



Notes: Pathways are indicated by (1)-(13) and A and B. D_{ij} refers to the discount coupons received by herder i in community j, I_{ij} to their insurance purchase, and Y_{ij} their long-run outcome. Denoting other herders from community j, termed "peers," as $-i$, their discount coupons received, insurance purchase, and long-run outcomes are D_{-ij}, I_{-ij}, and Y_{-ij}, respectively. The main causal effect of interest is A, where we estimate the LATE of I_{ij} on Y_{ij}, instrumenting I_{ij} by D_{ij}. The blue arrows present this main specification. The red pathway presents a standard, direct violation of the exclusion restriction. The green pathways present indirect violations of the exclusion restriction and violations of SUTVA, the purple pathways present violations of SUTVA. The black arrows indicate mechanical negative correlations. See Appendix B for more details.

discount coupons received by peers as a valid instrument to identify insurance purchase by peers.

Therefore we test for the robustness of our main results by including the mean number of discount coupons received by peers as additional instrument, and mean insurance purchase by peers as additional endogenous regressor in our main specifications in equations (2) and (3). Tables B3 and B4 present the second-stage results.

The results are qualitatively similar, although we lose statistical power due to the addition of another instrument and endogenous regressors and the lack of inclusion of community fixed effects. The effect on herd composition remains unchanged at a 23 percentage points reduction in the share of goats herded, significant at the five percent level (versus a 21 percentage points decrease when not controlling for potential interpersonal spillovers and including community fixed effects). The estimated effects for maximum, total and average years of education are now 2.5, 6.2, and 2.0 years respectively (versus 2.9, 7.3, and 2.5 when not controlling for potential interpersonal spillovers and including community fixed effects), with p-values 0.102, 0.085, 0.119, respectively. The estimated effect on whether or not children study full-time is 0.408 with a p-value = 0.105.[35] Overall, these checks for robustness to prospective interpersonal spillovers reinforce our central findings.

7 Mechanisms

We now explore candidate mechanisms that may drive the observed long-run outcomes of catastrophic drought insurance. We already excluded the possibility that take-up during the experiment induced IBLI purchase during the 12 months prior to the endline (Table D4). Due to insurance delivery supply constraints in our study villages after the experiment, we can also rule out the possibility that uptake between the experimental period and the endline explains any of the long-term effects we find. The observed effects must begun during the experimental period of (transitory) insurance uptake in the initial few years after IBLI was first introduced.

7.1 Impact dynamics over the study period

The observation that early IBLI adoption generated effects long after households ceased buying the insurance product raises the question of whether the observed effects began during the experimental treatment and persisted, or if they appear only later, after insurance coverage ended. A long-term effect from a transitory treatment may imply that initial IBLI uptake induce a shift in insured households' dynamic equilibrium behaviors. Unpacking that induced change requires exploring how effects emerge over time.

To do so, we estimate Equation (2) on the same outcomes after the third sales season ($\sim$1.5 years after the introduction) – the terminal point for our instrument – as well as at the end of the experiment, after the sixth sales season ($\sim$3 years after IBLI's launch). Figure 5 shows that the significant effect on herd composition materialized towards the end of the experiment, and then persisted and became stronger over time.[36] We see a negative and significant 9.0 percentage points reduction in the share of goats by the end of the experiment. For camels we already observe a marginally insignificant 7.1 percentage points (p-value = 0.179) increase in the share of camels by

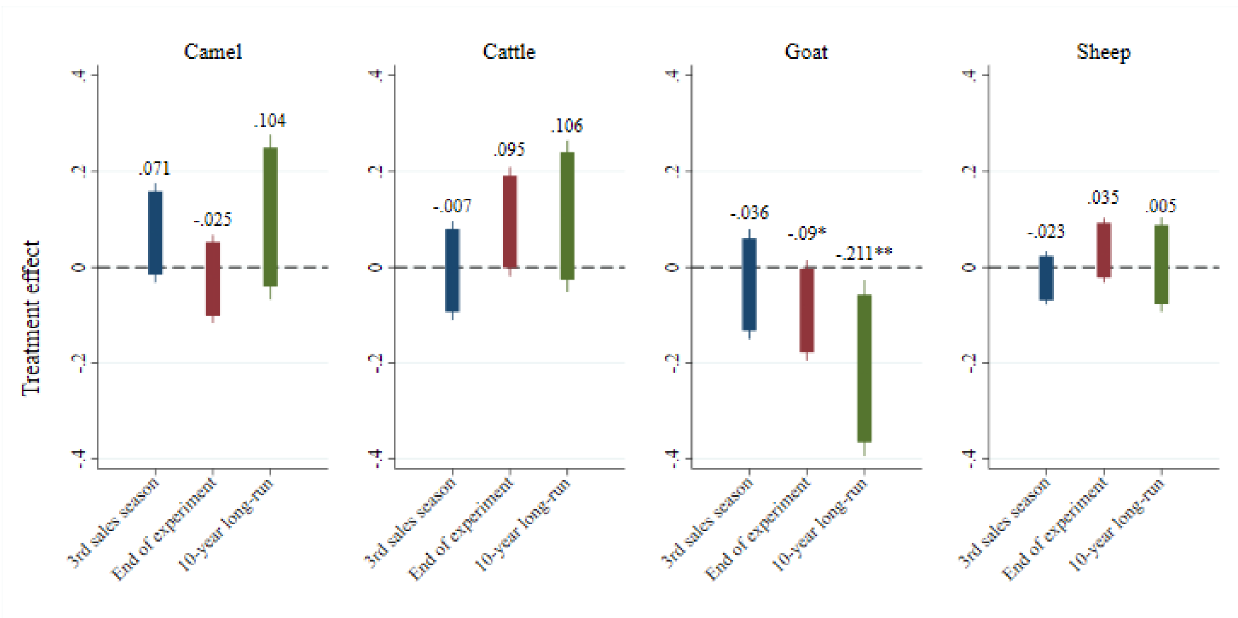
[35]In some cases the estimated coefficients for the effect of $\hat{\hat{I}}_{ij}$ are large and significant. Their significance is what we might expect given the mechanical correlation between discount coupons and therefore insurance purchase of recipients and their peers, and does not necessarily indicate a spillover. While the point estimates appear large, their effects refer to an increase from no purchase of insurance by any of the peers to purchase of insurance by all peers.

[36]See Appendix Table C12 and C13 for regression results.

the end of the third sales season, while for cattle there is a 9.5 percentage points (p-value = 0.105) increase in the share by the end of the experiment, after 3 years, which largely persists, even grows insignificantly, until the 10-year follow-up.

Figure 6 shows that the effects on education only materialized by the time of the 10-year follow-up, consistent with the effects on whether or not children are studying full-time, which also only appear by the time of the 10-year endline.[37] These results provide a strong indication that the effects on herd composition materialized before those on education.

Figure 5: Dynamic effects on herd composition

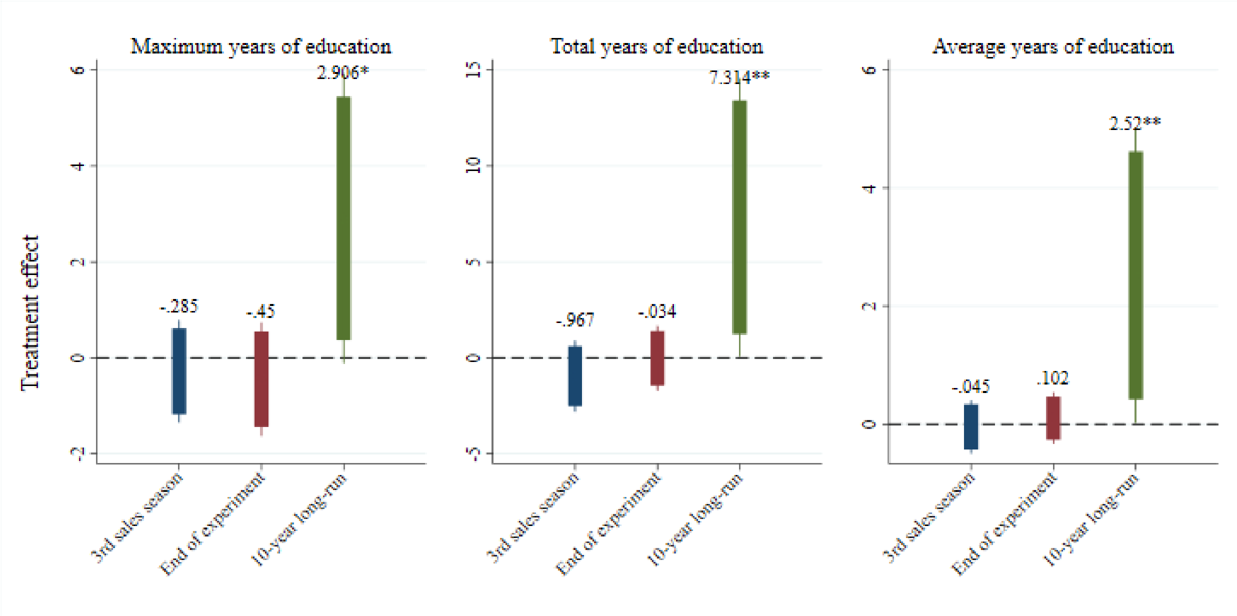


Notes: This figure presents the estimated LATE of any insurance purchase in the first three seasons – instrumented by the number of discount coupons received by recipients in the first three seasons – on "herd composition" measured as the number of animals of each animal type that the household herds expressed in CMVE divided by the total number of animals that the household herds expressed in CMVE i) after the third sales season, ii) after the end of the experiment (sixth sales season), and iii) at the 10-year follow up. The boxes present the 90 percent confidence intervals, and the lines represent the 95 percent confidence intervals. The numbers above the boxes present the estimate of the LATE. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

The effects on herd composition and educational attainment are substantively linked, not just coincidental in time. Larger animals like camels and cattle are more productive per TLU – through milk and calves produced – and thus generate more income, for which we provide suggestive evidence. While this positive income effect may have incentivized educational investment directly, the herd composition change and reduced goats herded also weakened household demand for child

[37]Refer to Table C14 in the Appendix for the results, and to Table C15 and Figure C2 in the Appendix for the child-time use results

Figure 6: Dynamic effects on maximum, total, and average years of education



Notes: This figure presents the estimated LATE of any insurance purchase in the first three seasons – instrumented by the number of discount coupons received by recipients in the first three seasons – on outcomes "Maximum years of education", "Total years of education", and "Average years of education" measured among household members who were school-aged at any point during initial three periods of experiments, i.e., 15-29 years old in Kenya and 15-17 years old in Ethiopia i) after the third sales season, ii) after the end of the experiment (sixth sales season), and iii) at the 10-year follow up. The boxes present the 90 percent confidence intervals, and the lines represents the 95 percent confidence intervals. The numbers above the boxes present the estimate of the LATE. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

labor. Children are far less likely to herd large animals like camels or cattle, than goats or sheep. Camels in particular are large, strong and ornery, managed overwhelmingly by adult men. Our results suggest that the observed changes in herd composition may have reduced the marginal productivity of child herding labor. This would have increased incentives to educate children, similar to Shah and Steinberg (2017) and Bau et al. (2024), and especially boys, who are most typically involved in herding of small animals. Appendix Table C7 indeed shows that the estimated effects on education arise due to the impacts on boys.The sequencing of observed impacts suggests strongly that IBLI's long-term effects arise due to induced shifts in pastoralist productions strategies, from lower-value, more liquid smallstock like goats, to larger, higher-value camels and cattle, with both endogenous income and derived child labor demand effects that strongly shift insured households' investments in children's education. Once these behavioral shifts occur, they stick even though the household no longer purchases IBLI. That is consistent with the observation that transitioning herd composition is risky. If drought strikes after one has liquidated goats and before the larger species has had time to calve, lactate and safely establish their young, one runs a heightened risk of catastrophic loss because calves and pregnant or lactating cows are especially vulnerable to dehydration, disease, and malnutrition (Shrum et al., 2018). IBLI seems to have been effective in facilitating transition to a more desirable herd portfolio.

7.2 Insurance coverage versus indemnity payments?

Consistent with the proposed mechanisms above, we investigate whether the long-run outcomes are driven by *ex ante* behavioral effects induced by reduced catastrophic risk exposure resulting from purchasing insurance, or from *ex post* impacts of indemnity payments triggered by (exogenous) low NDVI readings during droughts. The indemnity payments from insurance provided households a lump sum cash transfer, that could have relieved savings or liquidity constraints, incentivizing purchase of lumpy assets, or investments in education. This would parallel prior studies on the effects of cash transfer interventions (Angelucci, Attanasio, and Di Maro, 2012; Haushofer and Shapiro, 2016; Blattman et al., 2016; Baird, McIntosh, and Özler, 2019).

To investigate these potential channels, we modify our prior regression specification to include the receipt of indemnity payments as additional endogenous regressor, which are conditional on both (instrumented) insurance purchase and exogenous drought shocks. Therefore we estimate the following second-stage equation:

$$y_{ijT} = \gamma_0 + \gamma_1 \widehat{I_{ij}} + \gamma_2 \widehat{I_{ij}} \times R_j + \gamma_3 R_j + \gamma_4 y_{ij0} + \gamma_5 X_{ij0} + \gamma_6 D_{ij4}^{t=6} + \varepsilon_{ijT} \tag{6}$$

where R_j is a binary variable that equals 1 if the average exogenous NDVI index value for the three initial seasons for which we instrument is below the index trigger value (<20% in Kenya and <15% in Ethiopia) – and thus an indemnity payment is triggered in the index unit, conditional on insurance purchase – and 0 otherwise.

Note that during the initial three sales seasons, payouts were observed twice in Kenya, and not at all in Ethiopia. The coefficient γ_1 captures the effect of (predicted) insurance uptake on the outcome in the absence of a payout, which we can think of as the "peace-of-mind" (*ex ante*) effect of insurance (Tafere, Barrett, and Lentz, 2019). The combined effects of purchasing insurance and receiving the indemnity payment are captured by $\gamma_1 + \gamma_2$, which is the marginal effect of interest in the event an indemnity payout occurs. We do not incorporate community fixed effects because we are interested in comparing the estimated effect of insurance (without indemnity payments) to the estimated effect of insurance with indemnity payments, controlling for the severity of droughts. The fact that the index is triggered at the index unit level, implies that there is no community-level variation in insurance only and insurance with indemnity payments, and we need to leverage cross-sectional variation to estimate this.

Tables C16 to C17 in the Appendix show the results of estimating Equation (6) for the education and herd composition outcomes. The marginal effects of receiving insurance and an indemnity payment ($\gamma_1 + \gamma_2$) appear in the first row of the bottom panel of the tables, its p-value in the second row. We can see from these tables that in all cases the effects are driven by whether or not the respondent purchased insurance. None of the interaction effects of insurance purchase and the indemnity dummy and none of the combined marginal effects of insurance and the indemnity dummy are significant.

These results suggest that a cash liquidity injection from indemnity payments does not explain our long-run results. This is consistent with broader findings in the literature that cash transfers' short-run effects often do not persist to generate long-term effects (Araujo, Bosch, and Schady, 2017; Baird, McIntosh, and Özler, 2019; Blattman, Dercon, and Franklin, 2022; Blattman, Fiala, and Martinez, 2020). Rather, we demonstrate that reduced *ex ante* risk exposure and the behavioral changes it induces, not the cash transfers resulting from the indemnity payment, generate the long-run effects we observe.

This is consistent with prior findings of subjective well-being gains from insurance coverage even in the absence of payouts (Tafere, Barrett, and Lentz, 2019), as well as *ex ante* effects of insurance on increases in productive investments, irrespective of indemnity payments (Karlan et al., 2014; Cole and Xiong, 2017; Jensen, Barrett, and Mude, 2017; Hill et al., 2019; Matsuda, Takahashi, and Ikegami, 2019; Boucher et al., 2021; Stoeffler et al., 2022).

The changes in herd composition, and the shift from small liquid animals to large, lumpy productive animals can thus be related to *ex ante* reduction in risk exposure, and the behavioural effect this induces. This may have occurred because of a reduced felt need for precautionary savings in-kind, in the form of highly liquid goats, to cover potential drought-related expenditures on food (to replace lost milk production), fodder, water, and veterinary expenses. Expected insurance indemnity payments provide an alternative to in-kind savings (held in goats) to cover such drought-related expenditures.[38] The reduction in risk exposure may have also induced households to re-balance their livestock portfolio towards higher risk but higher return animals. Camels and cattle are more productive due to higher milk production and calving. They, however, also imply

[38] The phrase "sold a goat to insure a cow" is often heard among our population. Indeed, some portion of the herd composition shift may be the result of households selling goats to purchase insurance coverage. However, the estimated treatment effect on the share of goats exceeds by an order of magnitude the average insurance premia that households paid. So liquidating goats to pay insurance premia can only explain a small share of the observed herd composition shift.

an order of magnitude larger loss in case animals are lost due to disease, drought or other risks.

8 Conclusions

A growing literature has established that uninsured exposure to catastrophic aggregate shocks can have adverse effects on long-run human capital accumulation. It follows, therefore, that insurance against such shocks might boost human capital accumulation. Direct evidence on this important question had been lacking to date, however.

We test that hypothesis by exploiting the randomized encouragement design of the original impact evaluation of index-based livestock insurance (IBLI), a catastrophic drought insurance product introduced among pastoralist populations in northern Kenya and southern Ethiopia in 2010-12, and followed up with the original survey households ten years later. We find that insurance coverage sharply changed households' production strategies and increased children's educational attainment. Insured households decreased the herd share they kept in goats by 72% and sharply increased children's education as well as their likelihood of studying full-time. These effects are driven entirely by the insurance coverage itself rather than by receipt of cash indemnity payments triggered by drought events. The reduced *ex ante* drought risk exposure through IBLI coverage and the behavioral changes insurance caused generate the observed long-term effects, not the improved financial liquidity arising from lump-sum cash transfers due to indemnity payments.

Our research illuminates the important role that formal risk mitigation instruments can play for human capital accumulation. Our results are especially and immediately relevant for the major, four-country initiative now underway to scale the IBLI-based drought insurance program to reach 1.6 million pastoralists across the Horn of Africa. While IBLI can help facilitate households' transition to more productive livestock portfolios and thereby protect human capital from drought shocks and promote children's education, complementary interventions will are necessary to help relieve the continuing, severe poverty that afflicts many pastoralist households in the region.

References

Abadie, Alberto, Susan Athey, Guido W Imbens, and Jeffrey M Wooldridge (2022). “When Should You Adjust Standard Errors for Clustering?” *Quarterly Journal of Economics* 138.1, pp. 1–35.

Admassu, B., S. Nega, T. Haile, B. Abera, A. Hussein, and A. Catley (2005). “Impact Assessment of a Community-based Animal Health Project in Dollo Ado and Dollo Bay Districts, Southern Ethiopia”. *Tropical Animal Health and Production* 37.1, pp. 33–48.

Alderman, Harold, John Hoddinott, and Bill Kinsey (2006). “Long term consequences of early childhood malnutrition”. *Oxford Economic Papers* 58.3, pp. 450–474.

Anderberg, Dan and Karlijn Morsink (2020). “The introduction of formal insurance and its effect on redistribution”. *Journal of Economic Behavior & Organization* 179, pp. 22–45.

Anderson, Michael L. (2008). “Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects”. *Journal of the American Statistical Association* 103.484, pp. 1481–1495.

Angelucci, Manuela, Orazio Attanasio, and Vincenzo Di Maro (2012). “The Impact of "Oportunidades" on Consumption, Savings and Transfers”. *Fiscal Studies* 33.3, pp. 305–334.

Araujo, M. Caridad, Mariano Bosch, and Norbert Schady (2017). “Can Cash Transfers Help Households Escape an Intergenerational Poverty Trap?” In: *The Economics of Poverty Traps*. University of Chicago Press, pp. 357–382.

Baird, Sarah, Joan Hamory Hicks, Michael Kremer, and Edward Miguel (2016). “Worms at Work: Long-run Impacts of a Child Health Investment”. *Quarterly Journal of Economics* 131.4, pp. 1637–1680.

Baird, Sarah, Craig McIntosh, and Berk Özler (2019). “When the money runs out: Do cash transfers have sustained effects on human capital accumulation?” *Journal of Development Economics* 140, pp. 169–185.

Balboni, Clare, Oriana Bandiera, Robin Burgess, Maitreesh Ghatak, and Anton Heil (2022). “Why Do People Stay Poor?*”. *Quarterly Journal of Economics* 137.2, pp. 785–844.

Bandiera, Oriana, Robin Burgess, Narayan Das, Selim Gulesci, Imran Rasul, and Munshi Sulaiman (2017). “Labor Markets and Poverty in Village Economies”. *Quarterly Journal of Economics* 132.2, pp. 811–870.

Banerjee, Abhijit, Emily Breza, Esther Duflo, and Cynthia Kinnan (2019). “Can microfinance unlock a poverty trap for some entrepreneurs?” *National Bureau of Economic Research.* Working Paper No. 26346.

Banerjee, Abhijit Vinayak, Esther Duflo, and Michael Kremer (2016). “The influence of randomized controlled trials on development economics research and on development policy”. In: *The state of Economics, the state of the world*, pp. 482–488.

Barham, Tania, Karen Macours, and John A. Maluccio (2017). “Are Conditional Cash Transfers Fulfilling Their Promise? Schooling, Learning, and Earnings after 10 Years”. *CEPR Discussion Paper No. DP11937*.

Barrett, Christopher B, Teevrat Garg, and Linden McBride (2016). “Well-being dynamics and poverty traps”. *Annual Review of Resource Economics* 8.1, pp. 303–327.

Barrett, Christopher B., Michael R. Carter, and Jean-Paul Chavas, eds. (2019). *The Economics of Poverty Traps*. University of Chicago Press.

Barrett, Christopher B., Francis Chabari, DeeVon Bailey, Peter D. Little, and D. Layne Coppock (2003). “Livestock Pricing in the Northern Kenyan Rangelands”. *Journal of African Economies* 12.2, pp. 127–155.

Barrett, Christopher B., Paswel Phiri Marenya, John McPeak, Bart Minten, Festus Murithi, Willis Oluoch-Kosura, Frank Place, Jean Claude Randrianarisoa, Jhon Rasambainarivo, and Justine Wangila (2006). “Welfare dynamics in rural Kenya and Madagascar”. *Journal of Development Studies* 42.2, pp. 248–277.

Barrett, Christopher B. and Brent M. Swallow (2006). “Fractal poverty traps”. *World Development* 34.1, pp. 1–15.

Bau, Natalie, Martin Rotemberg, Manisha Shah, and Bryce Steinberg (2024). “Human capital investment in the presence of child labor”. *National Bureau of Economic Research. Working Paper No. 27241*.

Belissa, Temesgen Keno, Robert Lensink, and Marcel van Asseldonk (2020). “Risk and ambiguity aversion behavior in index-based insurance uptake decisions: Experimental evidence from Ethiopia”. *Journal of Economic Behavior & Organization* 180, pp. 718–730.

Berg, Erlend, Michael Blake, and Karlijn Morsink (2022). “Risk Sharing and the Demand for Insurance: Theory and Experimental Evidence from Ethiopia”. *Journal of Economic Behavior and Organization* 195, pp. 236–256.

Bettinger, Eric, Sten Ludvigsen, Mari Rege, Ingeborg F. Solli, and David Yeager (2018). "Increasing perseverance in math: Evidence from a field experiment in Norway". *Journal of Economic Behavior & Organization* 146, pp. 1–15.

Binswanger-Mkhize, Hans P. (2012). "Is There Too Much Hype about Index-based Agricultural Insurance?" *Journal of Development Studies* 48.2, pp. 187–200.

Blattman, Christopher, Stefan Dercon, and Simon Franklin (2022). "Impacts of industrial and entrepreneurial jobs on youth: 5-year experimental evidence on factory job offers and cash grants in Ethiopia". *Journal of Development Economics* 156, p. 102807.

Blattman, Christopher, Nathan Fiala, and Sebastian Martinez (2020). "The Long-Term Impacts of Grants on Poverty: Nine-Year Evidence from Uganda's Youth Opportunities Program". *American Economic Review: Insights* 2.3, pp. 287–304.

Blattman, Christopher, Eric P. Green, Julian Jamison, M. Christian Lehmann, and Jeannie Annan (2016). "The Returns to Microenterprise Support among the Ultrapoor: A Field Experiment in Postwar Uganda". *American Economic Journal: Applied Economics* 8.2, pp. 35–64.

Boucher, Stephen R., Michael R. Carter, Jon Einar Flatnes, Travis J. Lybbert, Jonathan G. Malacarne, Paswel Marenya, and Laura A. Paul (2021). "Bundling Stress Tolerant Seeds and Insurance for More Resilient and Productive Small-scale Agriculture". *NBER: National Bureau of Economic Research. Working Paper No. 29234*.

Boucher, Stephen R., Michael R. Carter, and Catherine Guirkinger (2008). "Risk Rationing and Wealth Effects in Credit Markets: Theory and Implications for Agricultural Development". *American Journal of Agricultural Economics* 90.2, pp. 409–423.

Bouguen, Adrien, Yue Huang, Michael Kremer, and Edward Miguel (2019). "Using Randomized Controlled Trials to Estimate Long-Run Impacts in Development Economics". *Annual Review of Economics* 11.1, pp. 523–561.

Caeyers, Bet and Marcel Fafchamps (2020). "Exclusion bias and the estimation of peer effects". *National Bureau of Economic Research. Working Paper No. 22565* 22565.

Carrillo, Bladimir (2020). "Early Rainfall Shocks and Later-Life Outcomes: Evidence from Colombia". *World Bank Economic Review* 34.1, pp. 179–209.

Carter, Michael, Alain de Janvry, Elisabeth Sadoulet, and Alexandros Sarris (2017). "Index Insurance for Developing Country Agriculture: A Reassessment". *Annual Review of Resource Economics* 9.1, pp. 421–438.

Chantarat, Sommarat, Andrew G. Mude, Christopher B. Barrett, and Michael R. Carter (2013). "Designing Index-Based Livestock Insurance for Managing Asset Risk in Northern Kenya". *Journal of Risk and Insurance* 80.1, pp. 205–237.

Charpak, Nathalie, Rejean Tessier, Juan G Ruiz, Jose Tiberio Hernandez, Felipe Uriza, Julieta Villegas, Line Nadeau, Catherine Mercier, Francoise Maheu, Jorge Marin, Darwin Cortes, Juan Miguel Gallego, and Dario Maldonado (2017). "Twenty-year Follow-up of Kangaroo Mother Care Versus Traditional Care." *Pediatrics* 139.1, e20162063.

Cissé, Jennifer Denno and Christopher B. Barrett (2018). "Estimating development resilience: A conditional moments-based approach". *Journal of Development Economics* 135, pp. 272–284.

Cole, Shawn A. and Wentao Xiong (2017). "Agricultural Insurance and Economic Development". *Annual Review of Economics* 9.1, pp. 235–262.

De Chaisemartin, Clément and Jaime Ramirez-Cuellar (2024). "At what level should one cluster standard errors in paired and small-strata experiments?" *American Economic Journal: Applied Economics* 16.1, pp. 193–212.

Dinkelman, Taryn (2017). "Long-Run Health Repercussions of Drought Shocks: Evidence from South African Homelands". *Economic Journal* 127.604, pp. 1906–1939.

Emerick, Kyle, Alain de Janvry, Elisabeth Sadoulet, and Manzoor H. Dar (2016). "Technological Innovations, Downside Risk, and the Modernization of Agriculture". *American Economic Review* 106.6, pp. 1537–1561.

Fruehwirth, Jane Cooley, Sriya Iyer, and Anwen Zhang (2019). "Religion and depression in adolescence". *Journal of Political Economy* 127.3, pp. 1178–1209.

Gray-Lobe, Guthrie, Parag A Pathak, and Christopher R Walters (2023). "The Long-Term Effects of Universal Preschool in Boston". *Quarterly Journal of Economics* 138.1, pp. 363–411.

Guryan, Jonathan, Kory Kroft, and Matthew J Notowidigdo (2009). "Peer effects in the workplace: Evidence from random groupings in professional golf tournaments". *American Economic Journal: Applied Economics* 1.4, pp. 34–68.

Haushofer, Johannes and Jeremy Shapiro (2016). "The Short-Term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya". *Quarterly Journal of Economics* 131.4, pp. 1973–2042.

Hill, Ruth Vargas, Neha Kumar, Nicholas Magnan, Simrin Makhija, Francesca de Nicola, David J. Spielman, and Patrick S. Ward (2019). "Ex ante and ex post effects of hybrid index insurance in Bangladesh". *Journal of Development Economics* 136, pp. 1–17.

Hoddinott, John, John A. Maluccio, Jere R. Behrman, Rafael Flores, and Reynaldo Martorell (2008). "Effect of a nutrition intervention during early childhood on economic productivity in Guatemalan adults". *Lancet* 371.9610, pp. 411–416.

Homewood, Katherine, Pippa Trench, Sara Randall, Godelieve Lynen, and Beth Bishop (2006). "Livestock health and socio-economic impacts of a veterinary intervention in Maasailand: Infection-and-treatment vaccine against East Coast fever". *Agricultural Systems* 89.2, pp. 248–271.

Huysentruyt, Marieke, Christopher B. Barrett, and John G. McPeak (2009). "Understanding Declining Mobility and Inter-household Transfers among East African Pastoralists". *Economica* 76.302, pp. 315–336.

Janzen, Sarah A. and Michael R. Carter (2019). "After the Drought: The Impact of Microinsurance on Consumption Smoothing and Asset Protection". *American Journal of Agricultural Economics* 101.3, pp. 651–671.

Jensen, Nathaniel and Christopher Barrett (2017). "Agricultural index insurance for development". *Applied Economic Perspectives and Policy* 39.2, pp. 199–219.

Jensen, Nathaniel, Nils Teufel, Rupsha Banerjee, Diba Galgallo, and Kelvin Mashisia Shikuku (2024a). "The role of heterogenous implementation on the uptake and long-term diffusion of agricultural insurance in a pastoral context". *Food Policy* 125, p. 102644.

Jensen, Nathaniel D., Christopher B. Barrett, and Andrew G. Mude (2016). "Index Insurance Quality and Basis Risk: Evidence from Northern Kenya". *American Journal of Agricultural Economics* 98.5, pp. 1450–1469.

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

Jensen, Nathaniel D., Francesco P. Fava, Andrew G. Mude, Christopher B. Barrett, Bernda Wandera-Gache, Anton Vrieling, Masresha Taye, Kazushi Takahashi, Felix Lung, Munenobu Ikegami, Polly Ericksen, Philemon Chelang'a, Sommarat Chantarat, Michael R. Carter, Hassan Bashir, and Rupsha Banerjee (2024b). *Escaping Poverty Traps And Unlocking Prosperity In The Face Of Climate Risk: Lessons From IBLI*. Cambridge University Press.

Jensen, Robert (2000). "Agricultural Volatility and Investments in Children". *American Economic Review* 90.2, pp. 399–404.

Karlan, Dean, Robert Osei, Isaac Osei-Akoto, and Christopher Udry (2014). "Agricultural Decisions after Relaxing Credit and Risk Constraints". *Quarterly Journal of Economics* 129.2, pp. 597–652.

Kraay, Aart and David McKenzie (2014). "Do poverty traps exist? Assessing the evidence". *Journal of Economic Perspectives* 28.3, pp. 127–148.

Little, Peter D, John McPeak, Christopher B Barrett, and Patti Kristjanson (2008). "Challenging orthodoxies: understanding poverty in pastoral areas of East Africa". *Development and Change* 39.4, pp. 587–611.

Lybbert, Travis J., Christopher B. Barrett, Solomon Desta, and D. Layne Coppock (2004). "Stochastic Wealth Dynamics and Risk Management among a Poor Population". *Economic Journal* 114.498, pp. 750–777.

Maccini, Sharon and Dean Yang (2009). "Under the Weather: Health, Schooling, and Economic Consequences of Early-Life Rainfall". *American Economic Review* 99.3, pp. 1006–1026.

Matsuda, Ayako, Kazushi Takahashi, and Munenobu Ikegami (2019). "Direct and indirect impact of index-based livestock insurance in Southern Ethiopia". *Geneva Papers on Risk and Insurance - Issues and Practice* 44.3, pp. 481–502.

McPeak, John (2005). "Individual and Collective Rationality in Pastoral Production: Evidence From Northern Kenya". *Human Ecology* 33.2, pp. 171–197.

McPeak, John G. and Christopher B. Barrett (2001). "Differential Risk Exposure and Stochastic Poverty Traps Among East African Pastoralists". *American Journal of Agricultural Economics* 83.3, pp. 674–679.

McPeak, John G., Peter D. Little, and Cheryl R. Doss (2011). *Risk and Social Change in an African Rural Economy: Livelihoods in Pastoralist Communities*. (London: Routledge Press.) 225 pp.

Meroni, Michele, Michel M. Verstraete, Felix Rembold, Ferdinando Urbano, and François Kayitakire (2014). "A phenology-based method to derive biomass production anomalies for food security monitoring in the Horn of Africa". *International Journal of Remote Sensing* 35.7, pp. 2472–2492.

Mishra, Khushbu, Richard A. Gallenstein, Mario J. Miranda, Abdoul G. Sam, Patricia Toledo, and Francis Mulangu (2021). "Insured Loans and Credit Access: Evidence from a Randomized Field Experiment in Northern Ghana". *American Journal of Agricultural Economics* 103.3, pp. 923–943.

Mobarak, Ahmed Mushfiq and Mark R. Rosenzweig (2013). “Informal Risk Sharing, Index Insurance, and Risk Taking in Developing Countries”. *American Economic Review* 103.3, pp. 375–380.

Molina Millán, Teresa and Karen Macours (2017). “Attrition in randomized control trials: Using tracking information to correct bias”. *Economic Development and Cultural Change.*forthcoming.

Noritomo, Yuma and Kazushi Takahashi (2020). “Can Insurance Payouts Prevent a Poverty Trap? Evidence from Randomised Experiments in Northern Kenya”. *Journal of Development Studies* 56.11, pp. 2079–2096.

Olea, José Luis Montiel and Carolin Pflueger (2013). “A Robust Test for Weak Instruments”. *Journal of Business & Economic Statistics* 31.3, pp. 358–369.

Prince, Stephen. D. (1991). “Satellite remote sensing of primary production: comparison of results for Sahelian grasslands 1981-1988”. *International Journal of Remote Sensing* 12.6, pp. 1301–1311.

Rahman, Khandker Wahedur (2023). “International migration and the religious schooling of children in the home country: evidence from Bangladesh”. *Journal of Population Economics* 36.3, pp. 1963–2005.

Riley, Emma (2018). “Mobile money and risk sharing against village shocks”. *Journal of Development Economics* 135, pp. 43–58.

Santos, Paulo and Christopher B. Barrett (2011). “Persistent poverty and informal credit”. *Journal of Development Economics* 96.2, pp. 337–347.

– (2019). “Heterogeneous Wealth Dynamics: On the Roles of Risk and Ability”. In: *The Economics of Poverty Traps*. Ed. by Christopher B. Barrett, Michael R. Carter, and Jean-Paul Chavas. University of Chicago Press, pp. 265–290.

Shah, Manisha and Bryce Millett Steinberg (2017). “Drought of Opportunities: Contemporaneous and Long-Term Impacts of Rainfall Shocks on Human Capital”. *Journal of Political Economy* 125.2, pp. 527–561.

Shrum, Trisha R, William R Travis, Travis M Williams, and Evan Lih (2018). “Managing climate risks on the ranch with limited drought information”. *Climate Risk Management* 20, pp. 11–26.

Sieff, Daniel F. (1999). “The effects of wealth on livestock dynamics among the Datoga pastoralists of Tanzania”. *Agricultural Systems* 59.1, pp. 1–25.

Son, Hyuk Harry (2023). “The Effect of Microinsurance on Child Work and Schooling”. Working Paper.

Stoeffler, Quentin, Michael Carter, Catherine Guirkinger, and Wouter Gelade (2022). “The Spillover Impact of Index Insurance on Agricultural Investment by Cotton Farmers in Burkina Faso”. *World Bank Economic Review* 36.1, pp. 114–140.

Tafere, Kibrom, Christopher B. Barrett, and Erin Lentz (2019). “Insuring Well-Being? Buyer’s Remorse and Peace of Mind Effects From Insurance”. *American Journal of Agricultural Economics* 101.3, pp. 627–650.

Takahashi, Kazushi, Christopher B. Barrett, and Munenobu Ikegami (2019). “Does Index Insurance Crowd In or Crowd Out Informal Risk Sharing? Evidence from Rural Ethiopia”. *American Journal of Agricultural Economics* 101.3, pp. 672–691.

Takahashi, Kazushi, Munenobu Ikegami, Megan Sheahan, and Christopher B. Barrett (2016). “Experimental Evidence on the Drivers of Index-Based Livestock Insurance Demand in Southern Ethiopia”. *World Development* 78, pp. 324–340.

The World Bank (2022). “Project Appraisal Document for a De-Risking, Inclusiong and Value Enhancement of pastoral economies in the horn of africa project”. PAD4750.

Tucker, C. J., C. L. Vanpraet, M. J. Sharman, and G. Van Ittersum (1985). “Satellite remote sensing of total herbaceous biomass production in the senegalese sahel: 1980–1984”. *Remote Sensing of Environment* 17.3, pp. 233–249.

Vrieling, Anton, Michele Meroni, Andrew G. Mude, Sommarat Chantarat, Caroline C. Ummenhofer, and Kees (C A. J. M.) de Bie (2016). “Early assessment of seasonal forage availability for mitigating the impact of drought on East African pastoralists”. *Remote Sensing of Environment* 174, pp. 44–55.

Appendix

A Balance and Attrition

A.1 Balance

This subsection presents the tests of balance of the randomized discount coupon offers for each season. We estimate the following equation for our pre-specified set of balance variables that were selected following Jensen, Barrett, and Mude (2017) and Takahashi et al. (2016)[39]:

$$k_{ijt} = \gamma_1 + \gamma_2 \mathrm{D}_{ijt} + \rho_j + \nu_{ijt} \tag{7}$$

where k_{ijt} denotes a characteristic of a household i in location j in sales season t, D_{ijt} is an indicator for whether or not the household i in location j received a discount coupon in sales season t, ρ_j is the location fixed effects, and ν_{ijt} is the error term, clustered at the household level.

In addition to the coefficient estimates and standard errors, we use the normalized difference as a scale-invariant measure of the size of the difference in means between households with and without discount coupons, which we calculate by:

$$\text{Normalized Difference} = \frac{\bar{X}_{treatment} - \bar{X}_{control}}{\sqrt{(s^2_{treatment} + s^2_{control})/2}} \tag{8}$$

where $\bar{X}$ represents the mean and s the standard deviation of a variable.

As stated in the main body of the text, results reported in Table A1 show that randomization was balanced across observables in each season.

A.2 Attrition

This subsection presents the ananlysis of attrition. At baseline, 1439 households participated in our panel survey. Ten years later we were able to track 1179, or 82% of these households (Table A2).

[39]Variables include: age of the household head, an indicator for whether the household is male headed, years of education of the household head, adult equivalent, dependency ratio, herd size in CMVE, annual income per capita in USD, and whether the household owned or farmed on agricultural land in the last 12 months.

We first verify if we have differential attrition across discount coupono assignment. Because our is the number of seasons that a household received a coupon during the first three sales seasons, we test for differential attrition by estimating Eq. (9):

$$\text{Attrition}_{ijT} = \delta_0 + \delta_1 \text{D}_{ij} + \gamma_j + \omega_{ij} \tag{9}$$

where $Attrition_{ijT}$ is an indicator that equals 1 if a household i in location j was interviewed at baseline (2009 in Kenya, 2012 in Ethiopia), but not during the long-run follow-up survey round (2020 in Kenya and 2022 in Ethiopia). D_{ij} is the number of sales seasons out of the initial three where a household received a discount coupon. γ_j represents location fixed effects, and ω_{ij} the error term, clustered at the household-level. Column (1) of Table A3 reports the results, and we do not find significant differential attrition by our instrument. As pre-specified in our pre-analysis plan we also estimate differential attrition based on the number of coupons received in all six sales seasons, and Column (2) of Table A3 shows that there is no differential attrition.

Discount rates may separately affect the probability of a household to attrit differentially, conditional on receiving a discount coupon. Therefore, we estimate the following equation to evaluate attrition by discount coupon receipt and discount rate for each sales season separately:

$$\text{Attrition}_{ijT} = \kappa_0 + \kappa_1 \text{D}_{ijt} + \kappa_2 \text{Discount Rate}_{ijt} + \kappa_3 \text{Absent}_{ijt} + \rho_j + \omega_{ijt} \tag{10}$$

where D_{ijt} is an indicator equal to one if a household i in location j in sales season t received a discount coupon. $\text{Discount Rate}_{ijt}$ is the coupon discount rate in percentages, defined as zero if the household did not receive any discount. Since some households drop out from the panel survey in a specific round, to return a round later, we include $Absent_{ijt}$, an indicator denoting that the household was absent from the panel survey in specific sales season t. ρ_j represents location fixed effects, and ω_{ijt} is the robust standard error. The estimated results reported in Table A6 show that there is no differential attrition by discount coupon receipt, except for sales season 3, where those who received a discount coupon are significantly less likely to attrit than those who did not receive a discount coupon, statistically significant at the 95 percent level. This effect is driven by individuals who are temporarily absent in round 3, but reappear in the data in later rounds. There is no differential attrition by the randomly assigned discount rate.

Finally, we consider selective attrition by our pre-specified observable household characteristics. We regress each household characteristic on the attrition indicator:

$$X_{ij0} = \zeta_0 + \zeta_1 Attrition_{ijT} + \rho_j^1 + \sigma_{ijt}^1 \tag{11}$$

where X_{ij0} is the vector of characteristics of household i in community j at baseline. In addition to each coefficient, we also conduct joint significance tests to verify if all characteristics combined are jointly statistically significantly different. As reported in the main text, Table A4 shows that households that are female-headed, that have fewer adults, and that do not own agricultural land were more likely to attrit from the sample.

As per the pre-analysis plan, we also test selective attrition by regressing the attrition indicator on the vector of baseline household characteristics. We estimate the following equation:

$$Attrition_{ijT} = \theta_0 + \theta_1 X_{ij0} + \rho_j^2 + \sigma_{ijt}^2 \tag{12}$$

where all variables are defined following Equation 11. The results reported in Table A5 show that an additional adult household member increases the likelihood of attritioon by 1 percentage points, significant at the 10 percent level. None of the other pre-specified observables significantly predict attrition.[40]

[40] In this table, we replace the missing values with a mean of existing observations and include a dummy variable indicating missing in the regression, to utilize information from all households. We use winsorized value for income per adult equivalent, earnings from livestock sale, and livestock expenditure.

Table A1: Balance by discount coupon receipt per season

	Received coupon vs. No coupon						
Sales Season Kenya: Sales Season Ethiopia:	2010 JF 2012 AS	2011 JF 2013 JF	2011 AS 2013 AS	2012 AS 2014 JF	2013 JF 2014 AS	2013 AS 2015 JF	F-test
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age of the household head	0.493	1.37	-0.243	0.0224	1.28	0.0177	3.94
	(1.05)	(1.04)	(1.01)	(0.959)	(0.944)	(1.09)	{0.685}
	[0.0515]	[0.0862]	[0.0173]	[0.0309]	[0.101]	[0.00159]	
Male headed household (=1)	-0.0206	-0.0265	-0.0340	-0.0373	0.00494	-0.0253	7.14
	(0.0248)	(0.0244)	(0.0243)	(0.0245)	(0.0251)	(0.0284)	{0.308}
	[0.0345]	[0.0235]	[0.00977]	[-0.00182]	[0.0790]	[-0.0608]	
Education of household head	-0.238	-0.0563	-0.0407	0.0914	-0.224	0.183	5.99
	(0.171)	(0.170)	(0.163)	(0.155)	(0.158)	(0.157)	{0.424}
	[-0.121]	[-0.0606]	[-0.0805]	[-0.0370]	[-0.153]	[0.0777]	
Adult equivalent	-0.00907	0.0569	-0.108	-0.0176	-0.137	-0.142	3.43
	(0.120)	(0.118)	(0.119)	(0.116)	(0.119)	(0.147)	{0.753}
	[0.0308]	[0.0414]	[-0.00252]	[0.0267]	[-0.0253]	[-0.0707]	
Dependency ratio	-0.00238	-0.00368	0.00527	0.0125	0.0148	-0.0123	4.59
	(0.0118)	(0.0114)	(0.0113)	(0.0110)	(0.0109)	(0.0123)	{0.597}
	[0.0446]	[0.0462]	[0.0940]	[0.129]	[0.138]	[-0.0634]	
Herd size (CMVE)	1.14	-0.917	-0.252	-1.36	0.453	-2.06	3.17
	(1.63)	(1.61)	(1.69)	(1.44)	(1.15)	(1.87)	{0.787}
	[-0.0200]	[-0.0637]	[-0.0410]	[-0.0261]	[0.0794]	[-0.0876]	
Annual income per AE (USD)	-4.77	-15.8	-3.28	11.1	-2.64	-20.0	4.03
	(10.2)	(15.5)	(13.7)	(10.6)	(12.8)	(16.4)	{0.673}
	[-0.0438]	[-0.113]	[-0.0875]	[0.0173]	[-0.0829]	[-0.0816]	
Own or farm agricultural land	-0.0293*	-0.00378	0.0151	0.0221	-0.0169	-0.00445	6.95
	(0.0174)	(0.0170)	(0.0157)	(0.0166)	(0.0159)	(0.0190)	{0.326}
	[0.152]	[0.204]	[0.290]	[0.259]	[0.180]	[-0.00469]	
F statistics of Joint F-test:	5.988	4.702	4.279	8.845	8.241	8.770	
P-value of Joint F-test:	0.649	0.789	0.831	0.356	0.410	0.362	

Notes: The table presents the effects of whether or not a household received a discount coupon prior to each sales season on our pre-specified balance variables. Each outcome is a characteristic of a household i in area j in sales season t. Columns (1) to (6) report mean differences, robust standard errors (in parentheses), and normalized differences (in square brackets) between the coupon recipients and non-recipients. All estimations include community fixed effects. Column (7) reports joint significance test for each variable across seasons where the first row presents the Chi-statistic and the second row presents the p-value of the test statistic in curly brackets. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Table A2: Number of households present in each round

	Kenya			Ethiopia		
	Total	Original sample	*Net* replacement	Total	Original sample	*Net* replacement
	(1)	(2)	(3)	(4)	(5)	(6)
R1	924	924	.	515	515	.
R2	924	887	37	506	474	32
R3	924	857	30	514	479	3
R4	924	838	19	513	470	8
R5	923	829	8	438	398	
R6	919	785				
R7	868	781				
Balanced sample		712 (77 %)			387 (75 %)	
Initial & Last		781 (85 %)			398 (77 %)	

Notes: The table shows the number of households interviewed in each round. Columns (1) and (4) show the number of households surveyed each panel survey round. Columns (2) and (5) show the number of sampled households in each round that are common with original samples in round 1, which constructs the balanced panel. Columns (3) and (6) show the number of households that were replaced. Rows "Balanced sample" and "Initial & Last" show the number of households surveyed in all periods, and that of $R1$ and $R7$, respectively.

Table A3: Differential attrition by the number of coupons received

	Outcome: Interviewed at baseline but not in the final round (=1)	
	(1)	(2)
N of coupons received – the initial three seasons	-.00764	
	(.00998)	
N of coupons received – all six seasons		-.00285
		(.00734)
N	1439	1439

Notes: The table presents the effect of the number of discount coupons received on attrition, where the outcome $Attrition_{ijT}$ is an indicator that equals 1 if a household i in location j was interviewed at baseline (2009 in Kenya, 2012 in Ethiopia), but not during the long-run follow-up survey round (2020 in Kenya and 2022 in Ethiopia). Estimated coefficients and robust standard errors (in parentheses) are reported in each column. All estimations include community fixed effects. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Table A4: Attrition by household baseline characteristics

	Independent variable: Interviewed at baseline but not in the final round (=1)
	(1)
Age of the household head	-2.04
	(1.33)
Male headed household (=1)	-.0555*
	(.0335)
Education of household head	.355
	(.229)
Adult equivalent	-.383***
	(.143)
Dependency ratio	-.00781
	(.0151)
Herd size (CMVE)	1.3
	(1.95)
Annual income per AE (USD)	20.8
	(15.9)
Own or farm agricultural land	-.0478*
	(.0254)
P-value value of joint F-test	0.016
N	1439

Notes: The table presents effects of each household characteristic on attrition among our sample, using different household characteristics as outcomes in each row. The variable $Attrition_{ijT}$ is an indicator that equals 1 if a household i in location j was interviewed at baseline (2009 in Kenya, 2012 in Ethiopia), but not during the long-run follow-up survey round (2020 in Kenya and 2022 in Ethiopia). Mean differences and robust standard errors (in parentheses) between the attrited and non-attrited households are reported. Attrition is defined as a household i in area j was interviewed at baseline, but not in the latest round. All estimations include community fixed effects. * denotes significance at 0.10; ** at 0.05; and *** at 0.01. The p-value of the joint significance test for all variables across attrition is reported at second from the bottom row.

Table A5: Joint test of selective attrition

	Outcome: Interviewed at baseline but not in the final round (=1)
	(1)
Age of the household head	-.000372
	(.000596)
Male headed household (=1)	-.0357
	(.0255)
Education of household head	.00429
	(.00441)
Adult equivalent	-.0122**
	(.00526)
Dependency ratio	-.0196
	(.0512)
Herd size (CMVE)	.000421
	(.000354)
Annual income per AE (USD)	.0000429
	(.0000718)
Own or farm agricultural land	-.0482
	(.0343)
P-value of joint F-test	0.024
N	1439

Notes: The table presents effects of attrition on pre-specified household characteristics jointly among our sample, where the outcome $Attrition_{ijT}$ is an indicator that equals 1 if a household i in location j was interviewed at baseline (2009 in Kenya, 2012 in Ethiopia), but not during the long-run follow-up survey round (2020 in Kenya and 2022 in Ethiopia). Estimated coefficients and robust standard errors (in parentheses) are reported. All estimations include community fixed effects. * denotes significance at 0.10; ** at 0.05; and *** at 0.01. The p-value of the joint significance test for all variables across attrition is reported at second from the bottom row.

Table A6: Differential attrition across coupon receipt status

	Outcome: Interviewed at baseline but not in the final round (=1)
	(1)
***Sale season 1*: 2010 JF (Kenya), 2012 AS (Ethiopia)**	
Received coupon	.0214
	(.026)
Discount Rate	-.000136
	(.000498)
***Sale season 2*: 2011 JF (Kenya), 2013 JF (Ethiopia)**	
Received coupon	-.0362
	(.0242)
Discount Rate	.000616
	(.000467)
***Sale season 3*: 2011 AS (Kenya), 2013 AS (Ethiopia)**	
Received coupon	-.0525**
	(.0249)
Discount Rate	.000704
	(.000478)
***Sale season 4*: 2012 AS (Kenya), 2014 JF (Ethiopia)**	
Received coupon	.00744
	(.0252)
Discount Rate	-.000327
	(.000474)
***Sale season 5*: 2013 JF (Kenya), 2014 AS (Ethiopia)**	
Received coupon	.00978
	(.0248)
Discount Rate	-.000154
	(.000464)
***Sale season 6*: 2013 AS (Kenya), 2015 JF (Ethiopia)**	
Received coupon	.0394
	(.0265)
Discount Rate	-.000524
	(.000372)
N	1439

Notes: The table presents the effect of whether or not a household has receive a coupon ("received coupon") and the discount rate assigned ("discount rate", ranging between 0% and 80%) on attrition, where the outcome $Attrition_{ijT}$ is an indicator that equals 1 if a household i in location j was interviewed at baseline (2009 in Kenya, 2012 in Ethiopia), but not during the long-run follow-up survey round (2020 in Kenya and 2022 in Ethiopia). Estimated coefficients and robust standard errors (in parentheses) are reported. All estimations include country and community fixed effects. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

B Interhousehold Spillovers

Our estimate of the Local Average Treatment Effect (LATE) is a valid estimator of the causal effect of IBLI if our design satisfies the following assumptions: (i) Stable Unit Treatment Values Assumption (SUTVA); (ii) the exclusion restriction; (iii) monotonicity (iv) exogeneity of the instrument.

To estimate the causal effect of IBLI on long-run outcomes, we use the number of randomized discount coupons received during the first three seasons of IBLI sales as an instrument for whether or not a respondent took up any IBLI during the first three seasons. This is a context where we should anticipate two-sided non-compliance, so we check that we satisfy the monotonicity assumption in Table C2. Our results demonstrate that the likelihood of IBLI take-up in the first three seasons monotonically increases with the number of coupons received during the first three seasons.

If we assume that the receipt of discount coupons and the take-up of insurance do not generate spillovers – and thus SUTVA is not violated – it is unlikely that the exclusion restriction is violated through spillovers. This is because discount coupons were randomly assigned across households in communities, were non-transferable and expired at the end of each season. However, if we relax SUTVA, we should consider spillovers in the second stage, from a herder's insurance purchase decision onto their peers' insurance purchase decision; from a herder's purchase decision onto their peers' outcomes; or from a herder's outcomes onto their peers' outcomes. Furthermore, spillovers may also arise in the first stage, where a herder's receipt of a discount coupon affects their peers' insurance purchase. Because the effect of a herder's discount coupons on their long-run outcomes still runs solely through the herder's insurance purchase, these spillovers would not violate the exclusion restriction. However, the effect of our instrument on insurance purchase now consists of a direct and an indirect effect.

Figure 4 summarizes all potential spillovers, of which not all are a concern from the perspective of estimating a valid LATE. For completeness, we start by providing examples of each potential spillover in our context in the list below before we discuss which of those create a concern from the perspective of generating a valid LATE.

- Pathway (1) and (2): The receipt of a discount coupon by a herder affects the likelihood that their peers take-up insurance, and vice versa. In our context, examples of this might be that herder i, upon receiving the discount coupon, also receives *information* about insurance that they communicate to $-i$, which makes $-i$, irrespective of their own coupon receipt, more likely to purchase insurance. Alternatively, receiving a discount coupon by i could lead to *status concerns* that (dis)incentivize $-i$ to purchase insurance, irrespective of their own coupon receipt.

- Pathway (3) and (4): The insurance purchase by a herder has an effect on the likelihood that their peer purchases insurance and vice versa. Examples of this in our context are *social learning*, where $-i$ learns about insurance from i, or *copying*, where $-i$ wants to exhibit the same behaviour as i. Another example is *free-riding*, which refers to the fact that i's insurance purchase decreases the incentive for $-i$ to purchase insurance. This may occur because i and $-i$ informally share risk through transfers, and $-i$ anticipates transfers following claim payments by i, or in case $-i$ views i's insurance purchase as an opportunity to learn about the insurance product.

- Pathway (5) and (6): The insurance purchase by herder i changes the outcomes of a peer (Y_{-ij}) directly, not through the outcomes of i (see pathway (7) and (8) below). An example would be a case where the willingness to share risk through informal transfers by either i or $-i$ is changed as a result of their insurance status. For example, Takahashi, Barrett, and Ikegami (2019) shows that a herder's insurance uptake has no effect on her willingness to transfer to peers, but insurance purchase by peers does increase herder i's willingness to transfer. Alternatively, if formal insurance is available, and i purchases insurance but $-i$ does not, i may become less willing to transfer to $-i$ because $-i$ refrained from protecting themselves by purchasing insurance and instead decided to free-ride on i's insurance purchase (Berg, Blake, and Morsink, 2022).

- Pathway (7) and (8): The outcomes of herder i affect the outcomes of their peers, or vice versa. This is empirically difficult to distinguish from the mechanisms discussed in pathways (5) and (6). Examples would be where claim payments received by i increase i's income, and as a result, i increases transfer to $-i$.

Based on Figure 4 we can categorize threats to a valid LATE as arising from a combination of violations of the exclusion restriction, SUTVA, and violations of SUTVA only.

From the perspective of the *exclusion restriction*, the only pathways of spillovers that are a concern are pathways from D_{ij} to Y_{ij} that do not run through I_{ij}. These are:

- pathway (1) $\rightarrow$ (6)
- pathway (1) $\rightarrow$ B $\rightarrow$ (7)

The following pathways are not a concern from the perspective of the exclusion restriction, because they all run from D_{ij} to I_{ij} to Y_{ij}:

- pathway (1) $\rightarrow$ (3) $\rightarrow$ A;
- pathway (1) $\rightarrow$ (3) $\rightarrow$ (5) $\rightarrow$ (7);
- pathway (11) $\rightarrow$ (4) $\rightarrow$ (6);
- pathway (11) $\rightarrow$ (4) $\rightarrow$ B $\rightarrow$ (7).

Any pathways that run from D_{-ij} to Y_{ij}, either through I_{ij} or I_{-ij} do not pose a violation of the exclusion restriction because they do not affect the causal effect of the instrument D_{ij} on I_{ij}. They do, however, change the overall population of compliers to treatment, and – if spillovers exist in the second stage – would thus affect the estimate of the $\hat{I}_{ij}$ on Y_{ij}. This can happen through:

- (2) $\rightarrow$ A;
- (2) $\rightarrow$ (4) $\rightarrow$ (6);
- (2) $\rightarrow$ (4) $\rightarrow$ B $\rightarrow$ (7);
- (10) $\rightarrow$ (3) $\rightarrow$ A;
- (10) $\rightarrow$ (3) $\rightarrow$ (5) $\rightarrow$ (7);
- (10) $\rightarrow$ (6)
- (10) $\rightarrow$ (B) $\rightarrow$ (7).

As we only have random variation in D_{ij} and D_{-ij}, we can only estimate the causal pathways (1), (2), (10), and (11). Any effects beyond this coming from D_{ij} – such as pathway (1) $\rightarrow$ (3) – cannot be causally interpreted. It is the result of the fact that instrumenting I_{-ij} with D_{ij} is required for a causal interpretation, but the existence of (11) implies that the exclusion restriction would be violated if we do so.

Therefore, we first focus on estimating the direct effects on the first stage only, which would include:

- pathway (1): D_{ij} on I_{-ij}
- pathway (2): D_{-ij} on I_{ij}
- pathway (10): D_{-ij} on I_{-ij}
- pathway (11): D_{ij} on I_{ij}

and the combinations of the two direct effects:

- pathways (1) and (10): D_{ij} & D_{-ij} on $\bar{I}_{-ij}$
- pathways (2) and (11): D_{ij} & D_{-ij} on I_{ij}

B.1 Estimation Strategies

To investigate spillovers empirically, we construct the following variables for $-i$:

- $-i's$ coupon receipt ($\overline{D}_{-ij}$): This is constructed by creating a variable for each herder i that is the mean of the number of coupons received in the first three seasons by all other herders ($-i$) in their community j:

$$\overline{D}_{-ij} := \frac{1}{N_j} \sum_{-i_j=1}^{n_j} [\text{No. of coupons received - first three seasons}]_{-ij}$$

  where [No. of coupons received - first three seasons]$_{-ij}$ is the total number of coupons distributed in the community to all herders except for i in the initial three seasons.

- $-$i's insurance uptake ($\bar{I}_{-ij}$): This is constructed by creating a variable for each herder i that is the share of herders $-i$ out of all herders in the community except for i that purchased any insurance during the first three seasons:

$$\bar{I}_{-ij} := \frac{1}{N_j} \sum_{-i_g=1}^{n_j} [\text{Any insurance purchased - first three seasons}]_{-ij}$$

  where [Any insurance purchased - first three seasons]$_{-ij}$ is a binary variable that is one if the households bought insurance at least once in the first three sales seasons.

We also create a vector of control covariates for all herders $-i$ in community j in the same way that we create the above-mentioned variables, which we define as $\overline{X}_{-ij0}$.

We show the summary statistics of these variables in Table B1. By construction – because all herders are included as i in D_{ij} and Y_{ij}, and they are also included as $-i$ in $\overline{D}_{-ij}$ and $\overline{Y}_{-ij}$ – the means of these $-i$ variables across the entire sample are always the same as the mean for the i variables, but the standard deviation is reduced. As a result, if one were to estimate correlations between these two variables, mechanically, we would expect a negative correlation.

Table B1: Summary statistics of the spillover variables

	Kenya				Ethiopia				Pooled			
	Mean/SD	Min	Max	Obs	Mean/SD	Min	Max	Obs	Mean/SD	Min	Max	Obs
D_{ij}: No. of coupons received – first three seasons	1.78	0.00	3.00	781	1.57	0.00	2.00	398	1.71	0.00	3.00	1179
	[0.87]				[0.60]				[0.79]			
I_{ij}: Any insurance purchase - first three seasons	0.43	0.00	1.00	781	0.45	0.00	1.00	398	0.44	0.00	1.00	1179
	[0.50]				[0.50]				[0.50]			
$\overline{D}_{-ij}$: Peers' mean no. of coupons received – first three season	1.78	1.65	1.88	781	1.57	1.35	2.00	398	1.71	1.35	2.00	1179
	[0.04]				[0.09]				[0.12]			
$\overline{I}_{-ij}$: Peers' any insurance purchase – first three seasons	0.43	0.15	0.82	781	0.45	0.00	1.00	398	0.44	0.00	1.00	1179
	[0.17]				[0.17]				[0.17]			
Peers' average: Age of household head in years	48.08	27.19	59.14	781	50.23	37.11	57.03	398	48.81	27.19	59.14	1179
	[6.14]				[4.55]				[5.74]			
Peers' average: Male headed household	0.63	0.00	0.88	781	0.79	0.50	1.00	398	0.68	0.00	1.00	1179
	[0.25]				[0.09]				[0.22]			
Peers' average: Household head's years of education	1.05	0.00	3.17	781	0.54	0.00	1.26	398	0.87	0.00	3.17	1179
	[0.76]				[0.42]				[0.70]			
Peers' average: Adult equivalent	4.68	3.59	6.37	781	4.94	3.90	6.30	398	4.77	3.59	6.37	1179
	[0.55]				[0.44]				[0.53]			
Peers' average: Dependency ratio	0.50	0.41	0.58	781	0.54	0.44	0.67	398	0.51	0.41	0.67	1179
	[0.03]				[0.04]				[0.04]			
Peers' average: Herd size (CMVE)	25.48	5.49	73.20	781	17.01	5.02	39.62	398	22.62	5.02	73.20	1179
	[19.99]				[7.29]				[17.28]			
Peers' average: Annual income per AE (USD)	121.45	11.96	339.46	781	102.79	7.91	245.99	398	115.15	7.91	339.46	1179
	[76.30]				[49.54]				[68.99]			
Peers' average: Own or farm agricultural land	0.18	0.00	0.88	781	0.65	0.00	1.00	398	0.34	0.00	1.00	1179
	[0.30]				[0.27]				[0.37]			
Peers' average: Fully settled	0.23	0.00	0.92	781	0.76	0.00	0.95	398	0.41	0.00	0.95	1179
	[0.23]				[0.13]				[0.32]			
Observations	781				398				1179			

Notes: The table presents the summary statistics – mean, standard deviation (in square brackets), minimum value, maximum value, and the number of observations of each variable – of the study sample, by country, and for the pooled sample. Adult equivalent is the weighted sum of number of the household members. Age-specific weights for adult equivalent are as follows: A household member between 16 to 65 (AE=1), a child under 5 (0.5 AE), a child between 5 to 15 (AE=0.7), a household member above 65 (AE=0.7).

Table B2: Spillover effects: First stage and mechanical correlation

	Outcome: Number of coupons received - first three seasons		Outcome: Any insurance purchase - first three seasons					
	D_{ij}: Recipient's	$\overline{D}_{-ij}$: Peers'	I_{ij}: Recipient's			$\overline{I}_{-ij}$: Peers'		
No. of coupons received – first three seasons	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D_{ij}: Recipient's		-0.005	0.119***		0.118***	-0.005		-0.005
		(0.004)	(0.017)		(0.017)	(0.006)		(0.006)
$\overline{D}_{-ij}$: Peers'	-0.225			-0.229*	-0.203		-0.101**	-0.103**
	(0.179)			(0.125)	(0.124)		(0.040)	(0.040)
Pathway (DAG)	(12)	(13)	(11)	(2)	(2);(11)	(1)	(10)	(1);(10)
Recipient controls (i)								
Peers' controls (-i)								
Community FE								
(Control) mean outcomes	1.707	1.707	0.237	.	0.237	0.446	.	0.446
Observations	1179	1179	1179	1179	1179	1179	1179	1179

Notes: The table presents the effects of number of coupons received by recipients and peers in the first three seasons on whether the recipients and their peers have received coupons and purchased any insurance during the first three seasons. Columns 1 and 2 show the mechanical correlation of the number of coupons received in the first three seasons between the recipients and their peers. Columns 3-5 show the effect of whether or not the recipient received a coupons, the average number of coupons received by their peers, and both jointly on any insurance purchased by the recipient. Columns 6-8 show the effect of whether or not the recipient received a coupons, the average number of coupons received by their peers, and both jointly on any insurance purchased by peers. D_{ij} and $\overline{D}_{-ij}$ are the number of coupons received by the recipient i and the mean of the number of coupons received in the first three seasons by all other herders ($-i$) in their community j, respectively. I_{ij} and $\overline{I}_{-ij}$ are any insurance purchase in the first three seasons by recipient i and the share of herders $-i$ out of all herders in the community except for i that purchased any insurance during the first three seasons, respectively. The row "Pathway (DAG)" indicates the potential spillover pathways that may exist in our first- or second- stages corresponding to the numbers in Figure 4. The row “Control mean” indicates mean outcomes for columns 1 and 2, and mean outcomes for those who did not purchase any insurance in the first three seasons for Columns 3-8. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Table B3: Spillover effects on education outcomes

	Of households members who were school-aged during the experiment			Share of children in the household		
	Maximum years of education	Total years of education	Average years of education	Working full-time	Working part-time	Studying full-time
	(1)	(2)	(3)	(4)	(5)	(6)
$\widehat{I_{ij}}$: Any insurance purchase - first three seasons	2.483	6.169*	1.967	-0.382	-0.132	0.408
	(1.520)	(3.587)	(1.262)	(0.294)	(0.259)	(0.252)
$\hat{\bar{I}}_{-ij}$: Peers' any insurance purchase – first three season	-13.013***	-31.142***	-9.761***	-0.643	1.362*	-0.552
	(3.814)	(7.826)	(2.940)	(0.932)	(0.704)	(0.724)
Recipient controls (i)						
Peer's controls (-i)	✓	✓	✓	✓	✓	✓
Control mean	7.255	13.275	5.296	0.345	0.208	0.159
Village FE						
Observations	742	742	742	376	376	376

Notes: The table presents the effects of any insurance purchase during the first three seasons by the recipient (I_{ij}) and the mean insurance purchase by peers I_{-ij} during the first three seasons as instrumented by the number of coupons received by the recipient i (D_{ij}) and the mean of the number of coupons received in the first three seasons by all other herders ($-i$) in their community j (D_{-ij}) on education outcomes. The dependent variables "Maximum years of education", "Total years of education", and "Average years of education" (Columns 1-3) are measured among household members who were school-aged at any point during the initial three periods of experiments, i.e., household members who are currently 15-29 years old in Kenya and 15-17 years old in Ethiopia (data in Ethiopia is limited to those up to 17 years old). The dependent variables "Working full-time", "Working part-time", and "Studying full-time" (Columns 4-6) were only measured at endline in Ethiopia, and represent the share of current children aged 5-17 in the household that are reported to be engaged in each activity. Data includes 742 of the 1179 households for columns 1-3, excluding households without household members who were school-aged during the experiment. Standard errors are clustered at the household-level, as this was the level of randomization. The row "Control mean" indicates the average outcomes for those who did not receive any coupons in the first three seasons. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

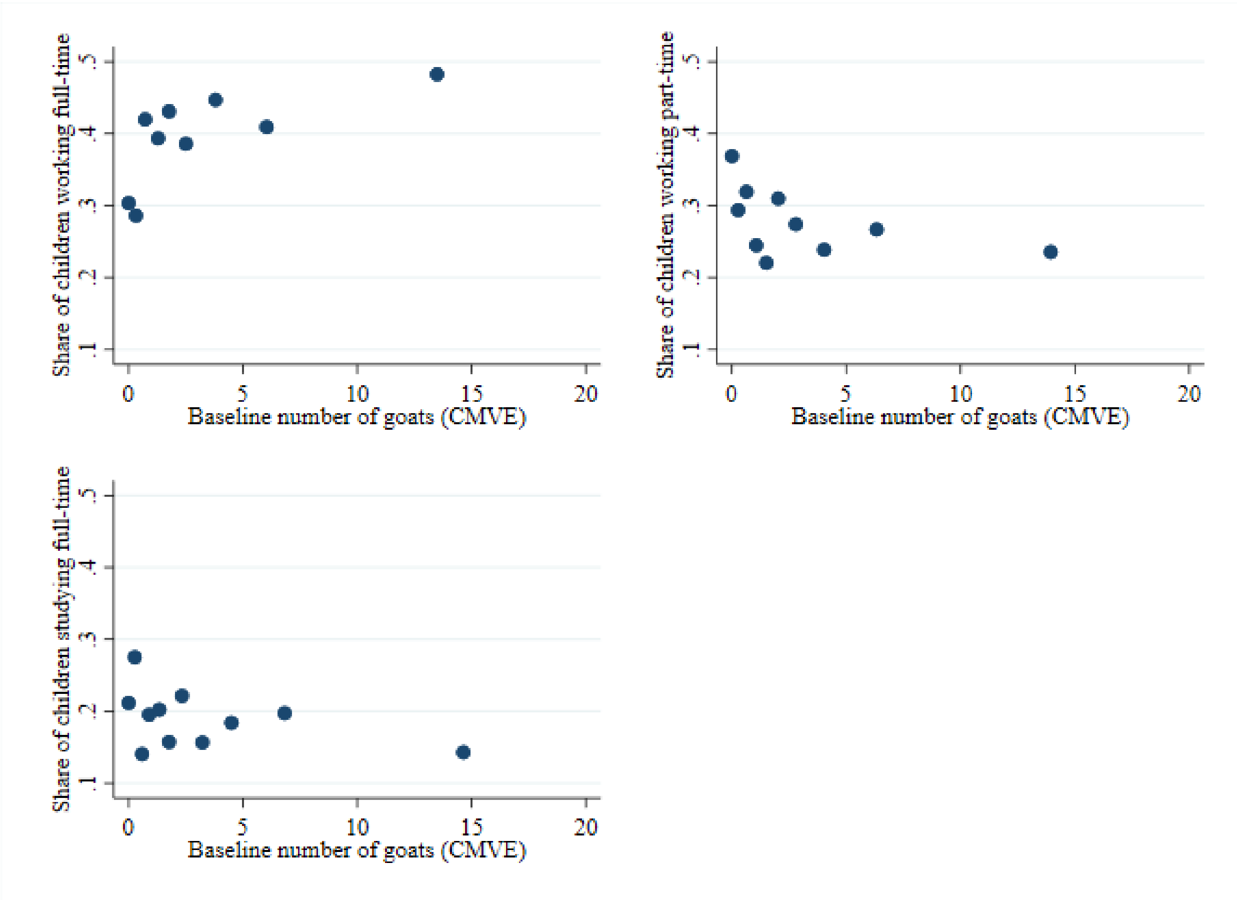
Table B4: Spillover effects on Prespecified primary outcome: Herd composition

	Outcome: N of animal type in CMVE / Total N of animals in CMVE					
	Camel	Cattle	Goats	Sheep	Camels & cattle	Goats & sheep
	(1)	(2)	(3)	(4)	(5)	(6)
$\widehat{I_{ij}}$: Any insurance purchase - first three seasons	0.130	0.108	-0.225**	-0.006	0.233**	-0.233**
	(0.091)	(0.084)	(0.097)	(0.051)	(0.115)	(0.115)
$\hat{\hat{I}}_{-ij}$: Peers' any insurance purchase – first three season	-0.309	0.307	0.009	-0.118	0.052	-0.052
	(0.196)	(0.193)	(0.230)	(0.118)	(0.262)	(0.262)
Recipient controls (i)						
Peers' controls (-i)	✓	✓	✓	✓	✓	✓
Control mean	0.255	0.311	0.293	0.141	0.566	0.434
Village FE						
Observations	987	987	987	987	987	987

Notes: The table presents the effects of any insurance purchase during the first three seasons by the recipient (I_{ij}) and the mean insurance purchase by peers I_{-ij} during the first three seasons as instrumented by the number of coupons received by the recipient i (D_{ij}) and the mean of the number of coupons received in the first three seasons by all other herders ($-i$) in their community j (D_{-ij}) on herd composition. The dependent variable "herd composition" is measured as the number of animals of each animal type that the household herds expressed in CMVE divided by the total number of animals that the household herds expressed in CMVE. Standard errors are clustered at the household-level, as this was the level of randomization. Data includes 987 of the 1179 households excluding households that are not currently herding any livestock. The row "Control mean" indicates the average outcomes for those who did not receive any coupons in the first three seasons. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. * denotes significance at 0.10; ** at 0.05; and *** at 0.01. In Kenya, 1 CMVE= 0.625 camel=1 cattle=10 goats/sheep, and in Ethiopia, 1 CMVE=0.4 camel=1 cattle=6.25 goats/sheep.

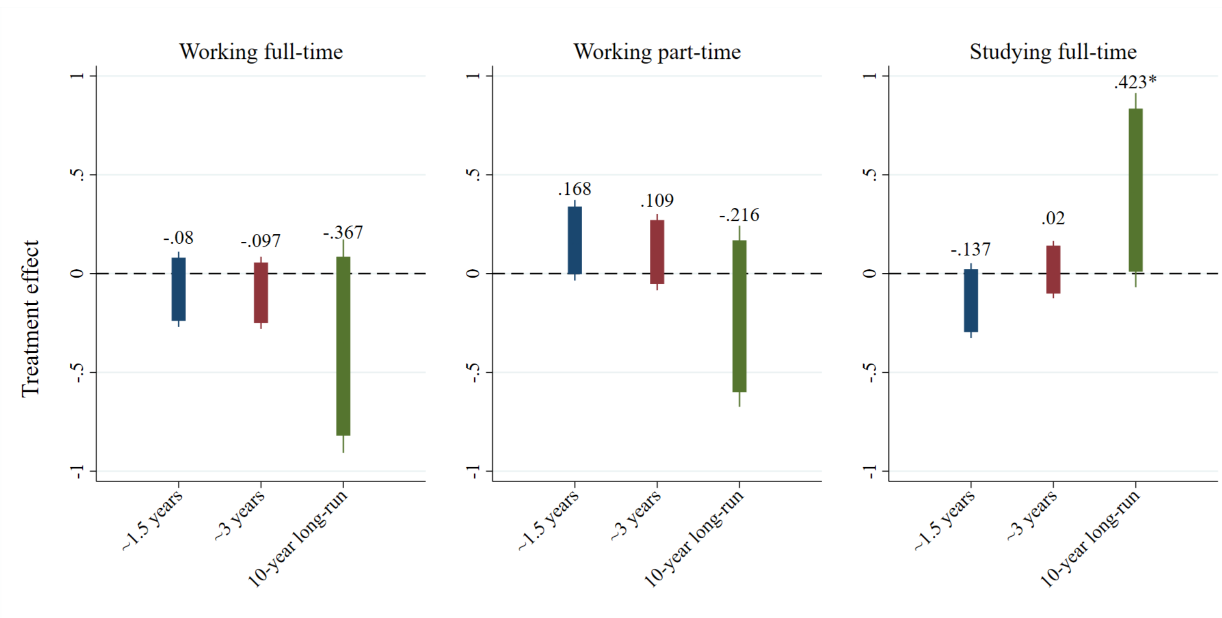
C Tables and Figures Referenced in Text

Figure C1: Child time use and number of goats at baseline survey



Notes: These figures present the bins scatter plot between share of children who are either working full-time (top left panel), working part-time (top right panel), or studying full-time (bottom left panel) and the number of goats (CMVE) at baseline survey, controlling for the baseline total livestock holdings. Cattle market-value equivalent (CMVE) is a unit to aggregate the animals across different animal types based on their market values using panel survey data. In Kenya, 1 CMVE= 0.625 camel=1 cattle=10 goats/sheep, and in Ethiopia, 1 CMVE=0.4 camel=1 cattle=6.25 goats/sheep. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Figure C2: Dynamic effects on maximum, total, and average years of education



Notes: This figure presents the estimated Local Average Treatment Effects of IBLI purchase in the first three seasons – instrumented by the number of discount coupons received in the first three seasons – on the share of children who are "working full-time", "working part-time", and "studying full-time" measured among household members who were school-aged as measured during the first three rounds of the panel survey, i.e., 15-29 years old in Kenya and 15-17 years old in Ethiopia i) after the third sales season, ii) after the end of the experiment (sixth sales season), and iii) at the 10-year follow up (Data in Kenya is not available in 10-year long-run). The boxes present the 90 percent confidence intervals, and the lines represents the 95 percent confidence intervals. The numbers above the boxes present the point estimate of the LATE. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Table C1: Summary statistics of outcome variables

	Kenya				Ethiopia				Pooled			
	Mean/SD	Min	Max	Obs	Mean/SD	Min	Max	Obs	Mean/SD	Min	Max	Obs
Herd size (CMVE)	12.96	0.00	349.80	781	16.51	0.00	498.78	398	14.16	0.00	498.78	1179
	[24.46]				[38.72]				[30.07]			
Share of camels in herd (CMVE)	0.31	0.00	1.00	619	0.10	0.00	1.00	395	0.23	0.00	1.00	1014
	[0.38]				[0.22]				[0.34]			
Share of cattle in herd (CMVE)	0.21	0.00	1.00	619	0.65	0.00	1.00	395	0.38	0.00	1.00	1014
	[0.35]				[0.23]				[0.38]			
Share of goats in herd (CMVE)	0.34	0.00	1.00	619	0.18	0.00	1.00	395	0.28	0.00	1.00	1014
	[0.35]				[0.17]				[0.30]			
Share of sheep in herd (CMVE)	0.14	0.00	1.00	619	0.06	0.00	0.83	395	0.11	0.00	1.00	1014
	[0.20]				[0.08]				[0.17]			
Annual total household cash earning (USD)	515.08	0.00	5636.45	781	564.31	0.00	3649.52	398	531.70	0.00	5636.45	1179
	[671.37]				[597.82]				[647.64]			
Maximum years of education	7.58	0.00	14.00	578	4.96	0.00	12.00	164	7.01	0.00	14.00	742
	[4.97]				[3.60]				[4.82]			
Herd management expenditure (USD)	139.34	0.00	3648.66	666	227.00	0.00	4817.14	398	172.13	0.00	4817.14	1064
	[290.75]				[425.09]				[349.53]			
Annual milk income (USD) (earnings and in-kind)	540.99	0.00	21957.05	781	111.00	0.00	11895.60	398	395.84	0.00	21957.05	1179
	[1361.23]				[634.35]				[1184.86]			
Livestock lost in the past 12 months (CMVE)	3.00	0.00	56.80	781	9.95	0.00	352.32	398	5.35	0.00	352.32	1179
	[6.38]				[24.68]				[15.59]			
N of lost camel	1.08	0.00	28.00	578	0.57	0.00	25.00	398	0.87	0.00	28.00	976
	[3.25]				[2.29]				[2.91]			
N of lost cattle	0.53	0.00	40.00	578	8.36	0.00	300.00	398	3.73	0.00	300.00	976
	[2.46]				[22.47]				[14.97]			
Number of lost goats/sheep	17.95	0.00	270.00	578	1.02	0.00	52.32	398	11.05	0.00	270.00	976
	[32.47]				[3.09]				[26.40]			
Distress sale in the past 12 months (CMVE)	0.49	0.00	25.60	781	.	.	.	0	0.49	0.00	25.60	781
	[2.01]				[.]				[2.01]			
Share of children working full-time	.	.	.	0	0.28	0.00	1.00	376	0.28	0.00	1.00	376
	[.]				[0.31]				[0.31]			
Share of children working part-time	.	.	.	0	0.18	0.00	1.00	376	0.18	0.00	1.00	376
	[.]				[0.30]				[0.30]			
Share of children studying full-time	.	.	.	0	0.23	0.00	1.00	376	0.23	0.00	1.00	376
	[.]				[0.29]				[0.29]			
IBLI uptake in the past 12 months (=1 if purchased)	0.00	0.00	1.00	781	0.15	0.00	1.00	398	0.05	0.00	1.00	1179
	[0.04]				[0.36]				[0.22]			
IBLI uptake in the past 12 months (CMVE)	0.02	0.00	13.80	781	1.80	0.00	100.00	398	0.62	0.00	100.00	1179
	[0.49]				[7.22]				[4.30]			
Observations	781				398				1179			

Notes: All columns present mean, standard deviations (in square brackets), and the number of observations for each variable. Age-specific weights for adult equivalent are as follows: A household member between 16 to 65 (AE=1), a child under 5 (0.5 AE), a child between 5 to 15 (AE=0.7), a household member above 65 (AE=0.7). Dependency ratio is calculated by the number of dependents (household members younger than 15 years old and older than 65 years old) divided by the number of household members. Herd size in CMVE is the sum of the animals herded by the household, aggregated using cattle market-value equivalent. Cattle market-value equivalent (CMVE) is a unit to aggregate the animals across different animal types based on their market values using panel survey data. In Kenya, 1 CMVE= 0.625 camel=1 cattle=10 goats/sheep, and in Ethiopia, 1 CMVE=0.4 camel=1 cattle=6.25 goats/sheep. Annual total household cash earning is the sum of income from the following categories: sale of livestock, sale of livestock products, crop cultivation, salaried employment, casual labor, business and petty trading, and other major sources of income excluding gifts and remittances during the recent 4 pastoral seasons. Herd management expenditure includes expenditure on water, fodder, supplementary feeding, and veterinary expenses.

Table C2: Checking monotonicity assumption

	Number of seasons purchase IBLI (%)			
Panel A. Number of coupons recipient's received	0	1	2	3
0	76.250	20.000	3.750	0.000
1	65.819	29.096	4.802	0.282
2	50.953	39.515	9.185	0.347
3	43.452	37.500	19.048	0.000

	Whether or not to purchase IBLI (%)	
Panel B. Number of coupons recipient's received	0	1
0	76.250	23.750
1	65.819	34.181
2	50.953	49.047
3	43.452	56.548

Notes: The table shows the relationship between the number of coupons recipients received and the purchase of IBLI in the initial three sales seasons. Panel A presents the number of seasons in which IBLI was purchased, while Panel B indicates whether IBLI was purchased in any of the initial three sales seasons.

Table C3: Number of animals by animal type

	N of animals (CMVE)				Raw N of animals			
	Camel	Cattle	Goat	Sheep	Camel	Cattle	Goat	Sheep
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any insurance purchased	1.446	-1.101	-0.396	-0.275	0.853	-1.101	-5.821	-3.479
	(4.428)	(4.856)	(0.967)	(0.585)	(2.713)	(4.856)	(8.016)	(5.211)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	9.290	8.037	3.264	2.543	5.638	8.037	21.512	16.850
Observations	1179	1179	1179	1179	1179	1179	1179	1179

Notes: The table presents estimated Local Average Treatment Effects of any insurance purchase in the first three seasons, instrumented by the number of discount coupons received in the first three seasons on the number of animals by animal type. The dependent variables for columns 1-4 present the number of each animal type expressed in CMVE, while columns 5-8 present the raw number of each livestock. Community fixed effects are included as randomization blocked at community level. Standard errors are clustered at the household-level, as this was the level of randomization. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. The row "Control mean" indicates mean outcomes for those who did not receive any coupons in the first three seasons. * denotes significance at 0.10; ** at 0.05; and *** at 0.01. In Kenya, 1 CMVE= 0.625 camel=1 cattle=10 goats/sheep, and in Ethiopia, 1 CMVE=0.4 camel=1 cattle=6.25 goats/sheep.

Table C4: Number of livestock – subsample analysis by baseline TLU class

	N of animals (CMVE)				Raw N of animals			
	Camel	Cattle	Goat	Sheep	Camel	Cattle	Goat	Sheep
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Low or middle baseline TLU class								
Any insurance purchased	-5.140	-5.154	-1.882*	-1.233*	-3.345	-5.154	-18.375*	-11.377
	(4.866)	(4.578)	(1.036)	(0.733)	(2.995)	(4.578)	(9.470)	(6.947)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	5.729	6.136	2.346	2.117	3.542	6.136	15.424	14.000
Observations	790	790	790	790	790	790	790	790
Panel B: High baseline TLU class								
Any insurance purchased	7.748	4.588	2.310	1.539	4.681	4.588	15.245	11.291
	(8.271)	(10.034)	(2.292)	(1.177)	(5.017)	(10.034)	(16.718)	(8.925)
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	19.295	13.381	5.845	3.740	11.524	13.381	38.619	24.857
Observations	389	389	389	389	389	389	389	389

Notes: The table presents estimated Local Average Treatment Effects of any insurance purchase in the first three seasons, instrumented by the number of discount coupons received in the first three seasons on the number of livestock holdings by baseline herd terciles, where panel A shows effects for households with herd sizes below 20 TLU at baseline (the lowest 67% of observations in terms of the baseline herd size distribution), and panel B shows effects for herd sizes above 20 TLU (the highest 33% of observations in terms of the baseline herd size distribution). The dependent variables for columns 1-4 present the number of each animal type expressed in CMVE, while columns 5-8 present the raw number of each livestock. Community fixed effects are included as randomization blocked at community level. Standard errors are clustered at the household-level, as this was the level of randomization. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. The row "Control mean" indicates mean outcomes for those who did not receive any coupons in the first three seasons for those who are in each subgroup. * denotes significance at 0.10; ** at 0.05; and *** at 0.01. In Kenya, 1 CMVE= 0.625 camel=1 cattle=10 goats/sheep, and in Ethiopia, 1 CMVE=0.4 camel=1 cattle=6.25 goats/sheep.

Table C5: Long-run effects of catastrophic drought insurance on income

	Aggregate	Mutually exclusive categories (USD)								
	Annual total household income (USD)	Annual in-kind milk income (USD)	Annual earnings from milk (USD)	Annual in-kind slaughter income (USD)	Annual earnings from slaughter (USD)	Annual animal birth income (USD)	Annual in-kind crop income (USD)	Annual earnings income from crop (USD)	Annual employment (food for work) income (USD)	Annual earnings from the rest (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Any insurance purchased	322.285	273.583	37.506	-20.925	47.719	-42.832	48.226***	5.381	-10.384	-38.772
	(510.801)	(306.754)	(154.578)	(36.817)	(35.202)	(98.798)	(16.955)	(29.255)	(8.667)	(204.527)
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[0.077]	[1.000]	[1.000]	[1.000]
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	1290.881	110.007	343.598	63.310	20.065	173.375	3.733	8.350	5.781	562.661
Observations	1179	1179	1179	1179	1179	1179	1179	1179	1179	1179

Notes: The table presents Local Average Treatment Effects of any insurance purchase in the first three seasons, instrumented by the number of discount coupons received in the first three seasons on income outcomes. The dependent variable of column 1 is the aggregated annual total household income (sum of columns 2-10 expressed in USD. The dependent variables of columns 2-10 are annual income from each category of income expressed in USD. Community fixed effects are included as randomization blocked at community level. Standard errors are clustered at the household-level, as this was the level of randomization. The FDR adjusted p-values (q-values) are reported in square brackets for outcomes that were not pre-specified, calculated according to the sharpened process Anderson (2008). Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. The row "Control mean" indicates the average outcomes for those who did not receive any coupons in the first three seasons. * denotes significance at 0.10; ** at 0.05; and *** at 0.01. Please refer to Appendix Table F1 and Appendix Table F2 for the definition of outcome variables.

Table C6: Long-run effects of catastrophic drought insurance on aggregated income

	Annual income (USD)		= 1 if the outcome > 0	
	Total livestock income	Total crop income	Extensive margin – Annual total livestock income	Extensive margin - Annual total crop income
	(1)	(2)	(3)	(4)
Any insurance purchased	351.948	52.944	0.072	0.090
	(444.734)	(34.924)	(0.110)	(0.087)
Controls	✓	✓	✓	✓
Control mean	557.964	26.863	0.796	0.120
Observations	1179	1179	1179	1179

Notes: The table presents Local Average Treatment Effects of any insurance purchase in the first three seasons, instrumented by the number of discount coupons received in the first three seasons on income outcomes aggregated over livestock and crop. The dependent variables of columns 1 and 3 are annual total livestock income expressed in USD and its dummy, respectively, while the ones in columns 2 and 4 are annual total crop income expressed in USD and its dummy, respectively. Community fixed effects are included as randomization blocked at community level. Standard errors are clustered at the household-level, as this was the level of randomization. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. The row "Control mean" indicates the average outcomes for those who did not receive any coupons in the first three seasons. * denotes significance at 0.10; ** at 0.05; and *** at 0.01. Please refer to Appendix Table F1 and Appendix Table F2 for the definition of outcome variables.

Table C7: Long-run effects of catastrophic drought insurance on educational attainment by gender

	Maximum years of education	Total years of education	Average years of education
	(1)	(2)	(3)
Panel A: Among male household members			
Any insurance purchased	3.469**	6.705**	3.070**
	(1.595)	(3.149)	(1.303)
Controls	✓	✓	✓
Control mean	6.575	9.261	4.883
Observations	478	499	499
Panel B: Among female household members			
Any insurance purchased	2.170	3.443	2.724
	(1.768)	(3.314)	(1.785)
Controls	✓	✓	✓
Control mean	6.306	8.194	5.530
Observations	346	346	346

Notes: The table presents Local Average Treatment Effects of any insurance purchase in the first three seasons, instrumented by the number of discount coupons received in the first three seasons on education outcomes by gender composition: Panel A presents the effects on the dependent variables of maximum, total, and average years of education only among male household members, while Panel B does so only among female household members. Community fixed effects are included as randomization blocked at community level. Standard errors are clustered at the household-level, as this was the level of randomization. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. The row "Control mean" indicates the average outcomes for those who did not receive any coupons in the first three seasons. Data includes households which have male and female members for each panel, excluding households without household members who were school-aged during the experiment. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Table C8: Education - not yet school age during the experiment but were at endline

	Maximum years of education	Total years of education	Average years of education
	(1)	(2)	(3)
Any insurance purchased	1.079	0.275	0.604
	(0.743)	(1.240)	(0.503)
Baseline outcome			
Controls	✓	✓	✓
Control mean	3.203	4.514	2.041
Observations	1015	1015	1015

Notes: The table presents Local Average Treatment Effects of any insurance purchase in the first three seasons, instrumented by the number of discount coupons received in the first three seasons on education outcomes. The dependent variables are "Maximum years of education", "total years of education", and "average years of education" among household members who were not yet school-aged during experiment (i.e., 6-14 in Kenya and 7-14 in Ethiopia at endline survey) but were at endline survey (2020 in Kenya and 2022 in Ethiopia). Community fixed effects are included as randomization blocked at community level. Standard errors are clustered at the household-level, as this was the level of randomization. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. The row "Control mean" indicates the average outcomes for those who did not receive any coupons in the first three seasons. Data includes 1015 of the 1179 households excluding households without household members who were school-aged during the experiment. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Table C9: Effects on the number of young adults (18-25 years old, Kenya only)

	Outcomes: Number of young adults	
	(1)	(2)
Any insurance purchased	0.204	0.106
	(0.308)	(0.270)
Baseline N of young adults	0.040	-0.246***
	(0.039)	(0.049)
Adult equivalent		0.287***
		(0.024)
Herd size (CMVE)		-0.002*
		(0.001)
Controls		✓
Control mean	0.912	0.912
Observations	781	781

Notes: The table presents estimated Local Average Treatment Effects of any insurance purchase in the first three seasons, instrumented by the number of discount coupons received in the first three seasons on the number of young adults. The dependent variable is "number of young adults" (18-25 years old). Community fixed effects are included as randomization blocked at community level. Standard errors are clustered at the household-level, as this was the level of randomization. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. The row "Control mean" indicates the average outcomes for those who did not receive any coupons in the first three seasons. The data for the outcome is only available in Kenya which yields 781 observations. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Table C10: Effects on educational outcomes - subsample analysis by baseline TLU class

	Of household members who were school-aged at any point during initial three periods of experiments			Share of children in the household		
	Maximum years of education	Total years of education	Average years of education	Working full-time	Working part-time	Studying full-time
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Low or middle baseline TLU class						
Any insurance purchased	5.658**	14.535**	5.254**	-0.326	-0.207	0.449
	(2.337)	(5.723)	(2.124)	(0.479)	(0.461)	(0.478)
Controls	✓	✓	✓	✓	✓	✓
Control mean	6.917	11.528	5.051	0.317	0.198	0.172
Observations	484	484	484	245	245	245
Panel B: High baseline TLU class						
Any insurance purchased	-2.143	-4.744	-0.411	-0.346	-0.317	0.311
	(2.708)	(5.804)	(1.898)	(0.325)	(0.202)	(0.273)
Controls	✓	✓	✓	✓	✓	✓
Control mean	8.067	17.467	5.884	0.410	0.231	0.129
Observations	258	258	258	131	131	131

Notes: The table presents estimated Local Average Treatment Effects of any insurance purchase in the first three seasons, instrumented by the number of discount coupons received in the first three seasons on education outcomes by baseline herd terciles, where panel A shows effects for households with herd sizes below 20 TLU at baseline (the lowest 67% of observations in terms of the baseline herd size distribution), and panel B shows effects for herd sizes above 20 TLU (the highest 33% of observations in terms of the baseline herd size distribution). The dependent variables "Maximum years of education", "Total years of education", and "Average years of education" (Columns 1-3) measured among household members who were school-aged at any point during initial three periods of experiments, i.e., 15-29 years old in Kenya and 15-17 years old in Ethiopia (data in Ethiopia is limited to those up to 17 years old). The dependent variables "Working full-time", "Working part-time", and "Studying full-time" (Columns 4-6) are measured among the share of children aged 5-17 in Ethiopia (The outcome data for Kenya is not available at endline) in the household. Standard errors are clustered at the household-level, as this was the level of randomization. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. The row "Control mean" indicates the average outcomes for those who did not receive any coupons in the first three seasons. Data includes 742 of the 1179 households for columns 1-3, excluding households without school-aged children meeting the criteria. Sample of columns 4-6 consist of children aged 5-17 in Ethiopia (The outcome data for Kenya is not available at endline). Community fixed effects are included as randomization was stratified at community level. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Table C11: Change in educational attainment outcomes and change in the small ruminants

		Subsample by reduction in small ruminants			Pairwise t-test		
	Full sample	Sharp reduction (>.75)	Moderate reduction (<=.75)	No reduction	(2)-(3)	(3)-(4)	(2)-(4)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Change in maximum years of education	3.66	4.18	3.68	2.79	0.49	0.90*	1.39***
	[4.46]	[4.28]	[4.67]	[4.42]			
Change in total years of education	6.46	8.20	5.56	4.43	2.63**	1.14	3.77***
	[9.90]	[10.63]	[9.39]	[8.59]			
Change in average years of education	3.54	4.05	3.40	2.82	0.65*	0.59	1.24***
	[3.54]	[3.58]	[3.50]	[3.39]			
Observations	742	342	193	207	535	400	549

Notes: The table presents summary statistics for the changes in educational attainment among household members who were school-aged at any point during initial three periods of experiments, i.e., 15-29 years old in Kenya and 15-17 years old in Ethiopia (data in Ethiopia is limited to those up to 17 years old) by the change in small ruminants (goats and sheep) from baseline (2009 in Kenya, 2012 in Ethiopia) to endline survey (2020 in Kenya and 2022 in Ethiopia). Column 1 displays the mean and standard deviations for the full sample, while columns 2-4 show them by subsample divided into the magnitude of reduction in small ruminants: sharp reduction (>.75), moderate reduction (<=.75), and no reduction. Columns 5-7 illustrate the pairwise differences with statistical differences between categories for each outcome. Data includes 742 of the 1179 households, excluding households without household members who were school-aged during the experiment. S * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Table C12: Herd composition — short-run and long-run

	Outcome: N of animal type in CMVE / Total N of animals in CMVE											
	Camel			Cattle			Goat			Sheep		
	3rd sales season	End of experiment	10-year long-run	3rd sales season	End of experiment	10-year long-run	3rd sales season	End of experiment	10-year long-run	3rd sales season	End of experiment	10-year long-run
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Any insurance purchased	0.071	-0.025	0.104	-0.007	0.095	0.106	-0.036	-0.090*	-0.211**	-0.023	0.035	0.005
	(0.053)	(0.047)	(0.088)	(0.053)	(0.058)	(0.081)	(0.059)	(0.053)	(0.094)	(0.028)	(0.034)	(0.050)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	0.312	0.287	0.255	0.334	0.329	0.311	0.244	0.267	0.293	0.109	0.117	0.141
Observations	1085	1069	987	1085	1069	987	1085	1069	987	1085	1069	987

Notes: The table presents estimated Local Average Treatment Effects of any insurance purchase in the first three seasons, instrumented by the number of discount coupons received in the first three seasons on herd composition at three time periods: i) after the third sales season, ii) after the end of the experiment (sixth sales season), and iii) at the 10-year follow up. The dependent variable "herd composition" is measured as the number of animals of each animal type that the household herds expressed in CMVE divided by the total number of animals that the household herds expressed in CMVE. Community fixed effects are included as randomization blocked at community level. Standard errors are clustered at the household-level, as this was the level of randomization. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. The row "Control mean" indicates the average outcomes for those who did not receive any coupons in the first three seasons. * denotes significance at 0.10; ** at 0.05; and *** at 0.01. In Kenya, 1 CMVE= 0.625 camel=1 cattle=10 goats/sheep, and in Ethiopia, 1 CMVE=0.4 camel=1 cattle=6.25 goats/sheep.

Table C13: Herd composition large versus small ruminants - short-run and long-run

	N of animals (CMVE) / Total herd size (CMVE)					
	Camels and cattle			Goats and sheep		
	3rd sales season	End of experiment	10-year long-run	3rd sales season	End of experiment	10-year long-run
	(1)	(2)	(3)	(4)	(5)	(6)
Any insurance purchased	0.060	0.062	0.209*	-0.060	-0.062	-0.209*
	(0.063)	(0.064)	(0.112)	(0.063)	(0.064)	(0.112)
Controls	✓	✓	✓	✓	✓	✓
Control mean	0.647	0.616	0.566	0.353	0.384	0.434
Observations	1085	1069	987	1085	1069	987

Notes: The table presents estimated Local Average Treatment Effects of any insurance purchase in the first three seasons, instrumented by the number of discount coupons received in the first three seasons on herd composition at three time periods: i) after the third sales season, ii) after the end of the experiment (sixth sales season), and iii) at the 10-year follow up. The dependent variable "herd composition" is measured as the number of animals of camels and cattle, and goats and sheep, respectively, that the household herds expressed in CMVE divided by the total number of animals that the household herds expressed in CMVE. Community fixed effects are included as randomization blocked at community level. Standard errors are clustered at the household-level, as this was the level of randomization. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. The row "Control mean" indicates the average outcomes for those who did not receive any coupons in the first three seasons. Data includes sub population of the 1179 households excluding households that are not herding the livestock. * denotes significance at 0.10; ** at 0.05; and *** at 0.01. In Kenya, 1 CMVE= 0.625 camel=1 cattle=10 goats/sheep, and in Ethiopia, 1 CMVE=0.4 camel=1 cattle=6.25 goats/sheep.

Table C14: Education outcomes —short-run and long-run

	Maximum years of education			Total years of education			Average years of education		
	3rd sales season	End of experiment	10-year long-run	3rd sales season	End of experiment	10-year long-run	3rd sales season	End of experiment	10-year long-run
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Any insurance purchased	-0.126	-0.283	2.906*	-0.632	0.168	7.314**	-0.014	0.144	2.520**
	(0.528)	(0.575)	(1.544)	(0.907)	(0.831)	(3.704)	(0.229)	(0.213)	(1.276)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	1.724	2.119	7.255	2.552	2.814	13.275	0.616	0.639	5.296
Observations	1041	1048	742	1041	1048	742	1041	1048	742

Notes: The table presents estimated Local Average Treatment Effects of any insurance purchase in the first three seasons, instrumented by the number of discount coupons received in the first three seasons on pre-specified education outcomes at three time periods: i) after the third sales season, ii) after the end of the experiment (sixth sales season), and iii) at the 10-year follow up. The dependent variables "Maximum years of education", "Total years of education", and "Average years of education" are measured among household members who were school-aged at any point during initial three periods of experiments, i.e., 15-29 years old in Kenya and 15-17 years old in Ethiopia (data in Ethiopia is limited to those up to 17 years old). Community fixed effects are included as randomization blocked at community level. Standard errors are clustered at the household-level, as this was the level of randomization. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. The row "Control mean" indicates the average outcomes for those who did not receive any coupons in the first three seasons. Data includes households which have male and female members for each panel, excluding households without household members who were school-aged during the experiment. * denotes significance at 0.10; ** at 0.05; and *** at 0.01. In Kenya, 1 CMVE= 0.625 camel=1 cattle=10 goats/sheep, and in Ethiopia, 1 CMVE=0.4 camel=1 cattle=6.25 goats/sheep. Please refer to Table 1 for the definition of outcome variables.

Table C15: Time use of children -— short-run and long-run

	Working full-time			Working part-time			Studying full-time		
	3rd sales season	End of experiment	10-year long-run	3rd sales season	End of experiment	10-year long-run	3rd sales season	End of experiment	10-year long-run
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Any insurance purchased	-0.074	-0.094	-0.363	0.153	0.112	-0.202	-0.128	0.017	0.423*
	(0.090)	(0.088)	(0.274)	(0.096)	(0.094)	(0.231)	(0.089)	(0.070)	(0.251)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	0.379	0.408	0.345	0.278	0.309	0.208	0.205	0.108	0.159
Observations	1040	1061	376	1040	1061	376	1040	1061	376

Notes: The table presents estimated Local Average Treatment Effects of any insurance purchase in the first three seasons, instrumented by the number of discount coupons received in the first three seasons on child time use outcomes at three time periods: i) after the third sales season, ii) after the end of the experiment (sixth sales season), and iii) at the 10-year follow up. The dependent variable is "children's time use" as the share of children aged 5-17 who study full-time, work part-time, and study full-time, respectively. Community fixed effects are included as randomization blocked at community level. Standard errors are clustered at the household-level, as this was the level of randomization. Columns 3, 6, and 9 report the estimated coefficients with 376 observations, which is due to the absence of this information in Kenyan sample at the endline while the other columns have both Kenya and Ethiopia samples. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. The row "Control mean" indicates mean outcomes for those who did not receive any coupons in the first three seasons. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Table C16: Payout effect on education outcomes

	Of household members who were school-aged during the experiment		
	Maximum years of education	Total years of education	Average years of education
	(1)	(2)	(3)
Predicted insurance purchase (γ_1)	3.324**	7.940*	3.203**
	(1.608)	(4.086)	(1.303)
Predicted insurance purchase $\times$ Indemnity dummy (γ_2)	-1.584	1.747	-7.758
	(6.388)	(11.46)	(5.652)
Indemnity dummy	0.292	-1.775	3.850
	(3.169)	(5.480)	(2.797)
Coef: $\gamma_1 + \gamma_2$	1.740	9.687	-4.555
p-val.: $\gamma_1 + \gamma_2$	0.778	0.356	0.405
Controls	✓	✓	✓
Control mean	7.255	13.275	5.296
Observations	742	742	742

Notes: The table presents estimated Local Average Treatment Effects of any insurance purchase in the first three seasons and the predicted receipt of indemnity payments, instrumented by the number of discount coupons received in the first three seasons and its interaction with the exogenous binary varaible that equals 1 if the average exogenous NDVI index value for the three initial seasons for which we instrument is below the index trigger value (<20% in Kenya and <15% in Ethiopia), on education outcomes. The dependent variables "Maximum years of education", "Total years of education", and "Average years of education" are measured among household members who were school-aged at any point during initial three periods of experiments, i.e., 15-29 years old in Kenya and 15-17 years old in Ethiopia (data in Ethiopia is limited to those up to 17 years old). Community fixed effects are included as randomization was stratified at community level. Standard errors are clustered at the household-level, as this was the level of randomization. The row labeled 'Coef' displays the effects of the payout, and the row labeled '*p-value*' shows its statistical significance. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. The row "Control mean" indicates the average outcomes for those who did not receive any coupons in the first three seasons. Data includes 742 of the 1179 households, excluding households without household members who were school-aged during the experiment. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Table C17: Payout effect on herd composition

	Outcome: N of animal type in CMVE / Total N of animals in CMVE			
	Camel	Cattle	Goats	Sheep
	(1)	(2)	(3)	(4)
Predicted insurance purchase (γ_1)	0.0485	0.126	-0.166*	0.00904
	(0.0987)	(0.0877)	(0.100)	(0.0637)
Predicted insurance purchase × Indemnity dummy (γ_2)	0.314	-0.0148	-0.442	0.0753
	(0.339)	(0.342)	(0.421)	(0.180)
Indemnity dummy	-0.139	-0.0150	0.248	-0.0572
	(0.166)	(0.162)	(0.209)	(0.0835)
Coef: $\gamma_1 + \gamma_2$	0.363	0.111	-0.608	0.084
p-val.: $\gamma_1 + \gamma_2$	0.261	0.736	0.136	0.617
Controls	✓	✓	✓	✓
Control mean	0.255	0.311	0.293	0.141
Observations	1014	1014	1014	1014

Notes: The table presents estimated Local Average Treatment Effects of any insurance purchase in the first three seasons and the predicted receipt of indemnity payments, instrumented by the number of discount coupons received in the first three seasons and its interaction with the exogenous binary varaible that equals 1 if the average exogenous NDVI index value for the three initial seasons for which we instrument is below the index trigger value (<20% in Kenya and <15% in Ethiopia) on herd composition outcomes. The dependent variable "herd composition" is measured as the number of animals of each animal type that the household herds expressed in CMVE divided by the total number of animals that the household herds expressed in CMVE. Community fixed effects are included as randomization blocked at community level. Standard errors are clustered at the household-level, as this was the level of randomization. The row labeled 'Coef' displays the effects of the payout, and the row labeled '*p-value*' shows its statistical significance. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. The row "Control mean" indicates mean outcomes for those who did not receive any insurance in the first three seasons. Data includes sub population of households excluding households that are not currently herding any livestock. * denotes significance at 0.10; ** at 0.05; and *** at 0.01. In Kenya, 1 CMVE= 0.625 camel=1 cattle=10 goats/sheep, and in Ethiopia, 1 CMVE=0.4 camel=1 cattle=6.25 goats/sheep.

D Pre-specified outcomes

Table D1: Prespecified primary outcomes: Herd size, earnings, education

	Herd size (CMVE)		Annual household cash earnings (USD)		Maximum years of education	
	(1)	(2)	(3)	(4)	(5)	(6)
Any insurance purchased	2.061	3.276	-6.587	17.411	2.944*	2.906*
	(8.662)	(8.839)	(207.341)	(208.250)	(1.536)	(1.544)
Controls		✓		✓		✓
Control mean	14.979	14.979	591.076	591.076	7.255	7.255
Observations	1179	1179	1179	1179	742	742

Notes: The Table presents estimated Local Average Treatment Effects of any insurance purchase in the first three seasons, instrumented by the number of discount coupons received in the first three seasons on pre-specified primary outcomes. The dependent variable “herd size” is measured as the number of livestock herded by the household in CMVE, “annual household cash earnings” is measured as self-reported seasonal cash income sources and amounts earned for the four seasons including sales of livestock, sales of livestock products, sales of crops, casual labor, employment and salary labor, trading expressed in USD, and "Maximum years of education" is measured among household members who were school-aged at any point during initial three periods of experiments, i.e., 15-29 years old in Kenya and 15-17 years old in Ethiopia (data in Ethiopia is limited to those up to 17 years old). Community fixed effects are included as randomization was stratified at community level. Standard errors are clustered at the household-level, as this was the level of randomization. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head’s years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. The row “Control Mean” indicates mean outcomes for those who did not receive any coupons in the first three seasons. Data includes 742 of the 1179 households, excluding households without household members who were school-aged during the experimen * denotes significance at 0.10; ** at 0.05; and *** at 0.01. In Kenya, 1 CMVE= 0.625 camel=1 cattle=10 goats/sheep, and in Ethiopia, 1 CMVE=0.4 camel=1 cattle=6.25 goats/sheep. Please refer to Table 1 for the definition of outcome variables.

Table D2: Prespecified primary outcomes: Herd composition

	Outcome: N of animal type in CMVE / Total N of animals in CMVE							
	Camel		Cattle		Goats		Sheep	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any insurance purchased	0.120	0.104	0.106	0.106	-0.220**	-0.211**	-0.007	0.005
	(0.089)	(0.088)	(0.082)	(0.081)	(0.095)	(0.094)	(0.051)	(0.050)
Controls		✓		✓		✓		✓
Control mean	0.255	0.255	0.311	0.311	0.293	0.293	0.141	0.141
Observations	987	987	987	987	987	987	987	987

Notes: The table presents estimated Local Average Treatment Effects of any insurance purchase in the first three seasons, instrumented by the number of discount coupons received in the first three seasons on pre-specified outcomes: herd composition. The dependent variable “herd composition” is measured as the number of animals of each animal type that the household herds expressed in CMVE divided by the total number of animals that the household herds expressed in CMVE. Community fixed effects are included as randomization was stratified at community level. Standard errors are clustered at the household-level, as this was the level of randomization. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head’s years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. The row "Control mean" indicates the average outcomes for those who did not receive any coupons in the first three seasons. Data includes 987 of the 1179 households excluding households that are not currently herding any livestock. * denotes significance at 0.10; ** at 0.05; and *** at 0.01. In Kenya, 1 CMVE= 0.625 camel=1 cattle=10 goats/sheep, and in Ethiopia, 1 CMVE=0.4 camel=1 cattle=6.25 goats/sheep.

Table D3: Prespecified secondary outcomes

	Herd management expenditure (USD)		Milk Income (USD)		Livestock loss (CMVE)		Distress sales (CMVE)		Livestock Sale (CMVE)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Any insurance purchased	2.590	-6.107	372.295	401.211	1.797	1.044	-0.328	-0.415	-1.135	-1.109
	(88.734)	(91.418)	(397.133)	(404.225)	(2.867)	(2.683)	(0.523)	(0.510)	(1.446)	(1.448)
Controls		✓		✓		✓		✓		✓
Control mean	207.775	207.775	455.696	455.696	5.503	5.503	0.381	0.381	2.595	2.595
Observations	1179	1179	1179	1179	1179	1179	781	781	1179	1179

Notes: The table presents estimated Local Average Treatment Effects of any insurance purchase in the first three seasons, instrumented by the number of discount coupons received in the first three seasons on pre-specified secondary outcomes. The dependent variable "herd management expenditure" is measured as the sum of the expenditure on water, fodder, supplementary feeding, and veterinary expenses over the past 12 months in USD, "milk income" is measured as the cash and in-kind income from milk expressed in USD, "livestock loss" is measured as the loss of livestock such as death expressed in CMVE, "distress sales" is measured as sales of livestock to cope with drought expressed in CMVE, and "livestock sale" is measured as sales for livestock expressed in CMVE. Community fixed effects are included as randomization was stratified at community level. Standard errors are clustered at the household-level, as this was the level of randomization. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. The row "Control Mean" indicates mean outcomes for those who did not purchase any insurance in the first three seasons. Data includes 781 of the 1179 households for columns 7 and 8 excluding households who did not sell any animals. * denotes significance at 0.10; ** at 0.05; and *** at 0.01. In Kenya, 1 CMVE= 0.625 camel=1 cattle=10 goats/sheep, and in Ethiopia, 1 CMVE=0.4 camel=1 cattle=6.25 goats/sheep.

Table D4: Prespecified secondary outcomes: IBLI purchase and children's activities

	IBLI uptake in the past 12 months (=1 if purchased)		IBLI uptake in the past 12 months (CMVE)		Working full-time		Working part-time		Studying full-time	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Any insurance purchased	0.033	0.036	-0.966	-0.936	-0.296	-0.363	-0.213	-0.202	0.437*	0.423*
	(0.043)	(0.044)	(0.889)	(0.907)	(0.270)	(0.274)	(0.240)	(0.231)	(0.265)	(0.251)
Controls		✓		✓		✓		✓		✓
Control mean	0.037	0.037	0.308	0.308	0.345	0.345	0.208	0.208	0.159	0.159
Observations	1179	1179	1179	1179	376	376	376	376	376	376

Notes: The table presents estimated Local Average Treatment Effects of any insurance purchase in the first three seasons, instrumented by the number of discount coupons received in the first three seasons on pre-specified secondary outcomes. The dependent variable "IBLI uptake" is measured in two ways: whether or not a household took up the insurance in the last 12 months before the endline survey, or the number of animals insured in the last 12 months in CMVE, and children's time use as the share of children aged 5-17 who worked full-time, worked and went to school (hence worked part-time), and studied full-time. Community fixed effects are included as randomization was stratified at community level. Standard errors are clustered at the household-level, as this was the level of randomization. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. The row "Control Mean" indicates mean outcomes for those who did not receive any coupons in the first three seasons. Data for columns 5 to 10 report the estimated coefficients with 376 observations, which is due to the absence of this information in Kenyan sample at the endline. * denotes significance at 0.10; ** at 0.05; and *** at 0.01. In Kenya, 1 CMVE= 0.625 camel=1 cattle=10 goats/sheep, and in Ethiopia, 1 CMVE=0.4 camel=1 cattle=6.25 goats/sheep.

E Comparing outcomes in TLU versus CMVE

In the analysis above, we used cattle market-value equivalent (CMVE) to aggregate the number of animals across animal species, instead of tropical livestock unit (TLU) that are typically used as a measure of the value of livestock assets. Since CMVE is a new aggregation unit to be us ed, we also construct variables in TLU i) to confirm that the values in CMVE is reasonable, and ii) to run the same estimations again with variables in TLU to check if the results are robust to changes in aggregation units.

Table E1 shows that our findings in the previous section regarding the herd sizes are robust to the changes in the unit of aggregation. The results are consistent with the results using CMVE measure in terms of sign, magnitude, and statistical significance, as expected. Note that the pattern for the composition for each country is also consistent. We confirm all the null results on TLU lost, TLU distress sales, TLU sold, and recent purchase of IBLI in the last 12 months window.

We also present results from quantile regression, examining the effects from the 10th to the 90th percentile in increments of every 10 percentiles. Table G3 reveals that the estimated coefficients are positive across all quantiles, and statistically significant at the 30th and 40th percentiles. This suggests that IBLI mechanically increases herd size at lower-middle quantiles. It is noteworthy that only 37% of the sample households maintained their original herd size quartile until the endline.

Table E1: Effects on livestock measured by TLU

		N of animal type in TLU / Total N of animals in TLU							
	Herd size	Camel	Cattle	Goat	Sheep	Livestock loss	Distress sales	Sold	IBLI purchase (in the last 12 months)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Any insurance purchased	2.434	0.091	0.122	-0.214**	0.004	0.352	-0.387	-1.276	-0.491
	(8.181)	(0.085)	(0.080)	(0.092)	(0.050)	(2.480)	(0.483)	(1.402)	(0.533)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	13.736	0.242	0.341	0.280	0.137	5.296	0.381	2.453	0.182
Observations	1179	987	987	987	987	1124	781	1131	1179

Notes: The table presents estimated Local Average Treatment Effects of any insurance purchase in the first three seasons, instrumented by the number of discount coupons received in the first three seasons on livestock related outcomes measured by TLU, instead of CMVE. The dependent variables are herd size, share of livestock, livestock loss, distress sales, livestock sold, and IBLI purchase in the last 12 months. “Herd composition” is measured as the number of animals of each animal type that the household herds expressed in TLU divided by the total number of animals that the household herds expressed in TLU. Community fixed effects are included as randomization was stratified at community level. Standard errors are clustered at the household-level, as this was the level of randomization. Data includes subpopulation of households of the 1179 households excluding households that are not having livestock outcomes. The row "Control Mean" indicates the average outcomes for those who did not receive any coupons in the first three seasons. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head’s years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. * denotes significance at 0.10; ** at 0.05; and *** at 0.01. Tropical Livestock Unit (TLU) is an integrated unit for aggregating cattle, camel, sheep, and goats by typical live body weight.1 TLU = 0.7 Camel = 1 Cattle = 10 Sheep/goats. In Kenya, 1 CMVE= 0.625 camel=1 cattle=10 goats/sheep, and in Ethiopia, 1 CMVE=0.4 camel=1 cattle=6.25 goats/sheep.

F Construction of income variables

Capturing income in this context is challenging due to the predominance of in-kind income sources. This section explains how we refined income variables in a reasonable manner. First, we aim to capture the overall income by considering all possible relevant sources, such as food-for-work employment programs. Second, we refine each income variable to include not only cash sales but also 'in-kind' income. For instance, we value the total produced milk (not just the amount sold) at the selling price. Finally, we try to reduce the measurement errors due to outliers.

We provide detailed definitions and describe our approach to imputing values, where necessary, to construct our income variables. The overarching strategy is to identify sources of in-kind income and calculate total income using reported amounts and available prices. To mitigate the impact of extreme values and reporting errors, we opt for the median price within the same location, type, or season/year, rather than relying on raw self-reported prices. For each livelihood activity, we compare reported earnings with total calculated in-kind income, typically expecting in-kind income to equal or exceed earnings. However, inaccuracies due to recall errors or typos may lead to discrepancies. In cases of inconsistency, we prioritize reported earnings over in-kind calculations due to their reliability (e.g., if reported total milk earnings are \$1,000 and calculated total in-kind is \$2,000, we use \$2,000 as the total milk income; if total earnings are \$1,000 and total in-kind is \$ 500, we use \$1,000).

To standardize data across the two countries, we normalize values using exchange rates, converting all amounts to USD. The conversion rates applied are KES/USD = 106.45 in 2020 and ETB/USD = 51.952 in 2022 for endline, and KES/USD = 77.35 in 2009 and ETB/USD = 17.70 in 2012 for baseline.

Here are the list of pre-specified income, annual total household cash earnings and annual milk income.

- "Annual total household cash earnings" (Pre-specified): defined as self-reported seasonal main income sources and amounts earned for the four seasons starting with the most recent dry and rainy seasons (e.g., sales of livestock, sales of livestock products, sales of crops, casual labor, employment and salary labor, trading, etc). We windsorize the earnings within round at the 99th percentile for analysis.
- "Annual milk income (earnings and in-kind)" (pre-specifed): defined as the income from milk (production including both sold and in-kind). The price is evaluated by the sold price using median within animal type, sublocation, season, and round. We windsorize size of

container and number of containers within round, animal type, and season at the 99th percentile for analysis. We replace income = earnings if income < earnings. This will be decomposed into in-kind and earnings.

The lists below is the total household income, and those used to construct that variable (mutually exclusive).

- "Annual total household income": defined as the aggregate sum of cash earnings and all other forms of in-kind income, including cash earnings, income from milk, crops, slaughter animals, animal births, and employment (such as food for work).
- "Annual animal birth income": is defined as in-kind income from animals born. The value is evaluated at the 20% of median sold price of adult animals within animal type and rounds. We widsornize number of animals birth within animal type and rounds at the 99th percentile for analysis.
- "Annual employment (food for work) income": is defined as the income from food for work employment program both in-kind and cash by the cash equivalent value. We use the median daily rate within sublocation and rounds. We windsornize number of days worked within round and sublocation at the 99th percentile for analysis.
- "Annual crop income": is defined as income from crop (harvest including both sold and in-kind). The price is evaluated at the median price within crop type and round. The quantity (kg) is windsroize by round. We replace income = earnings if income < earnings. We decompose it into in-kind and earnings to avoid double counting.[41]
- "Annual slaughter income incl. earnings": is defined as the in-kind income from slaughtered animals. The value is evaluated at the sold price of slaughtered animals if available, otherwise at the sold price.[42] If the price is missing, we use the median of sold price within sublocation, animal type, and season. We windsornize number of animals slaughtered by round at the 99th percentile for analysis.. We replace income = earnings if income < earnings.[43] We decompose it into in-kind and earnings to avoid double counting.

[41] We do not have information of in-kind in round 1.

[42] We restrict slaughter income for consumption and celebration, but due to data limitation we include all slaughter at R5 in Ethiopia.

[43] Due to data limitation, we bound it by earnings from slaughtered meat in Kenya, but by earnings from sale of livestock product in Ethiopia.

- " Annual earnings from the rest": is defined as annual total household cash earnings minus earnings from crop and slaughter. This is defined just to avoid double counting. This includes casual labor, employment and salary labor, trading, etc).

Appendix Table F1 and F2 provide summary statistics of income variables and their baseline, respectively. The annual total household income averages USD 1293.43 for Kenya, with a standard deviation (SD) of 1805.24, highlighting significant income variation. Ethiopia's mean total household income is lower at USD 770.89, with an SD of 904.29, suggesting a comparably diverse range of income. Focusing on cash earnings (pre-specified outcome), Kenyan households have an average of USD 515.08 with an SD of 671.37, while Ethiopian households show a mean of USD 564.31. Milk income (pre-specified outcomes) stands at a mean of USD 540.99 with an SD of 1,361.23 in Kenya. In contrast, Ethiopia reports a substantially lower mean of USD 85.18 and an SD of USD 246.72. The mean annual crop income is around UDSD 30-40. Slaughter income, indicative of revenue from the sale of livestock for meat, has roughly a mean of USD 70. The annual animal birth income averages USD 145. The baseline outcomes, capturing the initial state of these variables, mirror the current.

Table F1: Summary statistics of the income variables

	Kenya				Ethiopia				Pooled			
	Mean/SD	Min	Max	Obs	Mean/SD	Min	Max	Obs	Mean/SD	Min	Max	Obs
Pre-specified outcomes												
Annual total household cash earning (USD)	515.08	0.00	5636.45	781	564.31	0.00	3649.52	398	531.70	0.00	5636.45	1179
	[671.37]				[597.82]				[647.64]			
Annual milk income (USD) (earnings and in-kind)	540.99	0.00	21957.05	781	111.00	0.00	11895.60	398	395.84	0.00	21957.05	1179
	[1361.23]				[634.35]				[1184.86]			
Exclusive categories												
Annual total household income (USD)	1293.43	0.00	22689.29	781	763.23	0.00	9333.62	398	1114.45	0.00	22689.29	1179
	[1805.24]				[894.42]				[1578.09]			
Annual animal birth income (USD)	159.93	0.00	7589.79	781	96.06	0.00	5292.39	398	138.37	0.00	7589.79	1179
	[472.62]				[365.90]				[440.38]			
Annual employment (food for work) income (USD)	1.32	0.00	147.96	781	5.33	0.00	649.64	398	2.67	0.00	649.64	1179
	[8.36]				[43.47]				[26.21]			
Annual in-kind crop income (USD)	12.40	0.00	995.77	781	17.08	0.00	962.43	398	13.98	0.00	995.77	1179
	[68.85]				[90.95]				[77.01]			
Annual earnings from crop (USD)	15.49	0.00	1972.76	781	18.45	0.00	750.69	398	16.49	0.00	1972.76	1179
	[116.13]				[72.96]				[103.56]			
Annual in-kind milk income (USD)	137.60	0.00	18970.03	781	74.48	0.00	2125.04	398	116.29	0.00	18970.03	1179
	[1002.75]				[216.54]				[826.12]			
Annual sales from milk (USD)	403.39	0.00	4154.44	781	3.05	0.00	136.43	398	268.25	0.00	4154.44	1179
	[613.90]				[14.34]				[534.30]			
Annual in-kind slaughter income (USD)	63.71	0.00	2367.31	781	2.93	0.00	254.45	398	43.19	0.00	2367.31	1179
	[148.58]				[19.76]				[124.80]			
Annual earnings from slaughter (USD)	10.22	0.00	1127.29	781	54.56	0.00	1539.88	398	25.19	0.00	1539.88	1179
	[67.15]				[199.41]				[129.72]			
Annual earnings from the rest (USD)	489.38	0.00	5636.45	781	491.30	0.00	2221.28	398	490.02	0.00	5636.45	1179
	[664.12]				[500.31]				[613.51]			
Observations	781				398				1179			

Notes: The first two rows display our pre-specified income-related variables. The annual total household income represents the sum of all mutually exclusive categories for each component of income listed below. The currency is converted to USD using the exchange rates: KES/USD = 106.45 in 2020 and ETB/USD = 51.952 in 2022.

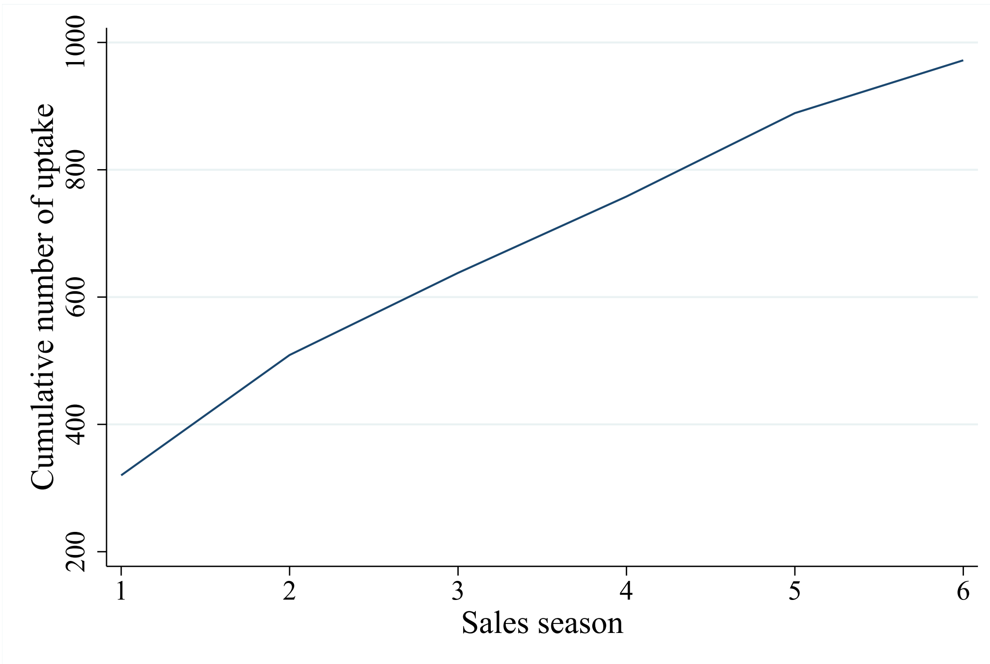
Table F2: Summary statistics of the baseline income variables

	Kenya				Ethiopia				Pooled			
	Mean/SD	Min	Max	Obs	Mean/SD	Min	Max	Obs	Mean/SD	Min	Max	Obs
Baseline pre-specified outcomes												
Baseline Annual household cash earnings (USD)	516.55	0.00	6877.83	781	462.92	0.00	5423.73	398	498.44	0.00	6877.83	1179
	[828.25]				[594.14]				[757.52]			
Baseline annual milk income (USD) (earnings and in-kind)	886.09	0.00	12192.44	781	161.81	0.00	2496.61	398	641.59	0.00	12192.44	1179
	[1668.25]				[265.31]				[1408.51]			
Baseline exclusive categories												
Baseline annual total household income (USD)	1570.40	0.00	16205.37	781	768.62	4.52	9820.90	398	1299.74	0.00	16205.37	1179
	[2038.94]				[829.83]				[1768.79]			
Baseline annual animal birth income (USD)	130.64	0.00	2053.01	781	58.98	0.00	1107.34	398	106.45	0.00	2053.01	1179
	[210.53]				[103.70]				[184.72]			
Baseline annual employment (food for work) income (USD)	5.24	0.00	1120.88	781	50.67	0.00	424.86	398	20.58	0.00	1120.88	1179
	[57.25]				[82.32]				[70.11]			
Baseline annual in-kind crop income (USD)	0.00	0.00	0.00	781	0.00	0.00	0.00	398	0.00	0.00	0.00	1179
	[0.00]				[0.00]				[0.00]			
Baseline annual earnings from crop (USD)	14.41	0.00	2262.44	781	14.28	0.00	406.78	398	14.36	0.00	2262.44	1179
	[138.19]				[48.33]				[115.90]			
Baseline annual in-kind milk income (USD)	862.22	0.00	12192.44	781	154.84	0.00	2496.61	398	623.43	0.00	12192.44	1179
	[1650.77]				[261.03]				[1392.59]			
Baseline annual sales from milk (USD)	23.87	0.00	437.17	781	4.78	0.00	146.61	398	17.43	0.00	437.17	1179
	[54.27]				[18.41]				[46.33]			
Baseline annual in-kind slaughter income (USD)	31.88	0.00	840.34	781	36.44	0.00	793.22	398	33.42	0.00	840.34	1179
	[56.82]				[95.45]				[72.20]			
Baseline annual earnings from slaughter (USD)	5.14	0.00	2262.44	781	5.34	0.00	216.50	398	5.21	0.00	2262.44	1179
	[82.39]				[22.84]				[68.34]			
Baseline annual earnings from the rest (USD)	497.00	0.00	6877.83	781	443.31	0.00	5423.73	398	478.88	0.00	6877.83	1179
	[814.35]				[594.36]				[747.54]			
Observations	781				398				1179			

Notes: The first two rows display our pre-specified income-related variables. The annual total household income represents the sum of all mutually exclusive categories for each component of income listed below. The currency is converted to USD using the exchange rates: KES/USD = 77.35 in 2009 and ETB/USD = 17.70 in 2012.

G Additional Tables and Figures Referenced in Text

Figure G1: Cumulative number of IBLI uptake



Notes: The figure shows the cumulative number of IBLI uptakes over the sales seasons during IBLI pilot periods among the sample households.

Table G1: The average market values of animals

	(1)	(2)	(3)	(4)	(5)	(6)
	Marsabit, Kenya			**Borana, Ethiopia**		
	KES	Cattle Equivalent	Data Rounds	Birr	Cattle Equivalent	Data Rounds
Camel	25,132	1.6	1-7	7,447	2.5	1-4
Cattle	15,617	1.0	1-7	3,023	1.0	1-4
Sheep	1,515	0.1	7			
Goats	1,561	0.1	7			
Sheep or Goat	2,308	0.15	1-6	484	0.16	1-4

Note: The table presents the market value of each species across during our study periods. Columns 1 and 4 show the value of each species in local currencies (KES for Kenya, Birr for Ethiopia). Columns 2 and 5 show values relative to the cattle equivalent (with cattle value set to 1 in each country). Columns 3 and 6 indicate the rounds of data collection during which these animals' market values were recorded.

Table G2: Effects on herd composition using all six seasons as instruments

	Outcome: N of animal type in CMVE / Total N of animals in CMVE					
	Camel	Cattle	Goats	Sheep	Camels & cattle	Goats & sheep
	(1)	(2)	(3)	(4)	(5)	(6)
Any insurance purchased (in six sales seasons)	0.149	0.101	-0.271**	0.020	0.253*	-0.253*
	(0.106)	(0.097)	(0.111)	(0.058)	(0.130)	(0.130)
Controls	✓	✓	✓	✓	✓	✓
Control mean	0.281	0.292	0.299	0.128	0.573	0.427
Observations	987	987	987	987	987	987

Notes: The table presents estimated Local Average Treatment Effects of any insurance purchase in the six seasons, instrumented by the number of discount coupons received in the six seasons on herd composition. The dependent variable “herd composition” is measured as the number of animals of each animal type that the household herds expressed in CMVE divided by the total number of animals that the household herds expressed in CMVE. Community fixed effects are included as randomization was stratified at community level. Standard errors are clustered at the household-level, as this was the level of randomization. Data includes 987 of the 1179 households excluding households that are not currently herding any livestock. The row "Control Mean" indicates the average outcomes for those who did not receive any coupons in the six seasons. Control variables include a dummy for male head, age of household head, share of male children, a dummy indicating whether the household head ever went to school, a dummy for whether the household is fully settled, and household size in adult equivalents. * denotes significance at 0.10; ** at 0.05; and *** at 0.01. In Kenya, 1 CMVE= 0.625 camel=1 cattle=10 goats/sheep, and in Ethiopia, 1 CMVE=0.4 camel=1 cattle=6.25 goats/sheep.

Table G3: Effects on herd size at different quantile in endline

	10th %-tile	20th %-tile	30th %-tile	40th %-tile	50th %-tile	60th %-tile	70th %-tile	80th %-tile	90th %-tile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Any insurance purchased	1.274	2.014	2.840*	3.623*	5.228	5.074	7.278	7.409	5.680
	(1.279)	(1.494)	(1.659)	(1.974)	(4.454)	(4.320)	(12.114)	(9.843)	(14.730)

Notes: The table presents estimated IV quantile regression of any insurance purchase in the first three seasons, instrumented by the number of discount coupons received in the first three seasons on total livestock size measured by CMVE. Community fixed effects are included as randomization was stratified at community level. Standard errors are clustered at the household-level, as this was the level of randomization. Data includes households of the 1179 households excluding households that are not having livestock outcomes. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. * denotes significance at 0.10; ** at 0.05; and *** at 0.01. In Kenya, 1 CMVE= 0.625 camel=1 cattle=10 goats/sheep, and in Ethiopia, 1 CMVE=0.4 camel=1 cattle=6.25 goats/sheep.

Table G4: Effects on education outcomes using all six seasons as instruments

	Of households members who were school-aged during the experiment			Share of children in the household		
	Maximum years of education	Total years of education	Average years of education	Working full-time	Working part-time	Studying full-time
	(1)	(2)	(3)	(4)	(5)	(6)
Any insurance purchased (in six sales seasons)	3.018	8.209*	2.541	-0.452	-0.255	0.577
	(1.864)	(4.420)	(1.558)	(0.444)	(0.401)	(0.451)
Controls	✓	✓	✓	✓	✓	✓
Control mean	5.889	8.333	4.833	0.575	0.000	0.000
Observations	742	742	742	376	376	376

Notes: This table presents the estimated Local Average Treatment Effect of any insurance purchase in the six seasons, instrumented by the number of discount coupons received in the six seasons, on education outcomes. The dependent variables "Maximum years of education", "Total years of education", and "Average years of education" (Column 1-3) are measured among household members who were school-aged at any point during initial three periods of experiments, i.e., 15-29 years old in Kenya and 15-17 years old in Ethiopia (data in Ethiopia is limited to those up to 17 years old). The dependent variables "Working full-time", "Working part-time", and "Studying full-time" (Column 4-6) are measured among the share of children aged 5-17 in Ethiopia (The outcome data for Kenya is not available at endline) in the household. Data includes 742 of the 1179 households for Columns 1-3, excluding households without school-aged children meeting the criteria. Columns 4-6 show the effects on child time-use as a share of children in the household. The sample consists of children aged 5-17 in Ethiopia (The outcome data for Kenya is not available at endline). Community fixed effects are included as randomization was stratified at community level. Standard errors are clustered at the household-level, as this was the level of randomization. The row "Control Mean" indicates the average outcomes for those who did not receive any coupons in the six seasons. Our control variables are the pre-specified balance variables presented in Table 1 and are: age of household head in years, whether the household is a male headed household, the household head's years of education, adult equivalent, dependency ratio, herd size in CMVE, annual income per adult equivalent in USD, whether the household owns or farms agricultural land, and is fully settled. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.