Drought-Proof Generosity: Index Insurance and Informal Livestock Lending in Northern Kenya*

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Abstract

I investigate whether formal insurance contracts crowd out informal risk-sharing arrangements that underpin livelihoods in developing economies. Using panel data from pastoralist households in Marsabit, northern Kenya, I instrument insurance uptake with randomly distributed price discount coupons and interact coverage with sub-location drought severity. Drought erodes informal support for uninsured households. A one-standard-deviation worsening of drought lowers the probability that an uninsured household receives a livestock gift by 11 percentage points and a cash or in-kind transfer by 12 points, and lowers the probability that it gives a transfer by 11 points. Livestock borrowing does not fall. Two-stage least squares estimates suggest that the same worsening of drought raises livestock borrowing by roughly 40 percentage points more for insured than uninsured households. In the reduced form, coupon recipients also give and receive more transfers as drought deepens. I find no evidence that insurance crowds out informal risk sharing, and suggestive evidence that it helps keep informal networks active during a drought.

Keywords: index insurance, informal risk sharing, crowding-in, drought, pastoralists, Kenya

JEL Codes: G22, O16, Q12, Q54, D71

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1 Introduction

Where financial markets are thin, households insure one another through gifts, loans, and reciprocal transfers, from village India to rural sub-Saharan Africa (Townsend, 1994; Coate and Ravallion, 1993). When formal insurance arrives, it can either crowd out these arrangements or crowd them in. It may crowd them out by reducing the need for reciprocal transfers and weakening the ties that sustain them (Attanasio et al., 2012). It may crowd them in by making insured households more reliable counterparties, better able to repay and reciprocate, which deepens trust and participation in informal networks (Dubois et al., 2008; Mobarak and Rosenzweig, 2012). The two forces need not act symmetrically on both sides of an informal loan. Insurance could change how much a household *borrows* from its network when it needs help, how much it *lends* to others in need, or both, with different implications for who ultimately bears risk within the network.

Pastoralist households in the arid and semi-arid lands of East Africa offer a useful test of these predictions. They face severe and recurrent drought with little access to formal finance. Without insurance, they rely on a portfolio of informal coping mechanisms: selling livestock at distressed prices, reducing food consumption, and borrowing and lending livestock through kinship networks (Fafchamps et al., 1998). Livestock is both the asset that insurance protects and the medium of informal exchange, so formal and informal insurance act on the same stock of wealth. Yet there is little evidence on how formal insurance changes these networks during an actual drought.

In this paper, I estimate the effect of index-based livestock insurance (IBLI) on informal livestock lending during drought in Marsabit, northern Kenya, where pastoralist households have been offered a satellite-based index insurance contract since 2010. IBLI compensates policyholders when predicted average livestock mortality, derived from satellite-measured vegetation conditions, exceeds a threshold (Chantarat et al., 2013). A randomised encouragement design, in which surveyed households received premium discount coupons at random, provides exogenous variation in insurance uptake that I use to instrument the decision to insure. I combine the four coupon rounds of the IBLI household survey (2010 to 2013) with sub-location variation in drought severity. This lets me estimate how the effect of insurance changes with contemporaneous drought within a household fixed-effects framework.

I first document how informal networks behave during drought in the absence of insurance. For uninsured households, a one-standard-deviation worsening of drought lowers the probability of receiving a livestock gift by 11 percentage points, of receiving a cash or in-kind transfer by 12 points, and of giving a transfer by 11 points. Livestock borrowing and lending do not change. Gift and transfer networks shrink during the crisis, while the livestock loan network does not.

Insured households respond differently. The same worsening of drought raises the probability of borrowing livestock by roughly 40 percentage points more for insured than for uninsured households. The estimate is large relative to the 13.5% mean borrowing rate because it is a local effect for the households whose purchase the coupon changed, measured over a large change in drought conditions. At average drought conditions, insurance has no detectable effect on borrowing. The estimate is significant at the 10% level with conventional clustered standard errors ($p = 0.09$) and at the 5% level under the wild restricted efficient bootstrap ($p = 0.02$). The reduced form, which relies only on random coupon assignment, is significant at the 5% level under every correction for the small number of clusters that I apply. The evidence is therefore suggestive. In the reduced form, coupon recipients also give and receive more cash and in-kind transfers as drought deepens, although the corresponding two-stage least squares estimates are not significant.

Insurance has no significant effect on livestock gifts received. Gifts serve as a placebo for the insurance effect, distinct from the drought effect on gifts reported above: if the coupon proxied for unobserved social

connectedness, gift receipt would respond to insurance as well. The estimate for drought-related herd migration is too imprecise to distinguish from zero, and insurance shows no effect on livestock lent by sampled households. Most borrowing partners are kin living in the same sub-location but outside the sample, so the lending that accommodates any additional borrowing falls largely outside what the survey observes. The borrowing result holds in sign under alternative drought measures but does not appear when coverage is lagged by one round, consistent with an effect that depends on holding a policy during the shock, although neither estimate is precise. An instrumental forest (Athey et al., 2019) detects no systematic heterogeneity in the effect, and estimates by baseline wealth stratum are too imprecise to distinguish from one another.

This paper primarily contributes to two strands of literature. The first studies how formal insurance interacts with informal risk sharing. Theory allows either sign. Formal or public insurance can crowd out informal transfers by weakening the incentives that sustain them (Arnott and Stiglitz, 1991; Attanasio and Ríos-Rull, 2000), or crowd them in by easing the commitment problem (Dubois et al., 2008), and index insurance can complement informal arrangements that absorb the idiosyncratic losses the index misses (Mobarak and Rosenzweig, 2013). The evidence is mixed. Ghana's national health insurance scheme crowded out informal transfers between households (Strupat and Klohn, 2018), formal insurance reduces transfers in the laboratory (Lin et al., 2014), and in a framed field experiment, mutual support within credit groups leads members to free-ride (Janssens and Kramer, 2016). Other field experiments examine how informal groups shape the demand for index insurance (Dercon et al., 2014). The closest evidence comes from southern Ethiopia, where Borana pastoralists lend cattle through clan networks under *dabare*, an arrangement much like the livestock loans studied here. Takahashi et al. (2019) elicit hypothetical *dabare* decisions toward randomly matched households and find that a household's own IBLI purchase does not change its willingness to lend, while a match's purchase weakly raises willingness to lend to that match. Matsuda et al. (2019) find that insured households in the same region receive more informal transfers after IBLI payouts. I add evidence on realised livestock loans and transfers over four sales seasons, with randomised variation in coverage and sub-location variation in drought. The two approaches are complementary: hypothetical questions capture willingness to lend, including in states of the world a survey may not observe, but not behaviour. Realised loans capture behaviour, but only on the side of each loan that falls inside the sample. The response I find is specific to in-kind livestock loans. The same specification shows no comparable response in informal cash borrowing (coefficient -0.04, $p = 0.83$), which fits insurance easing a repayment-risk constraint on reciprocal livestock exchange better than a general relaxation of household liquidity, although the cash-loan estimate is imprecise and I do not observe lenders' beliefs directly.

The second strand evaluates index insurance by its effects on how households cope with and recover from shocks. In northern Kenya, IBLI coverage reduced households' reliance on asset sales and meal reduction after the 2011 drought (Janzen and Carter, 2019), and it is associated with fewer distress livestock sales, more productive investment, and higher income (Jensen et al., 2017). Payouts may also help households below the poverty-trap threshold to recover (Noritomo and Takahashi, 2020), and in Mongolia, IBLI payouts sped the rebuilding of herds after a catastrophic winter (Bertram-Huemmer and Kraehnert, 2018). These studies focus on the household's own assets and consumption. I examine a margin they do not measure, borrowing livestock from kin, through which insurance could support recovery from a covariate shock. I also show that among uninsured households, drought shrinks the gift and transfer networks that would otherwise provide support.

The rest of the paper proceeds as follows. Section 2 describes the research setting and the IBLI insurance product. Section 3 presents the data and key variables. Section 4 outlines the empirical strategy. Section 5

presents the main results and robustness checks, and Section 6 discusses mechanisms. Section 7 concludes.

2 Setting

2.1 Pastoralism in Northern Kenya

Northern Kenya's arid and semi-arid lands receive low and variable rainfall, financial infrastructure is limited, and mobile pastoralism is the main livelihood. Marsabit County, formerly Marsabit District, comprises four sub-counties (North Horr, Saku, Moyale, and Laisamis), which divide into 20 electoral wards and, below those, into sub-locations, the level at which the survey sample is drawn (Kirui et al., 2022). The county covers roughly 70,000 km^2, an area similar in size to the Republic of Ireland, at an overall population density of 6 to 7 people per km^2. Size and density vary widely across sub-counties. Saku, which contains Marsabit town, covers 2,078 km^2 at roughly 22 people per km^2, while Laisamis covers 20,266 km^2 at roughly 3 people per km^2. A single sub-location can therefore cover an area far larger than a typical rural administrative unit elsewhere in Kenya, with a correspondingly small population. Annual rainfall ranges from 200 to 1,000 mm and averages 254 mm, one of the lowest averages among Kenya's counties (Kirui et al., 2022).

Livestock, including camels, cattle, goats, and sheep, are both the main productive asset and the main store of wealth for pastoralist households. Households manage seasonal variation in forage and water through herd migration, a strategy that requires a minimum herd size (Toth, 2015). During severe drought, livestock weaken, productivity falls, and mortality rises. Households that fall below the minimum herd threshold face nonlinear asset dynamics that slow recovery, consistent with poverty trap dynamics (Lybbert et al., 2004; Barrett et al., 2006; Santos and Barrett, 2011).

Informal livestock transfers are central to household risk management in this setting. Households routinely borrow and lend livestock through social and kinship networks to rebalance herds across species and locations, gain access to milk and other livestock products, and restock after losses. Unlike financial credit, these arrangements typically carry no fixed repayment date. The survey distinguishes short-term from long-term loans. A short-term loan resembles a milking loan, in which the household returns the animal once it stops lactating. A long-term loan can keep an animal, or its offspring, with the borrower for years, to be returned or reciprocated once the lender needs it or the borrower's herd recovers. Long-term arrangements account for 40 of the 49 loan events that record loan type (82%), consistent with the reciprocal, open-ended character of kinship-based livestock exchange documented elsewhere in pastoral East Africa (McPeak, 2004). Because loan type is recorded in only one round, I treat this share as indicative of typical loan duration. During drought, borrowing serves as a coping mechanism distinct from outright purchase or distress sales (McPeak, 2004; Fafchamps et al., 1998).

Each loan event records how many animals change hands. Borrowing households receive a median of 2 animals per event (mean 2.3, maximum 26). Shoats, meaning goats and sheep, make up 60% of borrowed animals, camels 17%, and cattle 11%. The survey records where each loan comes from and, in Round 6, the relationship between the two parties, but it does not identify the lending household. I therefore cannot link a loan to a named counterparty or test reciprocity between identified households directly. In Rounds 1 to 5, 89% of the livestock loans with a recorded origin come from within the borrower's own location or sub-location. In Round 6, family members account for 69% of lenders and clan members for 22%. The lending side looks the same. Of the 34 recorded livestock loans that sampled households made to others,

65% went to family members and 18% to clan members.[1] Because the survey samples between 30 and 100 households per sub-location, most of these nearby kin are not in the sample.

2.2 The IBLI Insurance Product

Insurers launched the Index-Based Livestock Insurance (IBLI) pilot in Marsabit District in January 2010. Unlike conventional indemnity insurance, IBLI compensates policyholders based on a predicted average livestock mortality index built from satellite-measured normalised difference vegetation index (NDVI) readings, which track vegetative cover as a proxy for rangeland conditions (Chantarat et al., 2013). The index is computed for index areas that group several sub-locations, so every insured household in an index area receives the same payout rate. The 16 survey sub-locations fall into four index areas (Central/Gadamoji, Laisamis, Loiyangalani, and Maikona) of four sub-locations each. Payouts trigger once predicted mortality exceeds a 15% deductible. Because compensation depends on an exogenous index rather than on individual losses, IBLI avoids the moral hazard and adverse selection problems common in traditional livestock insurance (Jensen and Barrett, 2017; Barnett et al., 2008).

Households choose how many tropical livestock units (TLUs) to insure, and the premium rate varies by region to reflect local risk. Across the 135 Marsabit unit-season records in the program's sales data (2010 to 2019), the premium rate averages 8.6% of the insured value (interquartile range 7.9 to 9.2%). Uptake fell after the first sales seasons. Total livestock insured in Marsabit fell from 5,965 TLU in 2010 to 413 TLU in 2012, and coverage among surveyed households fell from 27.6% in Round 2 to 9.4% in Round 5. Insured households receive payouts at the end of each dry season, in October or March, if the index triggers. Equity Bank and UAP/APA Insurance sold the product initially, and it has since scaled across Kenya's drylands and into other countries.

The IBLI research design added a randomised encouragement to boost uptake for evaluation. In each sales period, researchers randomly selected surveyed households to receive discount coupons on the premium for the first 15 TLU insured. Discounts range from 10 to 80% in steps of 10 percentage points, and the share of households receiving a coupon ranges from 57% in Round 2 to 91% in Round 5. This random assignment provides exogenous variation in insurance uptake.

3 Data

3.1 Household Panel

The analysis uses the IBLI Marsabit Household Survey, a panel collected by the International Livestock Research Institute (ILRI) and partner universities. The survey follows a proportionate stratified sample based on the 2009 census, drawing between 30 and 100 households from each of 16 sub-locations. Within each sub-location, one third of the sample comes from each of three herd-size strata: low (under 10 TLU), medium (10 to 20 TLU), and high (over 20 TLU). The initial panel contains 924 households. Households that drop out are replaced with households from the same stratum and sub-location, which keeps the panel at 924 households through Round 4, 923 in Round 5, and 919 in Round 6 (Kirui et al., 2022).

Rounds 1 through 6 (2009 to 2015) contain 5,530 household-round observations from 1,064 households. I exclude Round 7 (2020) because its questionnaire omits the item used to identify an active IBLI contract.

[1] Appendix Table 18 reports the full distribution of livestock loan counterparties in both directions, together with the sources of cash and in-kind transfers.

The omission reflects a change in the questionnaire after a five-year gap since Round 6, not the end of the program, which had scaled commercially across Kenya's drylands by 2020.

Round 1 predates the first IBLI sales and serves only as a pre-treatment check on the instrument. The coupon program ran during Rounds 2 through 5, so the primary estimation sample covers these four rounds, with 3,686 household-round observations from 985 households.[2] Household fixed-effects specifications drop households observed in only one of these rounds, leaving 3,652 observations in most tables. A separate specification uses Rounds 3 through 6 to lag treatment by one round.

The four main outcomes come from the survey modules on livestock intake, offtake, and herd migration, and Section 4 defines them. Livestock borrowing and lending come from separate modules, one on animals the household took in and one on animals it sent out. Because each respondent reports only its own household's side of a transaction, the two rates need not match even in a fully reciprocal network. The lending rate is also lower because most borrowers' counterparties are not in the sample (Section 2), so the matching lending is not observed.

Livestock loans are not the only informal arrangement the survey records. It also records general cash and in-kind transfers, cash loans, and credit for goods, which I use to test whether insurance affects informal support more broadly or only through livestock loans. General transfers, recorded in every round, divide into transfers received and transfers given. Like livestock loans, they flow mainly through kin and neighbours. Relatives outside the household account for 70% of the transfers households receive and 55% of those they give, and neighbours and friends account for nearly all of the rest (Appendix Table 18). In the estimation sample, a household receives a general transfer in 55.8% of household-round observations and gives one in 43.4%. Informal cash loans, recorded in Rounds 2 through 5, have a mean of 6.3%, and formal cash loans from the same module a mean of 2.5%. Informal credit for goods is recorded in Rounds 4 through 6 and has a mean of 49.2% in the two rounds (4 and 5) that overlap the estimation sample. I combine these into two broad indicators. Any informal support received equals the maximum of livestock borrowing, informal cash loans, and informal goods credit, wherever each is observed (mean 37.2%). Any informal support given equals the maximum of livestock lending and general transfers given (mean 46.2%).

The treatment variable comes from the IBLI contracts module. Coverage is 17.6% in the estimation sample and 12.0% in the full six-round panel, which includes Round 1, when no household yet held a policy.

The instrument comes from the discount coupon records. In the estimation sample, 72.2% of households receive a discount. Averaged over all households, including those who receive none, the discount equals 29.1% of the premium (SD 24.3); among recipients it averages 40.3% (SD 19.1). The main specification uses a binary indicator for receiving any discount, which does not require a functional form for how discount size maps to uptake. The continuous discount serves as a robustness check.

3.2 Drought Measure

I measure drought severity with the Standardised Precipitation-Evapotranspiration Index at a 3-month accumulation (SPEI-3), built from the TerraClimate archive and matched to each sub-location. For each survey round, I take the minimum SPEI-3 value during that round's recall period, which captures the worst drought stress the household faced. This measure averages -1.073 (SD 0.560) in the estimation sample and is centred at its mean for estimation. Because SPEI varies across sub-locations within an index area, it is

[2]The Rounds 2 to 5 sample contains 3,687 household-round observations with a coupon record, the count reported in Appendix Table 22. One of these lacks a control variable, which leaves 3,686. Dropping the 34 households observed in only one of these rounds then leaves 3,652.

not the variable that triggers IBLI payouts.

Appendix Table 22 reports summary statistics for every variable described above by wealth stratum, using the bins of the sampling design. Livestock borrowing and lending rise with wealth, and gift receipt rises from the low to the medium stratum, consistent with wealthier households holding larger and more active positions in these exchange networks. IBLI coverage and coupon receipt are nearly constant across strata, consistent with the random assignment of coupons.

4 Empirical Strategy

Households that choose to insure may differ along unobserved dimensions, including risk preferences, social connectedness, and financial literacy, that also affect their engagement with informal lending networks. An OLS regression of livestock borrowing on insurance status would therefore conflate the causal effect of insurance with selection.

I address this endogeneity by instrumenting IBLI uptake with the randomly distributed discount coupons, following the encouragement designs of Karlan et al. (2014) and Janzen and Carter (2019). The identifying assumption is that coupon receipt affects livestock borrowing only through its effect on the decision to purchase insurance.

The second stage is

$$y_{it}^{S} = \beta_1 \widehat{\text{IBLI}}_{it} + \beta_2(\text{SPEI}_{jt} \times \widehat{\text{IBLI}}_{it}) + \beta_3 \text{SPEI}_{jt} + \mathbf{X}_{it}'\boldsymbol{\phi} + \alpha_i + \gamma_t + \varepsilon_{it} \tag{1}$$

The second stage has two endogenous regressors, IBLI_{it} and $\text{IBLI}_{it} \times \text{SPEI}_{jt}$, which I instrument with coupon receipt, Z_{it}, and its interaction with drought, $Z_{it} \times \text{SPEI}_{jt}$. Because SPEI_{jt} enters the second stage directly, it also appears in each first stage, alongside the same controls and fixed effects. The first stage for the level of coverage is

$$\text{IBLI}_{it} = \delta_0 \text{SPEI}_{jt} + \delta_1 Z_{it} + \delta_2(\text{SPEI}_{jt} \times Z_{it}) + \mathbf{X}_{it}'\boldsymbol{\theta} + \alpha_i + \gamma_t + v_{it} \tag{2}$$

and the first stage for the interaction term takes the same form, with $\text{IBLI}_{it} \times \text{SPEI}_{jt}$ as the dependent variable and its own coefficients on SPEI_{jt}, Z_{it}, and $Z_{it} \times \text{SPEI}_{jt}$.

IBLI_{it} indicates whether household i held an active IBLI contract in round t, and Z_{it} indicates whether it received a discount coupon for the insurance sold in round t. SPEI_{jt} is the centred minimum recall-period SPEI-3 value for sub-location j in round t, where lower values mark worse drought. $\widehat{\text{IBLI}}_{it}$ and $\widehat{\text{IBLI}}_{it} \times \text{SPEI}_{jt}$ are the first-stage predicted values. I estimate the model separately for four binary outcomes y_{it}^{S}: whether the household borrowed livestock, lent livestock, received a livestock gift, or moved its herd to cope with drought. Livestock gifts serve as a placebo outcome, since gifts follow different social norms from reciprocal loans and should not respond systematically to insurance coverage. $\mathbf{X}_{it}$ contains three time-varying controls: log livestock wealth in TLU (Appendix B.1), household size, and settlement status (nomadic, partially settled, or fully settled). α_i and γ_t are household and round fixed effects. The parameter of interest is β_2, which captures how insurance changes the household's use of the loan network as drought worsens. Because SPEI is centred and IBLI is binary, β_3 measures how the outcome changes with drought for uninsured households, which I use to describe how informal networks respond to drought in the absence of insurance.

Standard errors are clustered by sub-location ($G = 16$), the level at which drought varies and within which coupons were randomised. With so few clusters, CV1 inference can over-reject, particularly when

cluster sizes are unbalanced (MacKinnon et al., 2023). In the estimation sample, cluster sizes range from 119 to 414 observations, a max-to-median ratio of 2.27 (Appendix Table 23). Section 5.6 therefore reports the reduced form with the cluster-jackknife (CV3) variance estimator and the 2SLS estimates with the wild restricted efficient bootstrap, and Appendix B.3 reports further diagnostics for the small number of clusters.

5 Results

5.1 OLS

The OLS regression replaces $\widehat{\text{IBLI}}_{it}$ and $\widehat{\text{IBLI}}_{it} \times \text{SPEI}_{jt}$ in Equation 1 with actual coverage and its interaction with drought. Because it does not require the instrument, it uses all six rounds, 5,445 household-round observations rather than the 3,652 in Rounds 2 to 5.

Table 1 reports the results. Every coefficient is small and far from significant. The IBLI by SPEI interaction ranges from −0.009 to 0.015 across the four outcomes and equals −0.006 for livestock borrowing, two orders of magnitude below the 2SLS estimate in Table 3. The IBLI level term never exceeds 0.021 in absolute value.

Table 1: OLS Estimates of IBLI on Coping Strategies

	Loan In	Loan Out	Gift In (Placebo)	Drought Herd
IBLI	−0.004	0.003	−0.020	−0.021
	(0.022)	(0.014)	(0.023)	(0.028)
IBLI × SPEI	−0.006	0.001	−0.009	0.015
	(0.035)	(0.021)	(0.066)	(0.049)
Household and round FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	5,445	5,445	5,445	5,445

Notes. OLS regression of each outcome on IBLI coverage, its interaction with centred SPEI, log TLU, household size, and settlement status, with household and round fixed effects. Standard errors clustered by sub-location ($G = 16$) in parentheses. The sample is larger than in Table 3 because this specification does not require the discount instrument to be observed. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Two features of the setting explain why OLS and 2SLS can differ. First, coverage is a choice, and the unobserved traits that lead a household to buy insurance without a coupon probably also shape its position in the informal loan network. A household with strong kinship ties already has a form of insurance. It may feel less need to buy IBLI or, if it does buy, have no reason to lean harder on the network during drought. A household with weak ties may buy IBLI precisely because it lacks that cushion, and still lack it during drought regardless of coverage. Both patterns push the OLS coefficient toward zero. Second, the two estimators target different quantities. OLS averages over everyone who ends up insured, including households that would have bought IBLI without a coupon, while 2SLS identifies the effect for compliers, the households whose purchase decision the coupon changed. If insurance matters most for households that would not otherwise buy it, the two estimates can differ sharply.

5.2 First Stage

Table 2 reports the first stage. Receiving any discount coupon raises the probability of IBLI coverage by 14.6 percentage points (SE 0.015, $p < 0.001$), more than four fifths of the 17.6% coverage rate in the estimation sample. Each additional percentage point of discount raises the probability of coverage by 0.24 percentage points, so the average discount among recipients (40.3%) implies a 9.5 percentage point increase. The F-statistics of 95.8 and 39.1 exceed conventional weak-instrument thresholds by a wide margin. In the full 2SLS model, which instruments both IBLI and its interaction with drought, the first-stage F-statistics are 46.8 for IBLI and 32.3 for the interaction.

Table 2: First Stage, Coupon Receipt and IBLI Coverage

	(1) Binary coupon	(2) Discount value
Any discount coupon	0.146^{***}	
	(0.015)	
Discount value (%)		0.0024^{***}
		(0.0004)
Household FE	Yes	Yes
Round FE	Yes	Yes
First-stage F	95.8	39.1
Observations	3,661	3,661

Notes. The dependent variable equals one if the household holds an active IBLI contract. Standard errors clustered by sub-location ($G = 16$) appear in parentheses. The sample covers Rounds 2 to 5, when the survey records coupon assignment. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Coupon receipt also does not predict borrowing before IBLI existed. Regressing Round 1 (2009) livestock borrowing on the largest discount a household later receives yields a coefficient of 0.0003 per percentage point of discount (SE 0.0006, $p = 0.62$, $N = 889$), so coupon assignment is unrelated to borrowing behaviour that predates the product.

5.3 Insurance and Livestock Borrowing during Drought

Table 3 reports the main estimates for the four outcomes. Panel A reports the reduced form and Panel B the 2SLS estimates. Because a lower SPEI value marks worse drought, a negative interaction coefficient means that insured households use a coping strategy more as drought worsens.

Insurance is associated with more livestock borrowing as drought worsens, though the evidence is suggestive. In the reduced form, households assigned a coupon borrow more as SPEI falls (coefficient -0.073, $p = 0.020$), an estimate that relies only on random assignment and requires no assumption about who takes up insurance. The 2SLS estimate scales this effect by the first stage. The interaction equals -0.726 ($p = 0.090$), significant at the 10% level but not at the 5% level. A one standard deviation fall in SPEI (0.56) implies an increase in the probability of borrowing roughly 40 percentage points larger for insured than for uninsured households. Section 5.6 shows that the reduced form remains significant at the 5% level under the cluster jackknife, and that the 2SLS estimate is significant at the 5% level under a bootstrap designed for instrumental variables with few clusters.

This magnitude exceeds the 13.5% mean borrowing rate and is large for two reasons. It is a local average treatment effect for compliers, the households that buy insurance because they receive a coupon, and it

Table 3: Effect of IBLI on Coping Strategies as Drought Worsens

	Loan In	Loan Out	Gift In (Placebo)	Drought Herd
Panel A. Reduced form				
SPEI × any coupon	−0.073**	−0.002	−0.025	−0.042
	(0.028)	(0.023)	(0.053)	(0.029)
Panel B. 2SLS				
IBLI × SPEI	−0.726*	0.055	−0.600	−0.441
	(0.400)	(0.225)	(0.560)	(0.358)
IBLI	0.194	−0.060	0.380	0.134
	(0.173)	(0.099)	(0.242)	(0.133)
SPEI	0.015	−0.021	0.191**	−0.035
	(0.132)	(0.029)	(0.078)	(0.092)
Wu-Hausman p-value	0.022	0.775	0.152	0.263
First-stage F, IBLI	46.8	46.8	46.8	46.8
First-stage F, interaction	32.3	32.3	32.3	32.3
Outcome mean	0.135	0.061	0.334	0.134
Observations	3,652	3,652	3,652	3,652

Notes. All specifications include household fixed effects, round fixed effects, log TLU, household size, and settlement status. SPEI is centred at its sample mean, and lower values mark worse drought. 2SLS models instrument IBLI and IBLI × SPEI with coupon receipt and coupon receipt × SPEI. CV1 standard errors clustered by sublocation ($G = 16$) in parentheses; stars use $t(15)$ critical values. Table 6 reports inference suited to a small number of clusters. The Wu-Hausman row reports a diagnostic p-value. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

measures a differential response to a large change in drought conditions rather than an average effect across rounds. At average drought conditions, the IBLI coefficient in Panel B equals 0.194 and is not significant, so insurance does not raise borrowing in ordinary years. Any effect appears only as drought becomes severe, and with 16 clusters it cannot be estimated precisely.

The Wu-Hausman test rejects exogeneity of IBLI for livestock borrowing ($p = 0.022$), so the gap between the OLS and 2SLS estimates (Section 5.1) is too large to attribute to sampling noise.

The other three outcomes show no significant response. The interaction for livestock lending is small relative to its standard error ($p = 0.810$). Because most lending partners are not in the sample (Section 2), a matching rise in lending would not be observed even if it occurred. Gift receipt serves as a placebo: if the coupon captured unobserved social connectedness, gift receipt should respond as well. The gift interaction carries a large standard error and is not significant ($p = 0.301$). Drought-related herd movement also shows no significant response ($p = 0.237$), but the point estimate of −0.441 is not small. Because it cannot be distinguished either from zero or from an effect similar in size to the borrowing result, I treat this outcome as inconclusive.

5.4 Other Informal Arrangements

Table 4 applies the specification of Table 3 to cash loans, goods credit, and general transfers of cash and in-kind goods.

None of these outcomes shows a significant 2SLS response. In particular, insurance does not raise cash borrowing. The interaction for informal cash loans equals −0.040 ($p = 0.833$), and the interaction for formal loans has the opposite sign ($p = 0.466$). Section 6 discusses what this contrast with livestock borrowing

Table 4: Effect of IBLI on Other Informal Arrangements as Drought Worsens

	Received					Given	
	General Transfer	Informal Cash Loan	Formal Cash Loan	Informal Goods Credit	Any Informal Support	General Transfer	Any Informal Support
Panel A. Reduced form							
SPEI × any coupon	−0.122*	−0.018	0.029	−0.097	−0.117**	−0.084**	−0.089**
	(0.063)	(0.015)	(0.018)	(0.124)	(0.045)	(0.036)	(0.040)
Panel B. 2SLS							
IBLI × SPEI	−0.697	−0.040	0.109	−0.582	−0.706	−0.590	−0.621
	(0.558)	(0.186)	(0.146)	(4.928)	(0.501)	(0.439)	(0.442)
Observations	3,652	3,652	3,652	1,822	3,652	3,652	3,652

Notes. Each column reports a separate regression with the controls and fixed effects in Table 3; coefficient standard errors in parentheses. Negative coefficients mean that insured households use the arrangement more as drought worsens. The informal goods credit module covers only Rounds 4 and 5 of the estimation sample, and its first-stage F-statistics (3.8 and 2.0) fall below weak-instrument thresholds, so I do not interpret that column. The Any Informal Support (Received) measure includes livestock borrowing; the Any Informal Support (Given) measure includes livestock lending. CV1 standard errors clustered by sub-location ($G = 16$); stars use $t(15)$ critical values. None of the Panel B estimates reaches the 10% level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

implies, although the cash-loan estimates are imprecise enough that an effect of similar size cannot be ruled out.

The reduced form is more informative. As drought worsens, coupon recipients give more general transfers ($p = 0.035$) and more informal support overall ($p = 0.042$), and receive more general transfers ($p = 0.074$) and more informal support overall ($p = 0.020$). The corresponding 2SLS estimates lose significance (p between 0.18 and 0.23), because scaling a marginal reduced-form estimate by an imprecisely estimated first-stage interaction inflates the standard error. Every 2SLS point estimate in the table except that for formal cash loans has the same negative sign as the livestock borrowing result. This is a pattern worth revisiting with more data.

5.5 How Informal Networks Respond to Drought

The models in Tables 3 and 4 also show how informal networks respond to drought among uninsured households, through the SPEI coefficient β_3 in Equation 1. Table 5 reports this coefficient next to the IBLI by SPEI interaction. A positive SPEI coefficient means the arrangement becomes less common as drought worsens.

Drought shrinks the gift and transfer networks that uninsured households rely on. A one standard deviation worsening of drought lowers the probability that an uninsured household receives a livestock gift by 10.7 percentage points ($p = 0.027$), about a third of the 33.4% mean. It lowers the probability of receiving a general transfer by 12.3 percentage points ($p = 0.034$) and of giving one by 11.0 percentage points ($p = 0.081$), and the broad measure of support given falls by 11.1 percentage points ($p = 0.052$). A covariate shock that hits most members of a network at once should produce this pattern. Relatives and neighbours who would normally give face the same drought, so there is less to share.

The livestock loan network behaves differently. For uninsured households, drought has no measurable effect on livestock borrowing ($p = 0.914$) or lending ($p = 0.485$), and both point estimates are close to zero.

Because livestock loans are long-term and reciprocal, and lent animals remain part of the lender's wealth, lenders may maintain these arrangements while cutting back on outright gifts and transfers.

Insured households do not follow this pattern. For every outcome where drought reduces activity among uninsured households, the IBLI by SPEI interaction is negative and larger in absolute value than the SPEI coefficient. The point estimates imply that insured households avoid the contraction in gifts and transfers and increase their livestock borrowing. Only the livestock borrowing interaction reaches the 10% level, however, so this broader pattern is suggestive.

Table 5: Drought and Informal Network Activity, Uninsured and Insured Households

	SPEI (uninsured)	IBLI × SPEI
Livestock		
Livestock borrowed	0.015	−0.726*
	(0.132)	(0.400)
Livestock lent	−0.021	0.055
	(0.029)	(0.225)
Livestock gifts received	0.191**	−0.600
	(0.078)	(0.560)
Cash and in-kind		
General transfer received	0.220**	−0.697
	(0.094)	(0.558)
General transfer given	0.197*	−0.590
	(0.105)	(0.439)
Broad measures		
Any informal support received	0.119	−0.706
	(0.109)	(0.501)
Any informal support given	0.198*	−0.621
	(0.094)	(0.442)
Observations	3,652	3,652

Notes. 2SLS estimates of Equation 1. Each row reports the SPEI coefficient and the IBLI × SPEI interaction from the same regression. CV1 standard errors clustered by sub-location ($G = 16$) in parentheses; stars use $t(15)$ critical values. The SPEI coefficient measures the effect of drought on uninsured households. A positive SPEI coefficient means the outcome falls as drought worsens. A negative interaction means insured households use the arrangement more than uninsured households as drought worsens. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

5.6 Robustness

Small number of clusters. With 16 clusters, conventional cluster-robust inference can be unreliable, so I check the main results against a series of clustering methods (MacKinnon et al., 2023). For the reduced form I use the CV3 variance estimator, a cluster jackknife that re-estimates the model with each sub-location removed in turn, with $t(15)$ critical values. For the 2SLS estimates I use the wild restricted efficient (WRE) bootstrap in its cluster form (Davidson and MacKinnon, 2010; Roodman et al., 2019). The WRE bootstrap imposes the null hypothesis, re-estimates the first stage efficiently under it, and applies the same cluster-level weight to the structural and first-stage residuals. With 16 clusters there are only $2^{16} = 65{,}536$ distinct Rademacher weight vectors, so I enumerate all of them rather than sample. I report equal-tail bootstrap p-values, which MacKinnon et al. (2023) recommend for instrumental variables because 2SLS t-statistics

need not be centred at zero. Table 6 reports the results.

Table 6: Inference with a Small Number of Clusters, Main Estimates

	Loan In	Loan Out	Gift In (Placebo)	Drought Herd
Panel A. Reduced form, SPEI × any coupon				
Coefficient	−0.073	−0.002	−0.025	−0.042
CV1 standard error	(0.028)	(0.023)	(0.053)	(0.029)
CV3 standard error	[0.029]	[0.021]	[0.049]	[0.028]
p-value, CV1	0.020	0.934	0.649	0.168
p-value, CV3	0.023	0.929	0.621	0.156
Panel B. 2SLS, IBLI × SPEI				
Coefficient	−0.726	0.055	−0.600	−0.441
CV1 standard error	(0.400)	(0.225)	(0.560)	(0.358)
p-value, CV1	0.090	0.810	0.301	0.237
p-value, WRE bootstrap	0.021	0.789	0.222	0.125
Observations	3,652	3,652	3,652	3,652

Notes. Specifications follow Table 3. All inference clusters by sub-location ($G = 16$). CV1 standard errors in parentheses and CV3 (cluster-jackknife) standard errors in brackets; both p-values use $t(15)$ critical values. The WRE bootstrap is the wild restricted efficient bootstrap of Davidson and MacKinnon (2010), applied at the sub-location level with Rademacher weights; all 2^{16} weight vectors are enumerated, and the p-value is equal-tail. Appendix Table 24 and Appendix Table 25 report further variants.

The reduced form is unaffected. For livestock borrowing, the CV3 standard error is 0.029 against 0.028 under CV1, and the p-value moves from 0.020 to 0.023. None of the other three reduced-form estimates approaches significance under either estimator. For the 2SLS estimate, the WRE bootstrap gives $p = 0.021$ for livestock borrowing, against 0.090 under CV1. The other three 2SLS estimates remain insignificant.

The estimates also depend on a small number of sub-locations. Two of them, Kalacha and Loiyangalani, carry far more partial leverage for the reduced-form coefficient than the others (0.19 and 0.15, against an average of 0.06), and the effective number of clusters is 10.5 rather than 16 (Carter et al., 2017). Removing either one moves the 2SLS estimate toward zero, to −0.51 and −0.45, but all 16 delete-one-cluster estimates keep the negative sign, and the reduced-form estimates range from −0.086 to −0.052. Appendix B.3 reports these diagnostics in full.

Alternative drought measures. Table 7 replaces the recall-period minimum SPEI with three alternative measures, instrumenting IBLI with the continuous discount value rather than the binary coupon indicator.[3] The recall-period mean yields an interaction of −0.430 ($p = 0.013$). The long dry season average (June to August) also yields a negative interaction (−0.680), though less precisely estimated ($p = 0.222$). The binary drought indicator equals one when SPEI falls below −1, so a positive interaction marks a larger response in drought. It equals 0.288 ($p = 0.067$). In this specification, insured households borrow 26.0 percentage points less than uninsured households in non-drought rounds ($p = 0.014$), and the gap closes during drought. Under all three measures, insurance shifts borrowing toward drought periods, as in the main result.

Timing of coverage. Drought conditions could affect both the purchase decision and borrowing within the same round. To address this concern, I replace current coverage with coverage in the previous round, instrumented by the previous round's coupon (Table 8). The lagged first stage remains strong ($F = 98.8$),

[3]With the main drought measure, the continuous instrument yields an interaction of −0.803 (CV1 standard error 0.515), close to the −0.726 obtained with the binary coupon indicator (Appendix Table 11).

Table 7: Robustness to Alternative Drought Measures, Livestock Borrowing

	(1) Recall mean	(2) Long dry season	(3) Binary drought
IBLI × drought measure	−0.430**	−0.680	0.288*
	(0.152)	(0.534)	(0.146)
IBLI	−0.107	−0.077	−0.260**
	(0.117)	(0.126)	(0.094)
Household and round FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	3,652	3,652	3,652

Notes. 2SLS estimates with CV1 standard errors clustered by sub-location ($G = 16$). Columns 1 and 2 use continuous SPEI measures, where lower values mark worse drought, so a negative interaction means more borrowing in drought. Column 3 uses an indicator equal to one when SPEI falls below −1, so a positive interaction means more borrowing in drought. Each model instruments IBLI and its interaction with the drought measure using the continuous discount value and its interaction with the same measure. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

but the lagged interaction for borrowing equals 0.110 ($p = 0.637$), the opposite sign from the main estimate and far from significant. Because IBLI contracts cover a single year, coverage in the previous round often does not protect the household during the current drought, so this result is consistent with a borrowing response that depends on coverage held at the time of the shock. Given the imprecision of both estimates, however, the comparison shows mainly that the lagged specification offers no support for the main result.

Table 8: Lagged IBLI Coverage, Rounds 3 to 6

	Loan In	Loan Out	Gift In (Placebo)	Drought Herd
$\text{IBLI}_{t-1}\times$ SPEI	0.110	0.204	0.157	0.017
	(0.229)	(0.369)	(0.408)	(0.202)
p-value	0.637	0.587	0.705	0.934
Lagged first-stage F	98.8	98.8	98.8	98.8
Observations	3,522	3,522	3,522	3,522

Notes. The treatment equals IBLI coverage in the previous round, instrumented by coupon receipt in the previous round. CV1 standard errors clustered by sub-location ($G = 16$) in parentheses; p-values use $t(15)$ critical values. Controls and fixed effects follow Table 3. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

5.7 Heterogeneity by Wealth

Table 22 shows that borrowing rises with herd size, from 10.2% among households with fewer than 10 TLU to 21.2% among households with more than 20 TLU. To test whether the effect of insurance also varies across households, I estimate an instrumental forest (Athey et al., 2019). The forest uses coupon receipt as the instrument and estimates the effect of IBLI on livestock borrowing as a function of drought severity, log livestock wealth, household size, settlement status, and district.

Table 9 reports how often each covariate determines a split in the forest. Drought severity accounts for 37% of splits and livestock wealth for 34%, while household size, district, and settlement status account for the rest. That drought severity leads the ranking is consistent with the parametric finding that the

Table 9: Instrumental Forest, Variable Importance

Covariate	Share of splits
SPEI (centred)	0.367
Log TLU	0.338
Household size	0.185
District	0.056
Settlement status	0.054

Notes. Variable importance from an instrumental forest estimated with the `grf` package, clustered by sub-location. The outcome is livestock borrowing, the treatment is IBLI coverage, and the instrument is coupon receipt.

effect of insurance depends on drought, imprecise as that finding is. Splitting households at 15 TLU, the mean estimated effect is 0.39 below the threshold and 0.23 above it. The forest does not, however, detect systematic heterogeneity. The best linear projection of the estimated effects onto the covariates yields no significant coefficient ($p > 0.20$ for all), and neither does a rank-weighted test (Appendix Table 19). With 16 clusters the forest has limited power, so I treat the subgroup comparison as descriptive. Appendix Table 20 reports the estimated effects across the wealth distribution.

A forest split at an internally chosen threshold cannot show whether a difference by wealth is large enough to trust, so I add two direct checks based on the household's *baseline* wealth stratum: the original sampling stratum (Section 3.1), fixed at the household's first appearance in the panel. I use the baseline stratum rather than contemporaneous TLU because current wealth moves with drought and with livestock borrowing itself. Across Rounds 2 to 5, 42% of households cross a wealth-tercile boundary at least once, so splitting a household fixed-effects panel on current wealth would scatter a household's observations across regressions and risk conditioning on an outcome of the behaviour under study. Under the baseline stratum, every household stays in one group throughout.

Table 10 shows the main specification re-estimated for livestock borrowing within each baseline stratum. All three point estimates have the same sign as the pooled result, and none is individually significant. The instrument is not uniformly strong across strata. The joint first stage clears 10 in the Low stratum, but the interaction's first stage falls to 8.3 in the Medium stratum and the level term's to 9.9 in the High stratum, so the Medium and High estimates warrant caution.

Table 10: Livestock Borrowing by Baseline Wealth Stratum

	Low (<10 TLU)	Medium (10–20 TLU)	High (>20 TLU)
Panel A. Reduced form			
SPEI × any coupon	−0.069*	−0.059	−0.111
	(0.033)	(0.037)	(0.083)
Panel B. 2SLS			
IBLI × SPEI	−0.832	−1.361	−0.638
	(0.520)	(1.710)	(0.592)
IBLI	0.255	0.526	0.007
	(0.195)	(0.753)	(0.332)
SPEI	−0.045	0.053	0.101
	(0.112)	(0.193)	(0.191)
Wu-Hausman p-value	0.201	0.386	0.090
First-stage F, IBLI	25.8	13.6	9.9
First-stage F, interaction	12.1	8.3	11.3
Households	348	313	290
Observations	1,329	1,201	1,122

Notes. Outcome is livestock borrowing (loan_in). Baseline wealth stratum is the household's original sampling stratum, fixed at first observation rather than recomputed each round; 97.5% of households retain a single stratum throughout the panel. Controls and fixed effects follow Table 3, applied separately within each stratum. CV1 standard errors clustered by sub-location in parentheses; stars use $t(15)$ critical values. No coefficient in Panel B reaches the 10% level in any stratum. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

I test the same question formally with a single pooled regression. I interact the IBLI × SPEI term with Medium and High baseline-wealth indicators and test jointly whether both interactions equal zero, that is, whether the slope differs from the Low-wealth baseline at all. Appendix Table 21 reports the results. Neither wealth interaction is individually distinguishable from zero, and the joint test gives no evidence that the slope differs by wealth ($F = 0.14$, $p = 0.870$). The first stage for the two higher-order interactions is weak ($F = 3.3$ and 5.3), so the test has limited power to detect a difference.

6 Mechanisms

The results show two patterns. Drought shrinks gift and transfer networks among uninsured households, and insured households appear to borrow more livestock during drought. I consider four channels that could produce the second pattern and bring additional evidence to bear on each, most of it reported in Appendix A. None of these tests is decisive on its own, and several rest on small samples or a single survey round.

Insurance makes a household a safer borrower. A lender in an informal network extends a loan when it expects the borrower to return or reciprocate it. Coverage protects part of the borrower's herd value during drought, which could lower the perceived risk that the borrower cannot repay. Formal credit markets work the same way, where insurance relaxes borrowing limits (Boucher et al., 2008). The lenders in this

setting are overwhelmingly kin. Family and clan members account for 92% of the livestock loans households receive (Appendix Table 18), which fits evidence that risk-sharing ties form among people who know and trust one another (Attanasio et al., 2012), and who are therefore well placed to observe whether a relative holds a policy.

Three pieces of evidence fit this channel. First, the response is specific to livestock. Insurance does not raise informal cash borrowing (Table 4), which it should if it worked only by relaxing household liquidity. Livestock loans rest on long-term reciprocal ties, and the lender bears the risk that a borrowed animal is not returned or replaced, so a repayment-risk constraint should bind most tightly there. Second, insurance appears to change how much households borrow, not only whether they borrow. With the total number of animals borrowed in the round as the outcome (Appendix Table 12), a one standard deviation worsening of drought raises the number of animals borrowed by coupon recipients by 0.18 more than by non-recipients in the reduced form ($p = 0.024$), about 41% of the 0.43-animal mean. The corresponding change in the probability of borrowing is about 30% of its mean, so the count rises somewhat more than proportionally, which suggests that insured households are not simply taking on more small loans. The 2SLS estimate has the same sign but is imprecise (-2.15, $p = 0.237$). Third, evidence from a closely related setting points the same way. Among Borana pastoralists in southern Ethiopia, Takahashi et al. (2019) find that a household's own IBLI purchase does not change its willingness to lend cattle under *dabare*, but a randomly matched household's purchase weakly raises willingness to lend to that household. This lender-side response mirrors the borrower-side response I find. Because *dabare* combines credit and insurance, they consider whether the effect reflects the insured match's creditworthiness. They find it unchanged when they control for perceived creditworthiness and favour an explanation based on reciprocity, since an insured household is better placed to help when the next covariate shock arrives, so their evidence does not separate the safer-borrower channel from expected reciprocity. They also caution that the result does not survive all of their robustness checks, and they report that actual transfer data from the same sites show no correlation between a household's own uptake and its transfers. I do not observe lenders' perceptions directly, and the number of animals lent by sampled households does not respond (Appendix Table 12), though most lenders to sampled borrowers are outside the sample.

Informal loans cover the basis risk that insurance leaves behind. IBLI pays out on a predicted area-average mortality rate, not on a household's own losses. A household whose herd losses exceed what the index predicts remains exposed, and IBLI in Marsabit carries substantial basis risk of this kind (Jensen et al., 2016). Mobarak and Rosenzweig (2012) show that informal risk sharing and index insurance can act as complements for this reason. The network absorbs the idiosyncratic part of a loss, while the index covers the covariate part that the network cannot. Under this channel, insured households turn to kin for animals when their own losses exceed the payout, and kin are more willing to help because the insurer has already absorbed part of the shock. The channel also fits the timing result. Borrowing responds to coverage held during the drought and not to coverage held in the previous round (Table 8), and basis risk only arises for a household that holds a policy when the index is computed. I cannot test this channel directly, because doing so would require matching each household's livestock losses to the payout it received, and the payout data are too thin to support that comparison.

Insurance brings resources from outside the network. Informal risk sharing works poorly against covariate shocks, because the households that would normally help face the same shock (Fafchamps et al., 1998). Section 5.5 shows this contraction among uninsured households. An IBLI payout arrives from an insurer, so it adds to the resources available within the network at the moment they are scarcest. This

channel would explain why every interaction in Table 5 points toward more activity for insured households, though only the livestock borrowing estimate is individually significant.

Two descriptive comparisons bear on this channel. Among ever-insured households in Round 7, those who report having received a payout score better on every food security measure I observe (Appendix Table 15). Their reduced Coping Strategies Index is 12% lower (Maxwell and Caldwell, 2008), and their Household Hunger Scale score is 22% lower (Ballard et al., 2011). They also report fewer days of borrowing food or relying on help from relatives. Payout recipients spend most of the money on food (52% of households) and on buying livestock (38%), and only a handful use it to repay debts or give to others (Appendix Table 16). Among the few households asked whether they changed their drought response because they expected a payment, the most common change is selling fewer animals. Payout recipients also borrow livestock less often in Round 7 (6.8% against 10.9%), which fits the same channel. Once a payout has arrived and been used to restock, the household needs less from its network. The borrowing response in the main estimates instead reflects coverage held while the drought is underway, before any payout is received. These comparisons are not causal. Payout receipt depends on where a household lives and when it held a policy, and the comparison rests on 44 recipients and 101 non-recipients. In the Round 6 cross-section, households that held coverage in the previous round report a less severe drought shock and a faster recovery, but neither estimate is close to significant ($p = 0.48$ and $p = 0.65$; Appendix Table 14).

Payouts give households resources to reciprocate. Livestock lending depends on expected future reciprocity (Ligon et al., 2002; Coate and Ravallion, 1993), so a payout could let an insured household meet obligations that keep its loans in place. The evidence for this channel is the weakest of the four. The reduced form shows that coupon recipients give more transfers during drought, but the corresponding 2SLS estimate is not significant (Table 4). The spending records point the other way. Only 8% of payout recipients report using the payout to repay debts and 4% to give to others (Appendix Table 16). Among insured households, the few that also belong to a group-based savings or lending scheme borrow livestock far more often than other insured households (46% against 12%; Appendix Table 17), which could reflect a greater capacity to reciprocate, but this comparison rests on 13 household-rounds. I therefore treat this channel as a hypothesis for future work.

The survey asks households directly whether they coped with food shortages by relying on assistance from others or by selling livestock, which would speak to all four channels. These questions appear only in Rounds 5 and 6, which requires lagged coverage and the lagged coupon, and in that subsample the lagged coupon barely predicts coverage (first-stage $t = 1.40$). The resulting estimates are uninformative (Appendix Table 13).

The evidence favours the safer-borrower and outside-resource channels, with basis-risk complementarity consistent with the data but untested. This pattern is broadly consistent with the theoretical prediction of Dubois et al. (2008), who show that formal insurance can strengthen informal risk sharing by easing the commitment problem, and with the absence of crowding out that Takahashi et al. (2019) find for IBLI in southern Ethiopia. It contrasts with the crowding out of informal transfers that Strupat and Klohn (2018) find for public health insurance in Ghana, a difference that may reflect the type of shock, since health shocks are largely idiosyncratic, which informal networks can absorb, whereas drought is covariate, which they cannot. The estimates do not support the concern raised by Arnott and Stiglitz (1991) that formal insurance weakens the incentives that sustain informal arrangements.

7 Conclusion

This paper asks how informal risk-sharing networks behave during drought, and whether formal insurance crowds them out or crowds them in. I study index-based livestock insurance among pastoralist households in Marsabit, Kenya, and use randomly assigned discount coupons to instrument for insurance coverage.

Among uninsured households, drought strains these networks, and a one standard deviation worsening of drought lowers the probability of receiving a livestock gift by 11 percentage points, of receiving a cash or in-kind transfer by 12 points, and of giving a transfer by 11 points. The livestock loan network does not contract, since uninsured households borrow and lend livestock at the same rate in drought as in normal conditions.

I find suggestive evidence that insurance raises livestock borrowing as drought worsens. The same worsening of drought raises the probability of borrowing by roughly 40 percentage points more for insured than for uninsured households. This estimate is significant at the 10% level with conventional clustered standard errors and at the 5% level under the wild restricted efficient bootstrap, and the reduced form, which relies only on random assignment, is significant at the 5% level under every correction for the small number of clusters that I apply. Insurance shows no significant relationship with livestock lending or gift receipt, and the herd movement estimate is too imprecise to rule out an effect similar in size to the borrowing result. The main result holds in sign under alternative drought measures but not when coverage is lagged by one round, consistent with an effect tied to coverage held during the shock.

I find no significant 2SLS evidence that insurance changes cash borrowing, goods credit, or general transfers. In the reduced form, however, coupon recipients give and receive more general transfers as drought deepens, and the 2SLS point estimates for these outcomes all point toward insured households avoiding the contraction that uninsured households experience.

Several limitations apply. First, inference on the main 2SLS estimate depends on the method, from $p = 0.09$ with conventional standard errors to $p = 0.02$ under the bootstrap, and the estimate leans on two of the 16 sub-locations, so it is suggestive of a large effect. Second, the estimates apply to coupon compliers, who may differ from the wider pastoralist population. Third, the lagged specification does not reproduce the main result, though neither estimate is precise. Fourth, the survey does not identify lending households, and most lenders are not in the sample, so I cannot observe the lending response or reciprocity between specific pairs. Finally, evidence on how many animals change hands is limited. The number of animals borrowed rises with coverage as drought worsens in the reduced form, but the 2SLS estimate is imprecise.

Taken together, the findings point toward index insurance and informal networks acting as complements rather than substitutes in northern Kenya, though the evidence is not definitive. They show that drought erodes informal gift and transfer networks and that insurance does not deepen that erosion, but not that formal insurance reliably crowds in informal borrowing, and they offer some indication that insurance helps keep networks active when a covariate shock leaves them short of resources. A larger sample, spanning more sub-locations or more rounds, would allow sharper tests. Future work could also survey both sides of each loan to observe reciprocity directly, match household losses to payouts to test whether informal loans cover the basis risk that insurance leaves behind, and elicit whether lenders view insured households as more reliable borrowers.

A Supplemental Tables and Figures

This appendix collects results from additional analyses that support, but are not central to, the main results. Section A.1 reports the main specification with the continuous discount instrument. Sections A.2 through A.7 report the evidence behind the mechanism discussion in Section 6. Sections A.8 and A.9 report additional output from the instrumental forest, and Section A.10 reports the pooled test of wealth heterogeneity. Several of these analyses use a single survey round or a small subsample, and I note the limits of each where it applies.

A.1 Continuous Discount Instrument

Table 11 compares the main specification, which instruments coverage with the binary coupon indicator, to the same specification instrumented with the continuous discount value. The two interaction estimates are close in magnitude. The continuous instrument yields a larger standard error, consistent with its weaker first stage (Table 2).

Table 11: Livestock Borrowing, Binary and Continuous Instruments

	(1) Binary coupon	(2) Discount value
IBLI × SPEI	−0.726*	−0.803
	(0.400)	(0.515)
IBLI	0.194	0.060
	(0.173)	(0.171)
SPEI	0.015	0.042
	(0.132)	(0.143)
Household and round FE	Yes	Yes
Controls	Yes	Yes
Observations	3,652	3,652

Notes. 2SLS estimates of Equation 1 for livestock borrowing with CV1 standard errors clustered by sub-location ($G = 16$). Column 1 reproduces Panel B of Table 3. Column 2 instruments IBLI and IBLI × SPEI with the discount value and its interaction with SPEI. Both specifications use Rounds 2 to 5, the rounds in which the instrument is observed. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

A.2 Intensive Margin: Number of Animals Borrowed and Lent

Table 12 applies the specification in Table 3 to the total number of animals a household borrows or lends in a round, summed across all loan events and set to zero for households with no loan event. These outcomes are counts, so the coefficients are in animals. The reduced form for animals borrowed is negative and significant at the 5% level, the same pattern as for the probability of borrowing. The 2SLS estimate has the same sign and is imprecise. The Wu-Hausman test again rejects the exogeneity of coverage ($p = 0.032$). Animals lent show no response.

A.3 Self-Reported Coping Strategies

In Rounds 5 and 6 the survey asks whether the household coped with food shortage by relying on assistance from others or by selling livestock. In Round 5 these questions were asked only of households that reported

Table 12: Effect of IBLI on the Number of Animals Borrowed and Lent as Drought Worsens

	Animals borrowed	Animals lent
Panel A. Reduced form		
SPEI × any coupon	−0.316**	0.026
	(0.126)	(0.139)
Panel B. 2SLS		
IBLI × SPEI	−2.152	−0.708
	(1.746)	(2.250)
IBLI	−0.026	0.787
	(1.011)	(1.502)
SPEI	0.158	0.114
	(0.369)	(0.285)
Wu-Hausman p-value	0.032	0.770
First-stage F, IBLI	46.8	46.8
First-stage F, interaction	32.3	32.3
Outcome mean (animals)	0.434	0.276
Observations	3,652	3,652

Notes. Outcomes are the total number of animals borrowed (survey item s6q30) or lent (s6q39) across all loan events in the round. Controls, fixed effects, and instruments follow Table 3. CV1 standard errors clustered by sub-location ($G = 16$) in parentheses; stars use $t(15)$ critical values. A negative interaction means insured households borrow or lend more animals as drought worsens. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

a food shortage in the previous 12 months. In Round 6 they were asked of every household. Because the coupon program ended after Round 5, I use coverage and coupon receipt from the previous round, as in Table 8. In this two-round subsample the lagged coupon predicts the lagged interaction term well ($t = 4.16$) but predicts lagged coverage itself only weakly ($t = 1.40$). Table 13 reports the estimates. Both interactions carry large standard errors, and are likely uninformative. The Round 5 filter on self-reported food shortage adds a further concern, since coverage could itself affect whether a household reports a shortage.

A.4 Self-Reported Shock Severity and Recovery

Round 6 asks households whether a drought affected them in the past year, how severe its effect was on their main activity, and how long they took to recover. Table 14 regresses the two answers on coverage in the previous round among households that report being affected. With a single round, I cannot include household fixed effects, so these are cross-sectional estimates. The 2SLS coefficients on coverage are negative for both outcomes, the sign I would expect if coverage cushions the shock, but neither is close to significant.

A.5 Payout Receipt and Food Security

Round 7 (2020) asks households whether they ever received an IBLI payout and adds a food security module. Neither can enter the panel estimates, because Round 7 has no coupon instrument and its questionnaire does not identify an active contract. Table 15 instead compares ever-insured households by whether they report having received a payout. The reduced Coping Strategies Index weights five consumption coping behaviours in the past seven days by severity (Maxwell and Caldwell, 2008), and the Household Hunger Scale scores three items on food deprivation in the past 30 days from 0 to 6 (Ballard et al., 2011). Higher values of both mean worse food security. Table 16 reports what recipients spent their payouts on and how households

Table 13: Lagged IBLI Coverage and Self-Reported Coping Strategies, Rounds 5 and 6

	Relied on assistance from others	Sold livestock
Panel A. Reduced form		
SPEI × coupon$_{t-1}$	−0.026	0.011
	(0.035)	(0.051)
Panel B. 2SLS		
IBLI$_{t-1}$× SPEI	0.451	0.456
	(0.899)	(1.078)
IBLI$_{t-1}$	−1.053	−0.266
	(0.947)	(1.432)
p-value, interaction	0.623	0.678
First-stage t, coupon$_{t-1}$ on IBLI$_{t-1}$	1.40	1.40
First-stage t, coupon$_{t-1}$× SPEI on interaction	4.16	4.16
Outcome mean, Rounds 5–6	0.35	0.59
Observations	1,300	1,300

Notes. Outcomes are survey items s7q8c and s7q8e. Controls and fixed effects follow Table 3. The treatment is IBLI coverage in the previous round, instrumented by coupon receipt in the previous round. First-stage t-statistics use CV1 standard errors. Outcome means are computed over all households answering the item in Rounds 5 and 6. CV1 standard errors clustered by sub-location ($G = 16$) in parentheses; p-values use $t(15)$ critical values. A negative interaction means insured households use the strategy more as drought worsens. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 14: Lagged IBLI Coverage, Drought Severity and Recovery, Round 6

	Severity (0–3)	Recovery time (1–5)
Panel A. OLS		
IBLI$_{t-1}$	0.065	0.069
	(0.119)	(0.174)
Panel B. 2SLS		
IBLI$_{t-1}$	−1.315	−1.250
	(1.802)	(2.696)
Log TLU	−0.021	−0.122**
	(0.063)	(0.052)
p-value, IBLI$_{t-1}$	0.477	0.650
First-stage F	20.4	21.3
Outcome mean	2.23	2.42
Observations	638	633

Notes. Sample restricted to Round 6 households that report being negatively affected by drought (survey module S7B). Severity (s7q13) runs from 0 (no effect) to 3 (large effect). Recovery time (s7q14) runs from 1 (within a month) to 5 (more than a year, not fully recovered), so a negative coefficient means faster recovery. Controls are log TLU, household size, and settlement status. The 2SLS model instruments coverage in Round 5 with coupon receipt in Round 5. Panel A reports conventional standard errors; Panel B reports CV1 standard errors clustered by sub-location ($G = 16$) with $t(15)$ critical values, and the first-stage F uses the same standard errors. Outcome means are computed over all drought-affected households. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

that expected a payment changed their response to drought. These comparisons are descriptive. Payout receipt depends on location and on when a household held a policy, both of which also shape its exposure to drought.

Table 15: Food Security and Informal Activity by Payout Receipt, Ever-Insured Households, Round 7

	No payout	Payout received
Reduced Coping Strategies Index	15.9	14.0
Days borrowing food or relying on help	1.54	1.41
Household Hunger Scale (0–6)	1.63	1.27
Livestock borrowed	0.109	0.068
Livestock lent	0.010	0.000
General transfer received	0.455	0.545
General transfer given	0.000	0.000
Households	101	44

Notes. Sample of households that held an IBLI contract in at least one of Rounds 2 to 6 and answered the Round 7 payout question. The reduced Coping Strategies Index uses the standard weights of 1, 2, 1, 3, and 1 on eating less preferred foods, borrowing food or relying on help, limiting portion sizes, restricting adult consumption, and reducing the number of meals. The second row reports the unweighted borrowing item alone. The Household Hunger Scale codes each of its three items as 0 (never), 1 (rarely or sometimes), or 2 (often). Rows below the food security measures are shares of households. No ever-insured household reports giving a general transfer in Round 7.

A.6 Group Membership and Informal Activity among Insured Households

Table 17 compares insured household-rounds by whether the household uses a group-based savings or lending scheme. Group members use every form of informal support more often. Only 13 insured household-rounds belong to such a group, too few to support a separate instrumented interaction, so I report the comparison as descriptive.

A.7 Counterparties in Informal Exchange

Table 18 reports who households exchange with. Panel A covers cash and in-kind transfers, recorded in every round from Round 2. Panel B covers livestock loans. The survey records the relationship to the other party only in Round 6, so Panel B rests on 49 borrowing and 34 lending events.

A.8 Instrumental Forest Diagnostics

Table 19 reports additional output from the instrumental forest in Section 5. The distribution of estimated effects has a median of 0.25 and a mean of 0.35, but a few estimates are extreme, ranging from -107.5 to 19.0. An instrumental forest divides the reduced form by a locally estimated first stage, and where the coupon barely predicts coverage within a leaf, the resulting estimate can be very large, so I rely on the median and on grouped averages rather than on individual predictions. The best linear projection finds no covariate that predicts the estimated effects, and the rank-weighted average treatment effect is negative and small relative to its standard error, so the forest does not detect systematic heterogeneity.

Table 16: Use of IBLI Payouts and Changes in Drought Response, Round 7

	Households	Share (%)
Panel A. Payout spent on (50 households)		
Food	26	52.0
Livestock purchases	19	38.0
Savings	6	12.0
Veterinary services and vaccination	5	10.0
Health services	4	8.0
Repaying debt	4	8.0
School fees	3	6.0
Fodder	3	6.0
Water for the household	3	6.0
Water for livestock	2	4.0
Gifts or sharing with others	2	4.0
Other	3	6.0
Panel B. Change in drought response because a payment was expected (20 households)		
No change	14	70.0
Sold fewer livestock	5	25.0
Kept children in school	3	15.0
Bought more food on credit	1	5.0
Bought less food on credit	1	5.0

Notes. Panel A uses survey item s15q47l, which records whether IBLI payouts were spent on each listed item. Panel B uses item `ibli_payouts_change_coping`, asked of households that said they changed their drought response because they knew payments were coming. Households can report more than one item, so shares do not sum to 100.

Table 17: Informal Activity among Insured Households by Savings-Group Membership

	Not a group member	Group member
Livestock borrowed	0.123	0.462
Livestock lent	0.048	0.077
Any informal support received	0.290	0.769
Any informal support given	0.457	0.692
Household-rounds	652	13

Notes. Sample of household-rounds with an active IBLI contract. Group members are households that report a CARE savings and lending group or another informal saving scheme as a saving or lending channel (survey item s14q2). Entries are shares of household-rounds.

Table 18: Counterparties in Informal Transfers and Livestock Loans

	Received / Borrowed		Given / Lent	
	Events	Share (%)	Events	Share (%)
Panel A. Cash and in-kind transfers, Rounds 2 to 7				
Relatives outside the household	3,499	70.2	1,909	55.3
Neighbour or friend	1,382	27.7	1,541	44.6
Relatives and neighbours	22	0.4	–	–
Other	78	1.6	3	0.1
Panel B. Livestock loans, Round 6				
Family member	34	69.4	22	64.7
Clan member	11	22.4	6	17.6
Neighbour or friend	3	6.1	4	11.8
Other member of the village	1	2.0	2	5.9

Notes. Panel A uses the transfer source recorded in module S9A. Panel B uses items s6q29a1 (borrowed) and s6q38d (lent), fielded in Round 6 only.

Table 19: Instrumental Forest, Distribution of Estimated Effects and Tests of Heterogeneity

Panel A. Distribution of estimated effects

	Value
Minimum	−107.50
25th percentile	0.050
Median	0.247
Mean	0.354
75th percentile	0.480
Maximum	18.99

Panel B. Best linear projection

	Coef.	SE	p
Constant	10.223	(9.915)	0.303
Log TLU	−0.802	(1.185)	0.499
Household size	0.650	(0.578)	0.261
Settlement status	−3.494	(2.908)	0.230
SPEI (centred)	6.945	(7.181)	0.334
District	−1.437	(2.093)	0.492

Panel C. Rank-weighted average treatment effect (AUTOC)

Estimate	−1.548	(2.253)

Panel D. Mean estimated effect by herd size

	Below threshold	At or above
10 TLU threshold	0.348	0.365
15 TLU threshold	0.395	0.228

Notes. Output from the instrumental forest described in Table 9, estimated with the `grf` package and clustered by sub-location. The outcome is livestock borrowing, the treatment is IBLI coverage, and the instrument is coupon receipt. Panel B standard errors are cluster- and heteroskedasticity-robust. Panel C ranks households by their estimated effect.

A.9 GRF CATE Profiles by Wealth

Table 20 reports the mean estimated CATE by ventile of livestock wealth, from the instrumental forest. While the estimates are noisy at the bin level, the broad pattern shows positive CATEs across most of the wealth distribution, with somewhat larger effects for households in the middle ranges (roughly 2–8 TLU).

Table 20: Mean CATE by Livestock Wealth Ventile

Ventile	Median TLU	Mean CATE	N
1	0.0	0.269	185
2	0.2	0.379	185
3	0.6	0.353	185
4	1.2	0.525	185
5	1.8	0.498	185
6	2.5	0.491	185
7	3.4	0.583	184
8	4.1	0.731	184
9	4.9	0.188	184
10	6.0	0.364	184
11	7.2	−0.046	184
12	8.5	−0.124	184
13	10.0	0.438	184
14	11.7	0.618	184
15	13.7	0.478	184
16	16.2	0.461	184
17	19.0	0.120	184
18	24.2	0.105	184
19	33.2	0.290	184
20	55.6	0.363	184

Notes: Estimated from an instrumental forest using `grf`. The treatment is IBLI, the instrument is coupon receipt, and the outcome is livestock borrowing. Clusters are sub-locations. TLU is the median within each ventile.

A.10 Pooled Test of Wealth Heterogeneity

Table 21: Does the Crowding-In Effect Vary by Wealth? A Pooled Test

	2SLS
IBLI × SPEI (Low, baseline)	−0.580
	(1.147)
IBLI × SPEI × Medium	−0.861
	(2.383)
IBLI × SPEI × High	0.068
	(1.697)
Joint test, both wealth interactions = 0 (F)	0.14
p-value	0.870
First-stage F: IBLI × SPEI	10.6
First-stage F: × Medium interaction	3.3
First-stage F: × High interaction	5.3
Household and round FE	Yes
Controls	Yes
Observations	3,652

Notes. Single pooled 2SLS regression on the full estimation sample. IBLI × SPEI is interacted with Medium and High baseline-wealth dummies (Low is the omitted category); the level terms IBLI, IBLI × Medium, IBLI × High, and SPEI × Medium/High are included but not shown. Medium and High enter only as interactions: as time-invariant household characteristics, their own level effect is absorbed by the household fixed effect. Six endogenous regressors, each instrumented by the corresponding interaction of the discount coupon; CV1 standard errors clustered by sub-location in parentheses. The joint test is a Wald test with an $F(2, 15)$ reference distribution. First-stage F-statistics are for the full six-instrument system. No coefficient in this table reaches the 10% level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

B Data and Descriptive Statistics

This appendix collects material describing the data itself: how each variable used in the main text is constructed from the raw survey and climate modules, full summary statistics on those variables by wealth stratum, the cluster structure, and the inference with a small number of clusters reported in Section 5.6.

B.1 Variable Construction

B.1.1 Outcome Variables

Livestock borrowed (loan_in). Constructed from Section 6D (Livestock Intake). A household–round observation is coded as 1 if any intake event in that round is classified as `s6q29 == "Loaned in/Borrowed"`.

Livestock lent (loan_out). Constructed from Section 6E (Livestock Offtake). Coded as 1 if any offtake event is classified as `s6q38 == "Loaned"`.

Livestock gifts received (gift_in). From Section 6D, coded as 1 if any intake event is `s6q29 == "Received as gifts"`.

Drought herd movement (drought_herd). From Section 10 (Herd Migration), coded as 1 if any satellite camp has reason `s10q6 == "for coping with drought"`.

B.1.2 Treatment and Instrument

IBLI coverage (ibli). From Section 15B (IBLI Contracts). Coded as 1 if the household has at least one non-missing contract record (`s15q19` non-missing) in the round.

Discount coupon (any_discount). From Section 15E (Game and Discount Coupon). Mapped to the survey round based on the sales period. The maximum discount value across relevant sales windows is used. Coded as 1 if any discount value > 0.

B.1.3 Drought Measure

SPEI-3 values are obtained from TerraClimate at the sub-location level. For each survey round, the recall period is defined as October of the prior year through September of the survey year (matching the survey's recall window). The minimum SPEI-3 value over this period captures peak drought stress. The variable is centred at its sample mean (-1.073) for estimation.

B.1.4 TLU Conversion

Livestock holdings from Section 6A are converted to tropical livestock units using the following weights: camels = 1.0, cattle = 0.7, goats = 0.1, sheep = 0.1. The sum across all species gives household TLU, which enters the estimation in logs ($\log(1 + \text{TLU})$).

B.2 Full Summary Statistics by Wealth Stratum

Table 22: Summary Statistics by Wealth Stratum

	Obs.	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
Panel A. Low Wealth (<10 TLU)								
Outcomes								
Livestock borrowed (loan in)	2,303	0.102	0.303	0.000	0.000	0.000	0.000	1.000
Livestock lent (loan out)	2,303	0.037	0.190	0.000	0.000	0.000	0.000	1.000
Livestock gifts received	2,303	0.260	0.439	0.000	0.000	0.000	1.000	1.000
Drought herd movement	2,303	0.120	0.325	0.000	0.000	0.000	0.000	1.000
Treatment and Instrument								
IBLI coverage	2,303	0.175	0.380	0.000	0.000	0.000	0.000	1.000
Discount coupon received (any)	2,303	0.717	0.450	0.000	0.000	1.000	1.000	1.000
Discount value, all households	2,303	28.8	24.2	0.0	0.0	30.0	50.0	80.0
Discount value, recipients only	1,652	40.2	18.9	10.0	20.0	40.0	60.0	80.0
Drought								
SPEI-3 recall minimum	2,303	-1.090	0.556	-2.179	-1.420	-1.111	-0.627	0.033
Controls								
Livestock (TLU)	2,303	3.6	2.9	0.0	0.9	3.1	5.9	9.9
Household size	2,303	5.6	2.5	1.0	4.0	5.0	7.0	19.0
Additional Informal Arrangements								
General transfer received	2,303	0.591	0.492	0.000	0.000	1.000	1.000	1.000
General transfer given	2,303	0.417	0.493	0.000	0.000	0.000	1.000	1.000
Informal cash loan received	2,303	0.051	0.221	0.000	0.000	0.000	0.000	1.000
Formal cash loan received	2,303	0.034	0.182	0.000	0.000	0.000	0.000	1.000
Informal goods credit received	1,182	0.459	0.499	0.000	0.000	0.000	1.000	1.000
Any informal support received (broad)	2,303	0.338	0.473	0.000	0.000	0.000	1.000	1.000
Any informal support given (broad)	2,303	0.436	0.496	0.000	0.000	0.000	1.000	1.000
Panel B. Medium Wealth (10 to 20 TLU)								
Outcomes								
Livestock borrowed (loan in)	784	0.171	0.377	0.000	0.000	0.000	0.000	1.000
Livestock lent (loan out)	784	0.092	0.289	0.000	0.000	0.000	0.000	1.000
Livestock gifts received	784	0.464	0.499	0.000	0.000	0.000	1.000	1.000
Drought herd movement	784	0.135	0.342	0.000	0.000	0.000	0.000	1.000
Treatment and Instrument								
IBLI coverage	784	0.181	0.385	0.000	0.000	0.000	0.000	1.000
Discount coupon received (any)	784	0.745	0.436	0.000	0.000	1.000	1.000	1.000
Discount value, all households	784	30.2	24.5	0.0	0.0	30.0	50.0	80.0
Discount value, recipients only	584	40.5	19.6	10.0	20.0	40.0	60.0	80.0
Drought								
SPEI-3 recall minimum	784	-1.013	0.591	-2.179	-1.431	-0.952	-0.544	0.033
Controls								
Livestock (TLU)	784	14.2	2.8	10.0	11.8	13.9	16.6	20.0
Household size	784	6.2	2.4	1.0	4.8	6.0	8.0	15.0
Additional Informal Arrangements								
General transfer received	784	0.506	0.500	0.000	0.000	1.000	1.000	1.000
General transfer given	784	0.444	0.497	0.000	0.000	0.000	1.000	1.000

Continued on next page

Table 22 (continued)

	Obs.	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
Informal cash loan received	784	0.052	0.223	0.000	0.000	0.000	0.000	1.000
Formal cash loan received	784	0.014	0.118	0.000	0.000	0.000	0.000	1.000
Informal goods credit received	434	0.571	0.495	0.000	0.000	1.000	1.000	1.000
Any informal support received (broad)	784	0.434	0.496	0.000	0.000	0.000	1.000	1.000
Any informal support given (broad)	784	0.482	0.500	0.000	0.000	0.000	1.000	1.000
Panel C. High Wealth (>20 TLU)								
Outcomes								
Livestock borrowed (loan in)	600	0.212	0.409	0.000	0.000	0.000	0.000	1.000
Livestock lent (loan out)	600	0.110	0.313	0.000	0.000	0.000	0.000	1.000
Livestock gifts received	600	0.450	0.498	0.000	0.000	0.000	1.000	1.000
Drought herd movement	600	0.183	0.387	0.000	0.000	0.000	0.000	1.000
Treatment and Instrument								
IBLI coverage	600	0.177	0.382	0.000	0.000	0.000	0.000	1.000
Discount coupon received (any)	600	0.708	0.455	0.000	0.000	1.000	1.000	1.000
Discount value, all households	600	28.4	24.4	0.0	0.0	30.0	50.0	80.0
Discount value, recipients only	425	40.1	19.2	10.0	20.0	40.0	60.0	80.0
Drought								
SPEI-3 recall minimum	600	-1.087	0.527	-2.179	-1.514	-1.079	-0.619	0.033
Controls								
Livestock (TLU)	600	39.4	23.5	20.1	24.5	32.0	46.5	270.6
Household size	600	5.9	2.4	1.0	4.0	6.0	7.0	13.0
Additional Informal Arrangements								
General transfer received	600	0.497	0.500	0.000	0.000	0.000	1.000	1.000
General transfer given	600	0.487	0.500	0.000	0.000	0.000	1.000	1.000
Informal cash loan received	600	0.123	0.329	0.000	0.000	0.000	0.000	1.000
Formal cash loan received	600	0.003	0.058	0.000	0.000	0.000	0.000	1.000
Informal goods credit received	231	0.506	0.501	0.000	0.000	1.000	1.000	1.000
Any informal support received (broad)	600	0.418	0.494	0.000	0.000	0.000	1.000	1.000
Any informal support given (broad)	600	0.537	0.499	0.000	0.000	1.000	1.000	1.000

Notes. Sample includes the 3,687 household-round observations in the primary estimation sample, Rounds 2 to 5 (2010 to 2013), the rounds in which the discount coupon instrument is observed. Wealth strata follow the sampling strata (Section 3.1: low <10 TLU, medium 10 to 20 TLU, high >20 TLU), measured contemporaneously in each household-round; because herd size changes over time, the panels here are not equal in size, unlike the original stratified sample at baseline. All binary/indicator variables are reported as proportions (0 to 1); Obs. gives the number of non-missing household-round observations for that variable within the panel. Discount value is measured in percentage points of the insurance premium (0 to 80); the recipients-only row conditions on a nonzero discount. Livestock (TLU), household size, and SPEI-3 retain their natural units. The informal cash-loan and formal cash-loan modules are fielded in Rounds 2 to 5 (fully covered here); the informal goods-credit module (Section 14D) is fielded in Rounds 4 to 6, so within this Rounds 2–5 sample it is observed only in Rounds 4 and 5, which is why its Obs. column is smaller than the other rows. TLU conversions: cattle 0.7, camels 1.0, goats and sheep 0.1.

B.3 Cluster Diagnostics and Inference with a Small Number of Clusters

The estimation sample spans 16 sub-locations. Table 23 summarises the distribution of cluster sizes in the 3,652 household-round observations used in the main specifications. The max-to-median ratio is 2.27.

Table 23: Cluster Size Distribution, Estimation Sample

	Min	1st Quartile	Median	Mean	Max
Observations per sub-location	119	154	183	228	414

Tables 24 to 26 extend Section 5.6. Table 24 reports every reduced-form estimate cited in the text under four methods: CV1 and CV3 standard errors with $t(15)$ critical values, and the wild cluster restricted (WCR) bootstrap with Rademacher and with Webb (2023) six-point weights. It also reports the WCR bootstrap confidence interval, obtained by inverting the bootstrap test, and the effective number of clusters $G^*(0)$ of Carter et al. (2017). The conclusions in the text hold under every method. The only estimate whose significance depends on the method is general transfers received, which is significant at the 10% level under CV1 and at the 5% level under CV3 and the Webb bootstrap.

Table 25 reports the 2SLS estimates with CR2 standard errors (Pustejovsky and Tipton, 2018) and Satterthwaite degrees of freedom, and the WRE bootstrap with both weight distributions and both symmetric and equal-tail p-values. The CR2 p-values sit close to the CV1 p-values in Table 6. The WRE p-values barely move across weight distributions or between symmetric and equal-tail versions.

The table also reports the cluster jackknife for the 2SLS estimates, which re-estimates the full 2SLS procedure with each sub-location removed. Because 2SLS is biased in finite samples, I centre the jackknife variance both on the full-sample estimate and on the mean of the delete-one-cluster estimates; a noticeable gap between the two would signal non-trivial bias. The two standard errors agree to the third decimal for every outcome, and for livestock borrowing they equal 0.421 and 0.420. The jackknife bias estimate for livestock borrowing is 0.069, about a sixth of its standard error, and the bias-corrected estimate is -0.795 ($p = 0.078$), slightly larger in magnitude than the uncorrected -0.726. The jackknife p-value for the uncorrected estimate, 0.105, sits just above the CV1 p-value of 0.090. The other 2SLS outcomes in Tables 12 and 4 show the same pattern: the two jackknife standard errors coincide, and the bias estimates are small relative to the standard errors.

Table 26 reports cluster-level leverage L_g, partial leverage $L_{g\beta}$ for the reduced-form coefficient on SPEI $\times$ any coupon, and the delete-one-cluster estimates for the reduced form and for the 2SLS interaction (MacKinnon et al., 2023). With 16 clusters and five regressors after partialling out the fixed effects, average leverage is $5/16 = 0.31$ and average partial leverage is $1/16 = 0.06$. Kalacha and Loiyangalani stand out on both measures. Removing Loiyangalani moves both estimates furthest toward zero, and removing Kalacha has a similar effect on the 2SLS estimate. Removing Dirib Gombo moves the 2SLS estimate the other way, to -0.94. No single sub-location reverses the sign of either estimate.

Table 24: Inference with a Small Number of Clusters, Reduced Form

Outcome	Coef.	CV1		CV3		WCR bootstrap		95% CI, WCR
		SE	p	SE	p	p, Rad.	p, Webb	
Livestock borrowed	−0.073	0.028	0.020	0.029	0.023	0.027	0.024	[−0.126, −0.011]
Livestock lent	−0.002	0.023	0.934	0.021	0.929	0.922	0.923	[−0.052, 0.037]
Livestock gifts received	−0.025	0.053	0.649	0.049	0.621	0.601	0.605	[−0.122, 0.088]
Drought herd movement	−0.042	0.029	0.168	0.028	0.156	0.159	0.156	[−0.096, 0.023]
Animals borrowed	−0.316	0.126	0.024	0.114	0.014	0.019	0.018	[−0.532, −0.072]
General transfer received	−0.122	0.063	0.074	0.056	0.045	0.051	0.048	[−0.260, 0.000]
General transfer given	−0.084	0.036	0.035	0.034	0.028	0.030	0.027	[−0.150, −0.010]
Any informal support received	−0.117	0.045	0.020	0.043	0.017	0.025	0.022	[−0.207, −0.022]
Any informal support given	−0.089	0.040	0.042	0.037	0.028	0.040	0.037	[−0.160, −0.006]

Notes. Each row reports the coefficient on SPEI × any coupon from the reduced form in Table 3, with 3,652 observations and 16 sub-location clusters. CV1 and CV3 p-values use $t(15)$ critical values. The CV3 standard error is the cluster jackknife, computed from the 16 delete-one-cluster estimates with the household and round fixed effects re-estimated each time. The WCR bootstrap imposes the null hypothesis and uses symmetric p-values. With Rademacher weights, all $2^{16} = 65{,}536$ weight vectors are enumerated; with Webb (2023) six-point weights, $B = 99{,}999$. The confidence interval inverts the Rademacher bootstrap test. $G^*(0) = 10.5$ for every row, since all rows share the same regressors. Adding the coupon level to the reduced form, so that it matches the first stage exactly, gives −0.081 for livestock borrowing (CV3 $p = 0.023$, WCR $p = 0.020$) and −0.105 for general transfers received (CV3 $p = 0.099$, WCR $p = 0.117$).

Table 25: Inference with a Small Number of Clusters, 2SLS

	Loan In	Loan Out	Gift In (Placebo)	Drought Herd
IBLI × SPEI	−0.726	0.055	−0.600	−0.441
CR2, Satterthwaite degrees of freedom				
Standard error	0.388	0.204	0.516	0.339
p-value	0.092	0.793	0.273	0.224
Cluster jackknife (CV3), $t(15)$				
Standard error, centred on $\hat{\beta}$	0.421	0.196	0.474	0.361
p-value	0.105	0.783	0.225	0.240
Standard error, centred on $\bar{\beta}$	0.420	0.196	0.474	0.361
p-value	0.105	0.783	0.225	0.240
Jackknife bias estimate	0.069	−0.001	0.055	0.010
Bias-corrected estimate	−0.795	0.056	−0.655	−0.452
p-value, bias-corrected	0.078	0.780	0.187	0.230
WRE bootstrap, Rademacher weights (all 2^{16} draws)				
p-value, symmetric	0.016	0.780	0.243	0.149
p-value, equal-tail	0.021	0.789	0.222	0.125
WRE bootstrap, Webb weights ($B = 9{,}999$)				
p-value, symmetric	0.016	0.777	0.236	0.144
p-value, equal-tail	0.021	0.793	0.208	0.120
Observations	3,652	3,652	3,652	3,652

Notes. Specifications follow Panel B of Table 3. CR2 standard errors follow Pustejovsky and Tipton (2018); the Satterthwaite degrees of freedom are 9.6 for all four outcomes. The cluster jackknife re-estimates the full 2SLS model, including the first stage and the fixed effects, with each sub-location removed in turn. $\hat{\beta}$ is the full-sample estimate and $\bar{\beta}$ the mean of the 16 delete-one-cluster estimates $\hat{\beta}^{(g)}$; the CV3 variance is $\frac{G-1}{G}\sum_g(\hat{\beta}^{(g)}-c)^2$ with $c=\hat{\beta}$ or $c=\bar{\beta}$. The jackknife bias estimate is $(G-1)(\bar{\beta}-\hat{\beta})$, the bias-corrected estimate is $G\hat{\beta}-(G-1)\bar{\beta}$, and its p-value uses the standard error centred on $\bar{\beta}$. The WRE bootstrap is the wild restricted efficient bootstrap of Davidson and MacKinnon (2010) at the sub-location level. It tests the null hypothesis that the IBLI × SPEI coefficient is zero, keeps the IBLI level term endogenous, and applies the same cluster weight to the structural and first-stage residuals.

Table 26: Leverage and Delete-One-Cluster Estimates, Livestock Borrowing

				Estimate without this sub-location	
Sub-location	N_g	L_g	$L_{g\beta}$	Reduced form	2SLS
Kalacha	414	0.682	0.189	−0.071	−0.511
Loiyangalani	395	0.889	0.150	−0.052	−0.454
Dirib Gombo	382	0.390	0.092	−0.081	−0.940
El Gade	185	0.283	0.080	−0.065	−0.789
Turbi	172	0.265	0.070	−0.076	−0.785
Bubisa	300	0.324	0.060	−0.074	−0.694
Dakabaricha	156	0.211	0.048	−0.077	−0.750
Logo Logo	182	0.206	0.047	−0.070	−0.682
Illaut	147	0.187	0.041	−0.077	−0.737
Kargi	384	0.459	0.040	−0.072	−0.733
Kurkum	128	0.162	0.036	−0.086	−0.750
Lontolio	122	0.140	0.034	−0.067	−0.710
Karare	216	0.296	0.033	−0.072	−0.680
South Horr	183	0.233	0.029	−0.081	−0.752
Ngurunit	167	0.172	0.028	−0.072	−0.808
Sagante	119	0.102	0.024	−0.075	−0.764
Full sample	3,652			−0.073	−0.726

Notes. N_g is the number of household-round observations in the sub-location. L_g is cluster leverage, the trace of the gth diagonal block of the hat matrix, and $L_{g\beta}$ is partial leverage for the coefficient on SPEI × any coupon, both computed after partialling out household and round fixed effects (MacKinnon et al., 2023). Average leverage is $5/16 = 0.31$ and average partial leverage is $1/16 = 0.06$. Rows are sorted by partial leverage. The last two columns re-estimate the reduced form and the 2SLS model with the sub-location removed and the fixed effects re-estimated.

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