Does Index Insurance Crowd In or Crowd Out Informal Risk Sharing? Evidence from Rural Ethiopia

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Abstract: We study how the introduction of a formal index insurance product affects informal risk sharing among pastoralists in southern Ethiopia. Using detailed social networks data, randomized incentives to purchase the insurance product, and hypothetical informal transfer data that mirror the existing customary arrangements, we find respondents' own formal insurance uptake has no significant effect on their willingness to share risk through customary institutions. We also find weak evidence that a randomly matched peer's insurance uptake positively influences respondents' willingness to make informal transfers to that match. Overall, our results imply that in this context index insurance does not crowd out informal risk sharing mediated by social networks.

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A suggested running head: Index insurance and informal risk sharing

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Evidence from Rural Ethiopia

Weather risks threaten the welfare of rural populations in poor agrarian economies. Interventions to address those threats therefore attract significant attention. Uninsured weather risk arises in part because most rural households in low-income economies lack access to conventional agricultural insurance due to market failures associated with asymmetric information, such as moral hazard and adverse selection, as well as high transaction costs for monitoring and state verification (Barnett, Barrett, and Skees 2008). Informal risk sharing arrangements based on risk pooling within social networks commonly fill part of the void left by formal financial markets failures (Townsend 1994; Besley 1995). But informal arrangements are typically best suited to managing idiosyncratic (i.e., household-specific) rather than covariate risk due to weather shocks.

A recent innovation in weather risk management, index insurance, aims to fill that gap. Index insurance indemnifies the losses predicted by objective measures strongly correlated with covariate shocks rather than the actual (and potentially idiosyncratic) losses experienced by policyholders. Because the insured's type and actions do not matter to payouts and individual loss verification is unnecessary, index-based products obviate key problems inherent to conventional agricultural insurance. As a result, index insurance has become popular over the past decade or so (Miranda and Farrin 2012; Smith 2016). Yet index insurance uptake rates remain low across many contexts in which it has been introduced (Giné, Townsend, and Vicker 2008; Cole et al. 2013), in part because of basis risk, i.e., the difference between the losses actually incurred and the losses insured based on index values (Miranda and Farrin 2012; Mobarak and Rosenzweig 2012; Elabed et al. 2013; Dercon et al. 2014; Karlan et al. 2014; Clarke 2016; Jensen, Mude, and Barrett 2018).

Basis risk can, however, potentially help reinforce the relationships between the demand for index insurance and participation in informal risk sharing, although theory yields ambiguous predictions. Dercon et al. (2014), Mobarak and Rosenzweig (2012, 2013), and Berg, Blake, and Morsink (2017) all highlight the technological complementarity between index insurance and informal risk sharing. Covariate weather risk that affects all network members can be insured by index insurance, while the residual basis risk can be insured by informal transfers. All else held constant, such complementarity should increase an informally insured individual's willingness to pay for index insurance while index insurance uptake should likewise reinforce informal risk pooling arrangements.

On the other hand, like conventional indemnity insurance (Arnott and Stiglitz 1991; Attansio and Ríos-Rull 2000; Lin, Liu and Meng 2014; Lenel and Steiner 2017; Strupat and Klohn 2018), index insurance could dampen demand for informal insurance and *vice versa*. For example, if an individual's utility depends in part on the aggregate wealth of one's network or if insurance is a public good under a joint liability scheme, then individual insurance uptake generates positive externalities and potentially a free-riding problem (de Janvry, Dequiedt, and Sadoulet 2014; Janssens and Kramer 2016). Similar effects arise if social norms compel socially connected individuals to share any insurance indemnity payment with uninsured peers in the event of a covariate weather shock (Munro 2015), or if index insurance encourages excessive risk-taking by reducing the marginal cost of risky assets or activities, thereby imposing external costs on network members

(Boucher and Delpierre 2014; Vasilaky et al. 2014). These mechanisms might cause index insurance uptake to fray the social fabric underpinning informal risk sharing arrangements.

So which effect, if either, dominates? This matters because index insurance products are not introduced into a risk management void; informal risk management arrangements are ubiquitous. Therefore, the net additional insurance coverage generated from index insurance uptake depends fundamentally on whether it crowds in or out or has no effect on pre-existing informal risk sharing arrangements. Studies of the relationship between index insurance and informal risk sharing arrangements to date have focused on the effects of social networks on index insurance uptake, via social learning, imitation or scale effects (Mobarak and Rosenzweig 2012; Trærup 2012; Cai, de Janvry, and Sadoulet 2015). Little is known about effects in the opposite direction: how index insurance uptake affects customary risk pooling arrangements mediated by social networks.

This article helps fill that void, by studying empirically the relationship between index insurance demand and informal risk sharing arrangements using unique experimental data collected among pastoralist communities in southern Ethiopia. The study area has experienced recurrent droughts every six or seven years, on average, since the mid-1970s, each causing widespread livestock mortality (Desta and Coppock 2004; Megeresa et al. 2014). Longstanding customary informal arrangements exist in the study area, most notably *dabare*, under which one household lends livestock to another on a temporary basis with the understanding that roles may reverse in the future. This informal reciprocal exchange is a credit-insurance contract exclusively used in the wake of an adverse shock. Because *dabare* transfers involve no interest payments and there is nontrivial risk that the giver will not receive a reciprocal transfer in a future period, this institution is best understood as informal insurance within the standard framework of economic theory. Many observers, however, indicate that *dabare* and other informal risk management institutions have been eroding over time (Lybbert et al. 2004; Huysentruyt, Barrett, and McPeak 2009; Santos and Barrett 2011; Hurst et al. 2012).

In an effort to help protect pastoralists' livelihoods against uninsured drought risk, a commercial index-based livestock insurance (IBLI) product was introduced in August 2012 by Oromia Insurance Company. A key concern in introducing IBLI was whether it would buttress or undermine informal risk sharing institutions such as *dabare*. The major objective of this study is to identify the impacts of one's and one's peers' IBLI uptake on the informal risk sharing links that underpin *dabare* transfers, in particular to test the hypothesis that IBLI crowds out informal risk management mechanisms.

There are at least four fundamental challenges in empirical research on this topic. First, index insurance uptake is subject to non-random selection, so one needs a credible identification strategy to establish a causal effect of formal insurance on informal risk sharing. Second, network formation is highly likely endogenous to unobservables that influence decisions to purchase IBLI and *vice versa*, which makes it difficult to establish causal relationships between the two. Third, even if endogeneity issues can be resolved, correctly identifying one's social network remains a tricky task. Finally, the state-dependent transfers that characterize informal insurance arrangements might not be triggered by events during the survey period, leading to attenuation bias arising from not observing transfers that would have occurred in unobserved states of nature (Dizon, Gon, and Jones 2015).

To address these four concerns, we employ a novel empirical strategy that combines randomized encouragement designs that provide a solid instrument for formal index insurance uptake with a random-matching-within-sample method to identify social networks and questions about hypothetical inter-household transfers otherwise unobservable during the survey period. More precisely, we randomly distributed discount coupons to generate exogenous price variation for IBLI uptake. The randomized coupon discount rate may provide a strong instrument that lets us identify the causal impact of a respondent's insurance uptake on inter-household transfer behaviors. Then, using best current practices, we elicited each respondent's network structure by matching him or her with eight other survey respondents randomly drawn from the sample, thereby avoiding bias in elicitation of the respondent's social network structure (Santos and Barrett 2008; Conley and Udry 2010; Maertens and Barrett 2013). For each match, respondents were asked about his or her willingness to make an informal transfer (*dabare*) to the match. This method obviates the attenuation bias inherent to using only actual transfers.

Our primary findings show no evidence of crowding out of existing informal risk sharing networks; those who purchase IBLI are no less likely to provide *dabare* transfers to their network members than are those who do not purchase IBLI. Nor is there any evidence of a free-riding problem wherein a respondent is less likely to buy IBLI when peers buy IBLI. Results from several estimations suggest that when random matches purchase IBLI, respondents become more likely to provide *dabare* transfers to that individual. Since *dabare* is a hybrid credit-insurance arrangement, such behavior may partly reflect a match's creditworthiness.[1] Controlling for within-sample variation in the average perceived creditworthiness of matches does not change the result, however. Thus, the result seems more consistent with a mechanism wherein index insurance crowds in informal risk sharing arrangements. While a range of specifications and estimators replicate this same effect, the crowding-in result does not stand up to all robustness checks.

So our evidence on the existence of crowding-in effects seems tenuous. Taken together, we conclude that formal index insurance does not compromise pre-existing informal social arrangements, and, if anything, seems to reinforce them in our setting.

This article contributes to the literature on the nexus between formal index insurance and informal risk sharing arrangements. While a handful of recent studies ask similar questions, they infer the relationships between the two only indirectly by examining whether the demand for index insurance is greater if it is sold to groups rather than to individuals (Vasilaky et al. 2014) or whether insurance uptake increases at the group level if the technological complementarity between index insurance and informal arrangements is explained to prospective purchasers (Dercon et al. 2014). Our study is closest to Mobarak and Rosenzweig (2012), Berg, Blake, and Morink (2017) and Munro (2015). Their results are, however, mixed. Mobarak and Rosenzweig (2012) and Berg, Blake, and Morink (2017) show evidence supporting the existence of technological complementarity (i.e., increased insurance demand among those engaged in informal arrangements) in observational data from rural India and in a lab experiment in Ethiopia, respectively, whereas Munro's (2015) evidence supports the free-riding hypothesis (i.e., the reduced insurance demand when subjects are allowed informal transfer) in lab experimental settings in India. These three studies focus on the effect of informal risk sharing arrangements on index insurance uptake, rather than the causal effect of index insurance on informal insurance. To the best of our knowledge, this study is the first to provide rigorous evidence that formal index insurance products do not degrade pre-existing informal arrangements.

Study Design and Summary Statistics

Study area, IBLI, and a quasi-experiment

The study took place on the Borana plateau in Oromia region in southern Ethiopia. Borana is an arid-to-semi-arid ecological zone characterized by a bimodal rainfall pattern broken into four seasons: a long rainy season (March to May), a long dry season (June to September), a short rainy season (October to November), and a short dry season (December to February). The vast majority of the population is pastoralists whose livelihoods depend primarily on extensive livestock grazing. They mainly herd cattle, and to a lesser extent goats, sheep and camels (Desta and Coppock 2004). Semi-nomadic or transhumant pastoralism from permanent settlements to neighboring communities is common in search of pasture and water in the face of seasonal forage and water scarcity. These pastoralists are overwhelmingly poor and extremely vulnerable to weather shocks. Recurrent catastrophic droughts have occurred regularly since the 1970s (i.e., 1973/74, 1983/84, 1991/92, 1999/00, 2005/06, 2011/12). Widespread drought-related livestock mortality has pushed pastoralists into poverty traps (Lybbert et al. 2004; Santos and Barrett 2011, 2018; Megersa et al. 2014; Barrett and Santos 2014).

IBLI was introduced by a consortium led by the International Livestock Research Institute (ILRI) in collaboration with the Oromia Insurance Company (OIC) in August 2012. The design of IBLI followed a successful pilot project in northern Kenya launched in 2010 (Chantarat et al. 2013). IBLI uses the standardized Normalized Differenced Vegetation Index (NDVI) – based on satellite imagery to measure rangeland conditions – as an index. Sales of IBLI occur twice a year directly preceding long and short rainy seasons (i.e., January-February and August-September). Contracts cover one full year, i.e., two rainy-dry season pairs. Pastoralists choose the number and species of animal to insure. Insurance premiums vary across animal species and geographic regions according to actuarial estimates of drought-related mortality risk. Indemnity payouts are triggered when the index falls below the 15th percentile of the historical (since 1981) index distribution. Once triggered, the amount of indemnity payouts depends on the realized NDVI and total herd values insured (Ikegami and Sheahan 2015). Since the first sales in Ethiopia in 2012, IBLI had been sold six times by the time of the data we use were collected; indemnity payouts occurred once, in October 2014. Uptake rates ranged from 12% to 30% per year in the sample.

To stimulate IBLI uptake and generate exogenous variation in the effective price faced by prospective IBLI purchasers, an experimental design was employed in each sales period. Discount coupons were distributed to randomly selected sub-samples of households, allowing them to purchase IBLI at a premium discount for up to 15 Tropical Livestock Units (TLUs)[2] insured. Rate discounts (hereafter, encouragement rates) ranged from 10 to 80 percent. Since randomization was independently implemented in each sales period, coupon recipients and realized encouragement rates changed across the sample households over time, generating exogenous, intertemporal, within-respondent variation. In each period, a randomly selected twenty percent of sample households did not receive a coupon.[3] Takahashi et al. (2016) examine factors affecting IBLI uptake in our study area and found that the distribution of discount coupons significantly increases uptake of IBLI. Thus, randomized receipt of a discount coupon serves nicely as an instrument for IBLI uptake.

Appendix table 1 shows the balance test of household characteristics prior to the coupon distribution in the latest two sales periods. While imbalance is found in several variables, such as the household head's age and educational attainment, these variables are not jointly statistically significantly different between discount coupon recipients and non-recipients, indicating that the randomization worked well.

Jensen, Mude, and Barrett (2018) demonstrate that basis risk substantially reduces demand for IBLI in neighboring northern Kenya. Basis risk might, however, be at least partially mitigated by informal risk sharing. In Borana, two main types of informal arrangements exist: *busa gonofa* and *dabare*. *Busa gonofa* is a gift of animals from the rich to the needy, a semi-compulsory restocking scheme with animals redistributed solely within the same lineage (sub-clan), acting more like a mandatory kinship tax than a voluntary risk pooling arrangement (Berhanu 2011). By contrast, *dabare* is a loan of cattle transferred voluntarily through clan (lineage) networks in response to a negative shock. Since contract enforcement is relatively easy for within-clan members, *dabare* partners are not necessarily direct acquaintances as long as both belong to the same clan, although it generally operates within closer relationships, such as friends and relatives (Tadese 2010). We therefore focus on whether the voluntary informal risk sharing arrangement *dabare* is affected by the introduction of IBLI.

Sampling strategy and survey methods

We surveyed 17 study sites (composed of one to three *reeras*, local administrative units, each containing 100-300 households) in eight *woredas* (local administrative units that encompass *reeras*) in Borana: Dilo, Teltele, Yabello, Dire, Arero, Dhas, Miyo, and Moyale. These study sites were selected to maximize geographic distribution and capture agro-ecological and livelihood variation. Sample households in each selected study site were randomly chosen from the population list, prepared by local government Development Agents (DAs) who supported the field work.

The first round of the household survey was implemented among 515 households in

March 2012, prior to the announcement of IBLI and the first sales period in August 2012. Thereafter, the follow-up surveys were conducted every March until 2015, for a total of four annual surveys. To maintain the sample size of around 500, attrited households were replaced by other households from the same study site that have similar TLU holdings as the attrited households. The attrition rate is low, however, only around 2% each round. Each survey round asked detailed questions about household characteristics, composition, activities, livestock holdings, income-generating activities, durable and non-durable assets, knowledge and experience of IBLI, and risk preferences (Ikegami and Sheahan 2015).

Social networks

The literature proposes several methods to capture informal social arrangements. Perhaps, the simplest and most popular method is to ask about actual inter-household monetary transfers and other informal exchanges. A drawback of this method is that we typically do not know the attributes of each household with which a respondent engages in transfers, as most inevitably fall outside the sample. Moreover, actual sharing may be observed only if negative shocks occur during the period covered by the survey questions, leading to underestimation of the extent of the true network (Dizon, Gong, and Jones 2015).

Another common strategy is to ask each respondent about his/her informal link to every other household in the sample. However, using Monte Carlo simulation, Santos and Barrett (2008) demonstrate that this "network within sample" method less reliably recovers the underlying social network structure than does the "random matching within sample" method pioneered by Conley and Udry (2010), in which the sample respondents are randomly matched with other selected individuals in a sample.[4]

This study employs the random-matching-within-sample method to elicit respondents' informal networks. More specifically, a new questionnaire module was added in the March 2015 survey round in which we assigned each respondent to eight households randomly drawn from the sample, and asked (1) whether the respondent knows the match, and (2) whether the respondent would be willing to transfer one or more cattle to the match if requested after the match suffers an adverse shock (replicating the *dabare* institution). To reduce recall and reporting errors as much as possible, we provided respondents with the match's information, such as age, clan, and residential location. We note that the second question above is hypothetical, which can overcome attenuation bias inherent to actual transfers data, but may not necessarily reflect respondent's actual behaviors. We expect that reporting bias would not be a serious problem in our study, however, as we encouraged respondents to carefully consider the context of asset transactions that have long prevailed in their society. Moreover, Santos and Barrett (2008) show that inferred insurance network derived from this approach closely matches actual network behavior among a different sample of Boran pastoralists from the same region.[5] That said, we do a range of robustness checks in empirical analysis.

The existing literature points out that the costs and benefits of informal risk sharing vary by geographic and social distance (Fafchamps and Gubert 2007). Although the recent diffusion of mobile phones might lower transaction costs and has facilitated long-distance risk sharing in several settings (Jack and Suri 2014; Blumenstock, Eagle, and Fafchamps 2016; Munyegera and Matsumoto 2016), whether physical distance is positively associated with informal arrangements remains an empirical question. In order to examine the role geographical distance might play, we randomly selected five matches among respondents in the same community and the remaining three matches from relatively far away, outside the community but within a 40-50 km radius of the respondent's permanent residence. We used GPS coordinates to identify those non-neighbors. The 40-50 km distance was chosen to be sufficiently far that most income risks are expected to be uncorrelated, but not so far that there is little chance to meet and form links. Since pastoralists are mobile and commonly trek dozens, if not hundreds, of kilometers (Liao et al., 2017), these geographical boundaries offer a coarse means of exploring how informal risk sharing is facilitated or constrained by geographic distance.

The literature also shows the importance of social distance to network links. For example, Attanasio et al. (2012) and Chandrasekhar, Kinnan, and Larreguy (forthcoming) find that socially close pairs tend to cooperate even without explicit enforcement, while distant pairs do not. In contrast, Vasilaky et al. (2014) find that when individuals are socially closer, they are less likely to collectively buy index insurance because of negative externality as suggested by Boucher and Delpierre (2014). These contradicting findings suggest that social distance play a different role in each specific context. In our setting, a particularly important factor might be kinship as Santos and Barrett (2011) show that the propensity to lend cattle is strongly and positively influenced by belonging to the same clan.

Summary statistics

Table 1 presents summary statistics of sample households in the 2015 survey. The sample is 513 households, mainly ethnic Borana, with a few Guji and Gabra. On average, households are large (6.9 persons), headed by a male with minimal or no formal education, poor – mean monthly household consumption per capita is about 300 birr[6] – and depend on livestock, including milk and meat production, for more than 80 percent of total household income. Livestock also comprise these households' main non-human asset, with average holdings of 18.9 TLU, dominated by cattle and supplemented with goats, sheep, and camels.

The uptake rate of IBLI in the August-September 2014 sales period was about 20 percent, which fell to 12 percent in January-February sales period in 2015, following a recurring pattern of lower uptake in the January-February sales window (Takahashi et al. 2016). Social networks are active; respondents express a willingness to transfer cattle to 3.8 out of 8 randomly selected matches within the sample, 3.1 of whom come from inside their village.

We elicited household's risk preference via an ordered lottery selection following Binswanger (1980). Each respondent was offered a chance to choose one of the six lotteries with payouts in birr of (50, 50), (45, 95), (40, 120), (30, 150), (10, 190), and (0,200) implemented with coin flip and real cash payouts. We define a respondent as highly risk averse if he/she chose either of the first two options, moderately risk averse if he/she chose either of the middle two options, and less risk averse if he/she chose one of the last two. About 12, 46, and 42 percent of respondents belong to the first, second, and third categories, respectively.

Table 2 cross tabulates observations between key variables of interest. Panel A shows that knowing the match is strongly positively associated with willingness to make an informal transfer. In 349 cases, a respondent is willing to transfer cattle to someone they claimed not to know. This could be surprising, but recognition of lineage names may suffice for some respondents to be *dabare* partners as long as the informal contract is enforceable through broader clan networks. We notice, however, 50 cases where a respondent shows willingness to make *dabare* transfers to unknown non-clan members, which would raise a concern that we may need to interpret the results with some caution.

As expected, respondents are far more likely to know (Panel B) and be willing to make transfers to matches from their own instead of a geographically distant community (Panel C). There are only 92 cases where a respondent knows a match from far away, even in this highly geographically mobile society. Detailed data examination reveals that the least reliable answers above (i.e., willing to make a transfer to unknown non-clan members) relate to those from distant communities (44 cases) rather than neighbors within their own community (6 cases).

Econometric Analysis

Benchmark empirical model

We now describe how we study the relationship between IBLI uptake and informal transfers econometrically. Let us define L_{ij} as equal to one if respondent household i is willing to transfer one or more cattle to randomly matched household ('match' or 'peer') j in times of need, and zero if not.[7] This link is not necessarily bidirectional, meaning $L_{ij} = L_{ji}$ need not hold, although *dabare* is generally recognized as a reciprocal institution. That is, if a respondent i is willing to transfer cattle to the match j, we consider that the informal link from i to j is established, regardless of whether i thinks it would receive a similar transfer from j nor whether j indicates a willingness to transfer to i in times of need.

Let D_i and D_j equal one if i and j purchase IBLI, respectively, and zero otherwise. We ultimately want to examine the causal effects of D_j and D_i on L_{ij} in order to identify the positive or negative effects of IBLI uptake on informal insurance, specifically *dabare*-type risk sharing arrangements in this setting. Since it takes time for information on a peer's IBLI uptake to spread, we use the lagged value of D_j (i.e., contract purchase six months earlier, in August-September 2014, and thus a contract still in force) to represent i's information in the March 2015 survey on j's IBLI uptake. The respondent's uptake, D_i, is captured by January-February 2015 actual purchase, so there is a clear temporal sequencing of the IBLI demand and informal insurance link variables.

Letting a superscript represent the timing of purchase for D_j, the benchmark dyadic model we estimate is specified as:

(1) $L_{ij} = \alpha_1 + \rho_1 D_i + \delta_1 D_j^{AS14} + \beta_1(X_i + X_j) + \tau_1(X_i - X_j) + \pi_1 W_{ij} + \varphi_1 + \omega_1 + u_{1ij}$

where X_i and X_j denote a vector of controls for household i and j characteristics, respectively; W_{ij} describes the attributes of the link between i and j (on which, more below), φ_1 and ω_1 are the study site fixed effects for household i and j, respectively, and u_{1ij} is the unobserved mean zero, normally distributed error term.

The vector X includes basic household characteristics, such as household head's gender, age, and completed years of education, log household per capita expenditure, TLU owned, and risk preference dummies elicited in the survey. We use the baseline (March 2012) values of X to minimize potential endogeneity concerns.[8,9] W_{ij} includes a dummy variable equal to one if i personally knows j, a dummy variable equal to one if j is relative,[10] which captures social proximity, and the physical distance (in kilometers and squared kilometers) between the permanent settlements of i and j.[11] Since L_{ij} can be directional, we do not impose a symmetric restriction of $\beta X_{ij} = \beta X_{ji}$, where $|X_i - X_j|$ and $|W_{ij}|$ should be used instead of $(X_i - X_j)$ and W_{ij} (Fafchamps and Gubert, 2007).[12] Following Attanasio et al. (2012), standard errors are clustered at the study site level to allow for possible correlations not only within dyadic pairs, but also across all dyads in the same study site.

The key parameters of interest are ρ_1 and δ_1. Rejecting the zero null hypothesis in favor of $\rho_1 > 0$ indicates that *i*'s IBLI purchase increases his/her willingness to transfer cattle to the match; $\delta_1 > 0$ indicates that match *j*'s insurance uptake induces more informal transfers from *i* to *j*. Statistically significant positive estimates for either or both parameters would be consistent with crowding-in effects of IBLI uptake on informal risk sharing arrangements. One important caveat is that $\delta_1 > 0$ could arise as well if *i* wishes to free-ride on *j*'s uptake of IBLI, as will be discussed in more detail below.

Identification strategy

Equation (1) above can be estimated by OLS, but parameter estimates will be biased and inconsistent due to the endogeneity of IBLI uptake. We therefore employ an instrumental variable (IV) estimation strategy using the random discount coupon rate of *i*'s insurance premium for the January-February 2015 sales period as an instrument for D_i, along with the same set of control variables as in equation (1). The set of estimated equations can then be rewritten as:

$$(2)\ D_{ij} = \alpha_2 + \gamma_2 Z_i + \delta_2 D_j^{AS14} + \beta_2(X_i + X_j) + \tau_2(X_i - X_j) + \pi_2 W_{ij} + \varphi_2 + \omega_2 + u_{2ij}$$

$$(3)\ L_{ij} = \alpha_3 + \rho_3 \widehat{D}_{ij} + \delta_3 D_j^{AS14} + \beta_3(X_i + X_j) + \tau_3(X_i - X_j) + \pi_3 W_{ij} + \varphi_3 + \omega_3 + u_{3ij}$$

where Z_i represents *i*'s randomly assigned coupon encouragement rate, which is, by design, orthogonal to the unobserved error terms in equations (2) and (3) and should have no independent relationship to L_{ij}. $\widehat{D}_{ij}$ is then the respondent's predicted IBLI uptake (instrumented with Z_i) based on the parameter estimates from equation (2). In equation (2) we allow *j*'s uptake of IBLI in the August-September 2014 sales period, D_j^{AS14}, to potentially influence *i*'s uptake in the subsequent, January-February 2015, sales period, D_{ij}. This could reflect social learning, imitation, or omitted relevant variables that are correlated within the network and over time.

While the IV strategy represented by equations (2) and (3) might allow us to properly identify a causal impact of i's own IBLI purchase on L_{ij}, the endogenous variable D_{ij} undesirably varies at the ij level in this specification because of the inclusion of j's characteristics in the first stage regression.

We therefore also use the conditional mixed process (CMP) estimator, proposed by Roodman (2011), suitable for a large family of multi-equation systems in which the dependent variable of each equation may have a different format (Asfaw, Battista, and Lipper, 2016). More specifically, we estimate the following set of recursive equations:

(4) $D_i = \alpha_4 + \gamma_4 Z_i + \beta_4 X_i + \varphi_4 + u_{4i}$

(5) $L_{ij} = \alpha_5 + \rho_5 \widehat{D}_i + \delta_5 D_j^{AS14} + \beta_5(X_i + X_j) + \tau_5(X_i - X_j) + \pi_5 W_{ij} + \varphi_5 + \omega_5 + u_{5ij}$.

So far, we have assumed that D_j^{AS14} is exogenous. Because D_j^{AS14} is pre-determined to i's transfer decision and because it is not based on i's endogenous real social network, but derived instead from randomly-assigned network, the match's D_j^{AS14} would be less likely to be correlated with unobservables in the main equation (5). Endogeneity concerns regarding D_j^{AS14} would nonetheless arise because of reflection problems (Manski 1993), i.e., neighbors behave similarly simply because they have similar characteristics or face a similar institutional environment. If this is the case, then unobserved factors could cause spurious correlation between insurance uptake and informal risk sharing. Although standard linear-in-means models generally suffer identification problems to distinguish real social effects from unobservable correlated effects, our estimation exploits the advantage of dyad regressions where we effectively control for exogeneous and correlated social effects by including both respondent's and match's exogeneous characteristics and study site fixed effects. This is akin to an extended version of the linear-in-means model proposed by Bramoullé, Djebbari, and Fortin (2009), which includes the mean of the outcome and characteristics of one's social network to identify endogenous social effects. Thus, our estimation strategy avoids the network-scale correlated effects problems common to this literature.

We nonetheless test whether the results are altered by different specifications and estimators, also treating both D_i and D_j^{AS14} as endogenous using both IV and CMP estimators. The encouragement rate of j's insurance premium in the August-September 2014 sales period (Z_j^{AS14}) is used as an additional instrument. The set of equations for the two endogenous variables IV estimation is specified as:

(6) $D_{ji}^{AS14}=\alpha_6 + \gamma_6 Z_i + \vartheta_6 Z_j^{AS14} + \beta_6(X_i + X_j) + \tau_6(X_i - X_j) + \pi_6 W_{ij} + \varphi_6 + \omega_6 + u_{6ij}$

(7) $D_{ji} = \alpha_7 + \gamma_7 Z_i + \vartheta_7 Z_j^{AS14} + \beta_7(X_i + X_j) + \tau_7(X_i - X_j) + \pi_7 W_{ij} + \varphi_7 + \omega_7 + u_{7j}$

(8) $L_{ij}=\alpha_8 + \rho_8 \widehat{D}_{ij} + \delta_8 \widehat{D}_{ji}^{AS14} + \beta_8(X_i + X_j) + \tau_8(X_i - X_j) + \pi_8 W_{ij} + \varphi_8 + \omega_8 + u_{8ij}$

Similar to the previous regressions, the endogenous variables D_i and D_j^{AS14} vary at the *ij* level. In order to suppress that variation, we also estimate a more parsimonious CMP with instrumental variables as:

(9) $D_i = \alpha_9 + \gamma_9 Z_i + \beta_9 X_i + \varphi_9 + u_{9i}$

(10) $D_j^{AS14} = \alpha_{10} + \vartheta_{10} Z_j^{AS14} + \beta_{10} X_j + \omega_{10} + u_{10j}$

(11) $L_{ij} = \alpha_{11} + \rho_{11}\widehat{D}_i + \delta_{11}\widehat{D}_j^{AS14} + \beta_{11}(X_i + X_j) + \tau_{11}(X_i - X_j) + \pi_{11} W_{ij} + \varphi_{11} + \omega_{11} + u_{11ij}$

Note that equations (9) to (11) are estimated jointly, using the full set of observations. In total, we have 8 peers for each of the 513 households, yielding 4104 observations in the main equation (11).[13] The predicted value of $\widehat{D}_i$ is the same regardless of when we use 513 or 4104 observations for equation (9) because each respondent household *i* appears exactly eight times in the latter specification. However, the predicted value of

D_j^{AS14} in equation (10) differs between the 513 and 4104 observations because random matching exogenously determines the number of times household j to be matched to a respondent *i* (from a minimum of 1 to a maximum of 19, with mean of 8). Thus, sample weighting adjustments are needed to correct for the random overweighting of certain households in equation (10).

Unfortunately, since only one likelihood is computed for each observation via CMP, it is infeasible to adjust weights only in the equation (10). Thus, we first estimate the CMP model with the full set of observations (i.e., ignoring weight adjustments) and then estimate equation (10) separately using each of the 513 households using predicted values in estimating equation (11). The results are qualitatively quite similar between these two methods. In what follows, therefore, we present only the unweighted CMP results, which involve standard error adjustments for generated regressors.

Empirical results

Table 3 shows the main linear probability model estimation results for the key variables of interest.[14] Column (1) corresponds to the result of the OLS estimator in equation (1), Columns (2) and (3) report the results of the IV and CMP estimators in equations (3) and (5), respectively, with only D_i treated as endogenous, and Columns (4) and (5) are for the result of the IV and CMP estimators with both D_i and D_j^{AS14} treated as endogenous, per equations (8) and (11).

Column (1) shows that match j's IBLI uptake is positively associated with respondent i's willingness to transfer. Columns (2) and (3) show qualitatively similar results. As reflected in column (2), the excluded instrument, the respondent's randomized coupon encouragement rate, is strongly correlated with the respondent's IBLI uptake, with a first-stage Kleibergen-Paap F-statistics of 198.2 (p=0.000).

Given that *dabare* transfer is a credit-insurance hybrid, the positive impact of a peer's uptake on a respondent's willingness to provide an informal transfer may partly reflect the creditworthiness of that peer. Yet we conjecture that such motivation may not be dominant because *dabare* is never extended to other credit purposes than *ex-post* risk mitigation and because it does not involve explicit interest in the contract. Furthermore, in an alternate specification below, we control for match fixed effects, which should remove any effects due to perceived creditworthiness. We therefore interpret this positive partial correlation as most likely reflecting either crowding-in or free-riding effects. If free-riding were a concern, however, those who purchase IBLI should limit their commitment to the informal *dabare* arrangement or even opt out altogether. We find no statistically significant effect of own IBLI uptake, however, although the negative (and insignificant) point estimate is similar in magnitude to the positive and significant coefficient estimate on j's IBLI uptake.

This result is consistent with related findings from the actual transaction data in the same study sites that show no correlation between one's IBLI uptake and interhousehold transfers (Bageant and Barrett 2017). Moreover, the dynamic interaction analysis that we implement below shows no negative impact of a peer's IBLI uptake on the respondent's subsequent uptake, further discrediting the free-riding hypothesis. We suspect that respondents were willing to make transfer to the insured peers because the insured are more likely to help respondents at the time of catastrophic covariate risk than the non-insured, all else held constant.

Once we treat D_j as endogenous, however, then the instrumented match's IBLI uptake becomes statistically insignificant and changes sign (Columns 4 and 5). Columns

(4) and (5) suffer from a weak instrument problem, however, as signaled by a first-stage Kleibergen-Paap F-statistic that is merely 3.47 for D_j, far below the Stock-Yogo weak ID test critical value of 7.03 at the 10% level.[15] Somewhat unexpectedly, the first-stage estimation result in supplementary appendix 1 reveals that match's randomized coupon encouragement rate does not induce significantly greater uptake in this particular sales period.[16]

Overall, these results suggest that formal insurance uptake, whether by the respondent or by a match within the respondents' network, does not crowd out customary risk sharing arrangements. Though not robust, there is some suggestive evidence that others' insurance uptake increases respondents' willingness to make informal transfers, i.e., some crowding in of informal insurance occurs as a result of formal index insurance uptake.[17]

Before moving on to robustness checks in the next section, other important findings in table 3 include the following. Informal transfers are considerably more likely among acquaintances, which is reasonable because cattle are such a valuable asset in the pastoralist community that a respondent would typically be unwilling to transfer to non-acquaintances. Informal transfers are also more likely among socially and geographically proximate households. While the coefficient estimate on distance is negative and significant, and that on its squared term is positive and significant, the relationship is merely declining at a diminishing rate, not U-shaped, as the minimum probability of the link formation falls outside of the sample coverage even for non-neighbor matched households. These results are consistent with the prior literature's findings that social and physical proximity are important for informal risk sharing, presumably because they decrease communication and transaction costs (Fafchamps and Gubert 2007; Chandrasekhar, Kinnan, and Larreguy forthcoming).

Robustness Checks

While we consistently found no evidence of crowding-out effects of IBLI uptake on informal insurance, we failed to establish robust, causal inference of crowding-in effects as the results vary depending on specifications and estimators. On the other hand, more structural estimation that treats peers' uptake, D_j, as endogenous suffers from a weak IV problem. We now explore the robustness of our findings through several alternative approaches to test the validity of the existence of crowding-in and the absence of crowding-out effects. A range of further robustness checks can be found in the supplementary appendix online.

Reflection problem

As previously discussed, one potential threat to our identification strategy is the prospective reflection problem suggested by Manski (1993). The match's D_j^{AS14} used in our dyadic regressions, however, would be less likely to be endogenous to correlated unobservables due partly to the random matching approach, which creates an *i.i.d.* exogenous directed network of prospective links, and partly to the use of both respondent's and match's exogeneous characteristics and their study site dummies to control for contextual and correlated effects. As a robustness check, we run additional regressions to include both respondent's and peer's individual fixed effects to control for the average characteristics of the network of matches. This eliminates individual unobserved time-invariant effects, such as the creditworthiness of the peer as well as tendency of trusting others or any other tendency of systematic reporting bias attributed to individual unobservable characteristics. This regression identifies the effect of IBLI on transfers off of the within-respondent and within-match variation in the specific, randomly selected dyadic relationship as a deviation from the average relationships each party has. This is similar to the "global differences" strategy that Bramoullé, Djebbari, and Fortin (2009) demonstrate identifies endogenous social effects under quite general conditions. Because respondent's uptake status is absorbed in the own fixed effect, the focus here is the coefficient of match's D_j^{AS14}. The regression result in Panel A of table 4 provides supporting evidence that match's IBLI uptake, D_j^{AS14}, has a positive impact on the respondent's willingness to transfer.

We can also test for the (non-) existence of correlated social effects by placebo tests, wherein we replace D_j^{AS14} with a pseudo-match k's IBLI uptake, D_k^{AS14}, for the sub-sample of prospective matches k who are unlikely to be socially connected with respondent *i*. The intuition behind this test is that if *i* and *k* are not socially connected, any correlation between *i*'s willingness to transfer to k would purely stem from reflection effects.

The ideal placebo test would therefore test the relationship between D_k^{AS14} and L_{ik}. Unfortunately, we do not know the full, true network of *i*, so in these data we can only explore whether D_k^{AS14} is related to L_{ij}. We therefore implement an imperfect placebo test using predicted dyadic relationships. Nonetheless, rejection of the null hypothesis that D_k^{AS14} is unrelated to L_{ij} would provide evidence of a reflection effect of correlated behaviors that are not actually due to the relationship between *i* and *j* but just due to belonging to the same general community at the same time. Failure to reject is a low power test that reflection effects do not confound the central findings in our main regression.

To execute this placebo test, we first estimate a probability of *i* knowing *j* based on observed characteristics of i and j using 4033 pairs (see supplementary appendix 3 for the estimation results) and then predict the probability of i knowing every other person in the sample. The mean predicted probability is 0.117. Then we take the sub-sample of prospective matches who are unlikely to be known by i, and randomly select k from the sub-sample then replace D_j^{AS14} with D_k^{AS14}. All other control variables are kept the same as in equation (3). Since the threshold probability of "less likely to know the match" is set arbitrarily, we conduct sensitivity tests to construct the sub-sample with (1) the probability lower than 0.15, (2) the probability lower than 0.25 and distance between pseudo-match (k) and i less than 50 km, and (3) the probability lower than 0.20 and distance between pseudo-match (k) and i less than 100 km. We randomly choose a maximum of eight pseudo-matches to each respondent, but some respondents have less than eight matches who meet those criteria. The estimated results by the CMP estimator presented in Panel B of table 4 show all the coefficient estimates on D_k^{AS14} are statistically insignificant, providing further support to our finding that our results are not driven by unobservable correlated effects.

Dynamic interaction effect

Another robustness check we implement concerns the prospective dynamics of IBLI uptake.[18] So far, we have implicitly assumed that D_i and D_j are determined independently from own and peer's previous experiences on IBLI. Yet, those who have purchased IBLI before may learn from the experience, or they might have different unobserved preferences or characteristics from those who have not bought IBLI, leading to autocorrelation in formal insurance uptake. Furthermore, a peer's uptake might have a significant impact on the respondent's own uptake through social learning, imitation or scale effects (Trærup 2012; Karlan et al. 2014; Cai, de Janvry, and Sadoulet 2015). Through any of these mechanisms, D_i could be affected by the lagged D_i and D_j.

To explore this possibility, we exploit the panel data and the dynamic roll-out of IBLI to recursively estimate D_i and D_j round-by-round via the CMP estimator by modifying equations (9)-(11). That is, we estimate D_i and D_j for the first sales period (August-September 2012) with Z_i and Z_j from that period as instruments, where lagged uptake rates were necessarily zero. Then we use those predicted values along with Z_i and Z_j in the subsequent (January-February 2013) sales period to predict D_i and D_j in the second sales period (IBLI 2), and so on until the August-September 2014 (IBLI 5) sales period for D_j and January-February 2015 (IBLI 6) for D_i. The recursive structure generates consistent parameter estimates because we know the true values of baseline uptake were zero, as the product was not yet available, indeed it had not yet even been announced publicly.

Those who purchased IBLI policies in the third and fourth sales periods received indemnity payouts in October 2014. If the receipt of a payment positively affects subsequent propensity to purchase insurance, we would expect the third and fourth round uptake variables to have positive and statistically significant coefficient estimates on sixth round uptake, perhaps differentially greater point estimates than those of the other sales periods, which did not generate indemnity payouts.

The estimation results, presented in table 5, contain several important new findings. First, we do not see any persistent, significant effect of own or peers' IBLI uptake on own current purchase of IBLI. Also, there is no differential impact on subsequent IBLI uptake for those who received indemnity payouts; the coefficient estimates on the third and fourth sales period uptake variables are uniformly statistically insignificant. Second, consistent with previous interpretation, we do not see any sign of free-riding, as would be reflected by a negative and significant coefficient estimate on peer's lagged IBLI uptake, if *i* would free ride on *j*'s contract in force. Most coefficient estimates on lagged peer's uptake are statistically insignificant.[19] Third, although *i*'s and *j*'s lagged uptake are individually insignificant, they have jointly significant impacts on subsequent uptake decisions in most sales periods. Last but foremost, when we take this recursive approach to addressing the prospective endogeneity of the IBLI uptake variables, the effect of the match's IBLI uptake on the respondent's willingness to make a *dabare* transfer increases significantly in magnitude and becomes statistically significant (Panel B).

Overall, these robustness checks confirm our central findings: formal index insurance uptake does not crowd out informal risk sharing arrangements and, if anything, may even crowd in willingness to make customary transfers. Moreover, our preferred estimation that incorporates dynamic interactions among peers supports the existence of a causal relationship from IBLI uptake to increased willingness to make *dabare* transfers, even if both the respondent and match's uptake are treated as endogenous. We conclude that formal insurance has no crowding-out or free-riding effects on informal social arrangements that support drought risk sharing in this setting.

Conclusions

Index insurance is increasingly considered an important tool to promote rural populations' resilience to shocks such as drought. However, the net additional insurance afforded by these products depends fundamentally on the extent to which they crowd out or crowd in informal insurance through customary institutions mediated by social networks. This paper offers novel empirical evidence on the relationship between uptake of formal index insurance products and informal transfers for very similar purposes. We exploit a unique data set from southern Ethiopia where index-based livestock insurance (IBLI) was recently introduced in pastoralist communities with pre-existing informal risk sharing arrangements known as *dabare*. Using randomized incentives to purchase IBLI and random matching of survey respondents to identify social networks within the communities, we explore whether respondents' and their network peers' formal index insurance uptake affects respondents' willingness to make informal risk sharing transfers to that same peer.

Our empirical findings can be summarized as follows. First, respondents' IBLI uptake has no significant effect on his/her willingness to transfer to peers. Commercial index insurance products do not crowd out informal risk management arrangements. Second, we find weak evidence that a household is more willing to make informal transfers to peers who take up IBLI, consistent with the crowding-in hypothesis. We show this result with several robustness checks, although the evidence is quite mixed. Third, we find no evidence of free-riding wherein one person's IBLI uptake negatively affects others' uptake. Fourth, informal transfers are more likely within socially and geographically proximate pairs, for whom communication and contract enforcement are presumably easier. Overall, our results convincingly dismiss the concern that formal insurance crowds out informal insurance in this context; if anything, it appears to crowd in customary risk sharing arrangements.

[1] Indeed, using different data from the same population we study, Santos and Barrett (2011) find that creditworthiness may play a role in inclusion in *dabare*-type risk sharing arrangements. They also, however, point out that in only 2.3% of their sample did the decision differ between loans and gifts, suggesting that loans and gifts seem empirically indistinguishable in this region.

[2] 1 TLU is equivalent to 1 cow, 0.7 camel, or 10 goats or sheep.

[3] Ten households received a 100% discount related to a parallel, but separate study.

[4] Maertens and Barrett (2013) elaborate on the case for random matching within sample.

[5] Since we do not have the detailed actual transfer data, we could not formally check whether the same applies to our sample. However, we doubt the reliability of the method broke down within the same population over the few years between their and our surveys.

[6] 1 USD is equivalent to 20.46 birr as of March 31, 2015.

[7] Conceptually, L_{ij} can be a continuous variable; however, because of the lack of such data, we exclusively focus on binary response in this study.

[8] Using the contemporary (March 2015) values of X does not qualitatively alter the estimation results.

[9] For a dummy variable, e.g., gender of household head, the sum and difference of x_i and x_j do not make sense. Thus, we instead use a dummy equal to one if household heads *i* and *j* are same gender.

[10] There is no case when j is a relative not known by a respondent *i*.

[11] We use the permanent settlement of each household because some members of some households migrate seasonally trekking their herds (Liao et al. 2017).

[12] Since matches are randomized, we have only 217 symmetric pairs in our data set. Thus, regressions with symmetric dyads have a low statistical power. Nonetheless, as a robustness check, we run those regressions and confirm our main findings discussed below: (1) Respondent's own IBLI uptake has no impact on his/her willingness to *dabare* transfer; and (2) The impact of match's decision on informal transfer is significantly positive only when treated as exogenous.

[13] Because a few key variables are missing for several households, the final number of observations used in estimation is 4033.

[14] The first-stage regression results with control variables are in supplementary appendix 1, while more parsimonious estimation results with fewer control variables in the main regression results are in supplementary appendix 2. Conditioning that a respondent knows a family j, the main regression results are effectively the same as the ones presented in the main text.

[15] Using the additional instrument equal to one if a respondent receives a discount coupon worsens the problem, as the Kleibergen-Paap F-statistic falls to 2.707.

[16] The insignificant coefficient estimate is also found when we run OLS on the 513 household observations where the dependent variable is IBLI uptake in the August-September 2014 sales period and one of the regressors is the randomized encouragement rate for the corresponding period. Such a weak correlation is rather exceptional. We found statistically significant effects of encouragement rates on IBLI uptake in other sales periods.

[17] Put differently, our results can be interpreted as in line with Lenel and Steiner (2017), that the amount of informal transfer is reduced when solidarity partners do not avoid individual losses by purchasing formal insurance.

[18] As additional robustness checks presented in supplementary appendix 6, we have established that only response to acquaintances matters. So we now limit our estimation sample to relevant observations, excluding non-acquaintances.

[19] We also used the average IBLI uptake rate among eight randomly assigned peers in the previous round as a regressor, along with the control variables used in equation (4). Consistent with table 4's results, the average peer's IBLI uptake rate has no significant impact on the respondent's subsequent uptake, indicating that the observed willingness to transfer to the insured peer is not due to the expectation of free-riding. The result is shown in supplementary appendix 4.

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Table 1. Selected Household Characteristics, 2015

Ethnicity: Borana (=1)	0.914
	(0.280)
Household size (person)	6.926
	(2.644)
Household head characteristics	
Male (=1)	0.783
	(0.412)
Age (years)	52.471
	(17.947)
Education attainment (years)	0.520
	(1.847)
% with no education	89.474
	(30.719)
Household economy	
Monthly consumption per capita (birr)	299.706
	(140.959)
% income from livestock	83.766
	(20.535)
Owned animals (TLU)	18.910
	(30.757)
IBLI uptake	
August-September 2014 (=1)	0.203
	(0.402)
January-February 2015 (=1)	0.119
	(0.323)
Informal risk sharing	
Number of matches willing to transfer cattle (max=8)	3.834
	(2.672)
Risk preference	
Highly risk averse (=1)	0.115
	(0.319)
Moderately risk averse (=1)	0.460
	(0.499)
Less risk averse (=1)	0.425
	(0.495)
Sample size	513

Standard deviations are in parentheses.

Table 2. Bivariate Relationships Between Key Variables

Panel A		
	Willing to make informal transfer	
Knows match	No	Yes
No	1352	349
Yes	785	1618

Panel B		
	Geographic Distance	
Knows match	Not far	Far
No	254	1447
Yes	2311	92

Panel C		
	Willing to make informal transfer	
Geographic Distance	No	Yes
Not far	948	1617
Far	1189	350

Table 3. Estimation Results of Respondents' Willingness to Transfer

		Di as endogenous		Di and Dj as endogenous	
	OLS	IV	CMP	IV	CMP
	(1)	(2)	(3)	(4)	(5)
Own IBLI uptake: Di (=1)	-0.005	-0.029	-0.022	-0.030	-0.019
	(0.040)	(0.124)	(0.124)	(0.129)	(0.124)
Match's IBLI uptake: Dj (=1)	0.033**	0.033**	0.033**	-0.189	-0.212
	(0.015)	(0.015)	(0.015)	(0.261)	(0.172)
Know family (=1)	0.395***	0.395***	0.395***	0.397***	0.395***
	(0.031)	(0.030)	(0.031)	(0.030)	(0.031)
Relative (=1)	0.214***	0.214***	0.214***	0.220***	0.215***
	(0.023)	(0.022)	(0.023)	(0.022)	(0.023)
Distance	-0.003**	-0.003**	-0.003**	-0.003**	-0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Distance squared/1000	0.029*	0.030**	0.029*	0.027*	0.029*
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Other control variables	YES	YES	YES	YES	YES
Study site dummies	YES	YES	YES	YES	YES
First stage Kleibergen-Paap rk Wald F statistic		198.241		3.468	
First stage Shea's F test for excluded instruments (p-value)					
D_i		11.44(0.04)		5.90 (0.012)	
D_j				1.43 (0.269)	
Observations	4033	4033	4033	4033	4033

Clustered standard errors at the study site level in parentheses: *** p<0.01, ** p<0.05, * p<0.1
Control variables include: A dummy equal to 1 if household heads *i* and *j* are same gender, and the sum and differences of household size, heads' age and its squared, head's years of education, log per capita expenditure, TLU, and risk preference.

Table 4. Robustness Checks: Reflection Problems

	Panel A	Panel B		
		Placebo test		
	Fixed effects	Probability of Know k <0.15	Distance < 100km & Probability of Know k <0.20	Distance < 50km & Probability of Know k <0.25
	(1)	(2)	(3)	(4)
Match's IBLI uptake (=1)	0.277**			
	(0.098)			
Instrumented own IBLI uptake (=1)		-0.011	-0.032	-0.040
		(0.125)	(0.122)	(0.146)
Pseudo-Match's IBLI uptake (=1)		-0.008	0.009	-0.018
		(0.017)	(0.022)	(0.018)
Observations	4033	3938	3755	3065

Clustered standard errors at the study site level in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Control variables included but not reported here: Panel A: The sum and differences of household size, heads' age and its squared, head's years of education, log per capita expenditure, TLU, and risk preference, and i and j fixed effects.

Panel B: A dummy equal to 1 if household heads i and j are same gender, and the sum and differences of household size, heads' age and its squared, head's years of education, log per capita expenditure, TLU, and risk preference, and study site dummies.

Table 5. Recursive Estimation of IBLI Uptake and IV Estimation of Transfer Likelihood

Panel A: 1st stage

	i's IBLI uptake						*j*'s IBLI uptake				
	IBLI 1	IBLI2	IBLI 3	IBLI 4	IBLI 5	IBLI 6	IBLI 1	IBLI2	IBLI 3	IBLI 4	IBLI 5
Respondent (i)											
IBLI uptake at the 1st period		0.401***	0.114	0.739	-0.173	-0.237		0.024	0.106	0.090	0.074
		(0.139)	(0.398)	(0.621)	(0.559)	(0.372)		(0.046)	(0.111)	(0.136)	(0.133)
IBLI uptake at the 2nd period			0.978	-1.092	0.552	0.978			-0.217	-0.009	-0.217
			(0.867)	(1.430)	(1.307)	(1.053)			(0.172)	(0.206)	(0.179)
IBLI uptake at the 3rd period				0.218	0.001	-0.341				-0.045	-0.094
				(0.413)	(0.368)	(0.500)				(0.096)	(0.190)
IBLI uptake at the 4th period					0.157	0.046					0.038
					(0.260)	(0.286)					(0.161)
IBLI uptake at the 5th period						-0.036					
						(0.496)					
Match (j)											
IBLI uptake at the 1st period		-0.035	0.081	-0.100	-0.156	-0.011		0.362***	0.168	0.505	-0.078
		(0.049)	(0.119)	(0.129)	(0.101)	(0.077)		(0.136)	(0.195)	(0.345)	(0.281)
IBLI uptake at the 2nd period			0.094	0.273*	0.152	0.133			0.765*	-0.744	0.893
			(0.135)	(0.149)	(0.146)	(0.155)			(0.433)	(0.939)	(0.743)
IBLI uptake at the 3rd period				-0.165	0.042	0.015				0.177	-0.500
				(0.121)	(0.151)	(0.107)				(0.565)	(0.505)
IBLI uptake at the 4th period					0.053	0.116**					0.118
					(0.086)	(0.057)					(0.305)
IBLI uptake at the 5th period						-0.063					
						(0.110)					
Encouragement rate	0.403***	0.069*	0.136**	0.197***	0.116**	0.348***	0.420***	0.123**	0.098	0.185***	0.134***
	(0.059)	(0.040)	(0.062)	(0.063)	(0.058)	(0.090)	(0.065)	(0.053)	(0.066)	(0.060)	(0.043)
Other control variables						YES					
Study site fixed effects						YES					
Joint significance test for lagged uptake (chi-squared)											
Respondent (i)		8.29***	13.75***	2.66	0.92	1.24		0.27	1.69	0.61	3.03
Match (j)		0.50	3.67	3.42	5.63	8.14		7.07***	10.00***	2.79	1.83
Both respondent (i) and match (j)		10.36***	13.91***	8.95	14.93*	26.46***		8.39**	11.34**	3.69	6.44
Observations						2358					

Panel B: Main equation

	Willingness to lend a cow to *j*
Instrumented own IBLI uptake (January-Feburuary, 2015)	-0.024
	(0.153)
Instrumented match's IBLI uptake (September/October, 2014)	0.137*
	(0.071)
Relative (=1)	0.206***
	(0.024)
Distance	-0.003
	(0.003)
Distancd squared	0.036
	(0.028)
Other control variables	YES
Study site dummies	YES
Observations	2358

Clustered standard errors at the study site level in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Other control variables in the 1st stage variables include: Discount premium rate at each corresponding sales period for respondent and match, household size, head's characteristics (gender, age, age squared and completed years of education), log per capita expenditure, TLU and its square, a dummy equal to one if moderate risk averse, and a dummy equal to one if less risk averse. *i*'s (*j*'s) household characteristics are used for *i*'s (*j*'s) uptake equations as controls.

Other control variables in the 2nd stage variables include: A dummy equal to 1 if household heads i and j are same gender, and the sum and differences of household size, heads' age and its squared, head's years of education, log per capita expenditure, TLU, and risk preference.

Appendix Table 1. Balancing Test for Key Variables

Sales period	Aug-Sep 2014			Jan-Feb 2015		
Coupon	Non-recipient	Recipient	p-value	Non-recipient	Recipient	p-value
Ethnicity: Borana (=1)	0.912	0.917	0.858	0.931	0.913	0.558
	(0.285)	(0.276)		(0.255)	(0.283)	
Household size (person)	6.863	6.827	0.903	6.941	6.808	0.651
	(2.718)	(2.615)		(2.626)	(2.637)	
Head's Male (=1)	0.814	0.788	0.571	0.812	0.789	0.609
	(0.391)	(0.409)		(0.393)	(0.409)	
Head's Age (years)	50.480	51.766	0.522	48.396	52.274	0.054*
	(18.634)	(18.012)		(16.964)	(18.339)	
Head's Education Attainment (years)	0.657	0.487	0.405	0.356	0.561	0.319
	(1.922)	(1.827)		(1.197)	(1.972)	
Head % with no education	84.314	90.754	0.058*	90.099	89.320	0.820
	(36.547)	(29.002)		(30.016)	(30.923)	
Monthly consumption per capita (birr)	346.199	325.026	0.310	332.083	328.538	0.866
	(194.388)	(186.943)		(163.678)	(194.208)	
% income from livestock	79.946	76.970	0.313	79.839	77.004	0.339
	(22.688)	(27.548)		(25.409)	(26.956)	
Owned animals (TLU)	19.637	18.414	0.693	22.418	17.735	0.132
	(25.103)	(28.652)		(39.912)	(24.129)	
Highly risk averse (=1)	0.157	0.105	0.139	0.089	0.121	0.364
	(0.365)	(0.306)		(0.286)	(0.327)	
Moderately risk averse (=1)	0.451	0.465	0.804	0.475	0.459	0.766
	(0.500)	(0.499)		(0.502)	(0.499)	
Less risk averse (=1)	0.392	0.431	0.482	0.436	0.420	0.775
	(0.491)	(0.496)		(0.498)	(0.494)	
Joint F-test: *p* -value			0.540			0.482

Standard deviations are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

The presented variables are drawn from the 2014 survey, prior to the distribution of discount coupons in August-September 2014.