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

Kazushi Takahashi [a], Christopher B. Barrett [b], Munenobu Ikegami [c]

[a] *Faculty of Economics, Sophia University*

[b] *Charles H. Dyson School of Applied Economics and Management, Cornell University*

[c] *International Livestock Research Institute (ILRI)*

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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 and randomized incentives to purchase a novel index-based livestock insurance product, we find that a randomly matched peer's insurance uptake positively influences respondents' willingness to make informal transfers to that match. By contrast, respondents' own formal insurance uptake has no significant effect on risk sharing through customary institutions. Overall, our results suggest that in this context index insurance does not crowd out, and may even crowd in informal risk sharing mediated by social networks.

Keywords: drought, livestock, pastoralism, social networks

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1. Introduction

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 et al., 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 the covariate risk typically associated with 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 the 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 et al., 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; Jensen et al., 2014; Clarke, 2016).

Theoretically, the relationship between the demand for index insurance and participation in informal risk sharing arrangements is ambiguous. Dercon et al. (2014) and Mobarak and Rosenzweig (2012, 2013) highlight the technological complementarity between index insurance and informal risk sharing. Covariate weather risk that affects all networks 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, informal insurance could dampen index insurance demand and *vice versa*. For example, if an individual's utility depends in part on the aggregate wealth of one's network, then individual insurance uptake generates positive externalities and potentially a free-riding problem (de Janvry et al., 2014). 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 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 (Trærup, 2012; Karlan et al., 2014; Cai et al., 2015). Little is known about effects in the opposite direction: how index insurance uptake affects customary risk pooling arrangements mediated by social networks.

This paper 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*, a form of informal reciprocal exchange under which one household lends livestock to another on a temporary basis in the wake of an adverse shock, with the understanding that roles may reverse in the future.[1] Many observers indicate that *dabare* and other informal risk management institutions have been eroding over time (Lybbert et al. 2004; Huysentruyt et al. 2009; Santos and Barrett 2011).

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.

There are at least four fundamental challenges in empirical research on this topic.

[1] *Dabare* is not used to lend animals to increase the recipient's herd size in the absence of an adverse shock. Other informal institutions play this (and other) role(s).

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 et al., 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 provides 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 main finding is that, in most specifications, when random matches purchase IBLI, respondents become more likely to provide *dabare* transfers to that individual. When we refine the analysis further, we find that these positive inter-personal spillover effects are observed only when the respondent personally knows the match and when the respondent correctly believes the match purchased IBLI. Errors in subjective belief and prospective measurement error in response appear to attenuate the estimated effects in the less refined specification. Such behavior is consistent with index insurance crowding in informal risk sharing arrangements. We find no evidence of a free-riding problem wherein a respondent is less likely to buy IBLI when peers buy IBLI nor of crowding out of existing informal risk sharing networks wherein those who purchase IBLI are less likely to provide *dabare* transfers to their network members. These results remain largely robust to a range of specifications and estimators; our main findings do not appear driven by measurement errors or omitted variable bias. Taken together, we conclude that, if anything, formal index insurance reinforces informal social arrangements in our setting.

This study contributes to the literature on the nexus between formal 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) and Munro (2015). Their results are, however, mixed. Mobarak and Rosenzweig (2012) show evidence supporting the existence of technological complementarity (i.e., increased insurance demand among those engaged in informal arrangements) in rural India, 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. Both 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. Our study reinforces Mobarak and Rosenzweig's (2012) findings by showing that index insurance uptake positively affects informal insurance provision. To the best of our knowledge, this study is the first to provide rigorous evidence that formal insurance products do not break down pre-existing informal arrangements but might instead complement or even reinforce them.

The rest of the paper is organized as follows. Section 2 explains the study setting, the design of the IBLI product, the sampling framework and survey methods, and presents summary statistics. Section 3 explains our estimation strategy, followed by discussion of estimation results. Section 4 discusses a range of robustness checks. Section 5 concludes.

2. Study Design and Summary Statistics

2.1. *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). Mobile 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 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, 2016; 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 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 sample.

To stimulate IBLI uptake and manufacture 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. Discount rates ranged from 10 to 80 percent. Since randomization was independently implemented in each sales period, coupon recipients and realized discount 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) examined factors affecting IBLI uptake in our study area and found that the distribution of discount coupons significantly increases uptake of IBLI. Thus, randomized

[2] 1 TLU is equivalent to 1 cow, 0.7 camel, or 10 goats or sheep.
[3] Ten households received a 100% discount for a specific purpose related to a parallel, but separate study.

receipt of a discount coupon would serve nicely as an instrument for IBLI uptake.

Appendix 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 et al. (2014) 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: *dabare* and *busa gonofa*. *Dabare* involves a loan of cattle transferred voluntarily to a friend or relative struck by a negative shock. By contrast, *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). We therefore focus on whether the voluntary informal risk sharing arrangement *dabare* is affected by the introduction of IBLI.

2.2. *Sampling strategy and survey methods*

We surveyed 17 *reeras* (equivalent to a sub-district 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).

2.3. *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 each household with which a respondent engages in transfers, as most inevitably fall outside the sample. It follows that the data collected from this method fail to control for correlated social effects, where neighbors behave similarly simply because they have similar characteristics (Manski, 1993). 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 et al., 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 is 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 (lineage), 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 Santos and Barrett (2008) show that inferred insurance network derived from this approach closely matches actual network behavior among a different sample of Borana pastoralists from the same region. 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-

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

distance risk sharing in several settings (Jack and Suri, 2014; Blumenstock et al., 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., 2016), 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 et al. (2015) 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 (lineage).

2.4. *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[5] – 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

[5] 1 USD is equivalent to 20.46 birr as of March 31, 2015.

3.8 out of 8 randomly selected matches within the sample, 3 of whom come from outside 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. Perhaps surprisingly, in 349 cases a respondent is willing to transfer cattle to someone they claimed not to know. This raises a concern that we may need to interpret the results with some caution, although it could be that recognition of lineage names suffices for some respondents even when they do not know the specific individual with whom they were randomly matched. 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 (not shown) reveals that the least reliable answers in Panel A (i.e., willing to make a transfer to unknown persons) relate to those from distant communities (289 cases) rather than neighbors within their own community (60 cases).

3. Econometric Analysis

3.1. *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. This link is not necessarily symmetric, meaning $L_{ij} = L_{ji}$ need not hold.[6] 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.

[6] The symmetry restriction could not be imposed anyway for the practical reason that we do not observe all dyadic pairs, *ij* and *ji*, in our data.

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.

The benchmark dyadic model we estimate is specified as:

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

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, although we emphasize that these are merely control variables.[7] W_{ij} includes a dummy variable equal to one if *i* personally knows *j*, a dummy variable equal to one if *j* is relative,[8] which captures social proximity, and the physical distance (in kilometers) between the permanent settlements of *i* and *j*.[9] Since L_{ij} can be unidirectional, 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). 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

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

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

[9] We use the permanent settlement of each household because some members of some households migrate seasonally trekking their herds (Liao et al., 2016).

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.

3.2. *Extension*

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, along with the same set of control variables as in equation (1). The set of estimated equations can then be rewritten as:

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

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

Z_i represents *i*'s randomly assigned coupon discount 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 , to potentially influence *i*'s uptake in the subsequent, January-February 2015, sales period, D_{ij}, hence the subscripting. 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 et al., 2016). More specifically, we estimate the following set of recursive equations:

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

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

So far, we have assumed that D_j is exogenous because *j* is randomly assigned to *i*. If *j*'s decision is independent from *i* in the real world, then D_j should be an exogenous random variable in the main equation (5). Yet if the informal insurance network forms endogenously, bias may arise due to omitted variables.

To test whether the results are altered by different specifications and estimators, we also run regressions by treating both D_i and D_j as endogenous using both IV and CMP estimators. The discount rate of *j*'s insurance premium in the August-September 2014 sales period (Z_j^{AS2014}) is used as an additional instrument. The set of equations for the two endogenous variables IV estimation is specified as:

$$D_{ij}=\alpha_6+\beta_6(X_i+X_j)+\tau_6(X_i-X_j)+\pi_6W_{ij}+\gamma_6Z_i+\gamma_6Z_j^{AS2014}+\varphi_6+\omega_6+u_{6ij} \quad (6)$$

$$D_{ji}=\alpha_7+\beta_7(X_i+X_j)+\tau_7(X_i-X_j)+\pi_7W_{ij}+\gamma_7Z_i+\gamma_7Z_j^{AS2014}+\varphi_7+\omega_7+u_{7j} \quad (7)$$

$$L_{ij} = \alpha_8+\rho_8\widehat{D}_{ij}+\delta_8\widehat{D}_{ji}+\beta_8(X_i+X_j)+\tau_8(X_i-X_j)+\pi_8W_{ij}+\varphi_8+\omega_8+u_{8ij} \quad (8)$$

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

$$D_i = \alpha_9+\beta_9X_i+\gamma_9Z_i+\varphi_9+u_{9i} \quad (9)$$

$$D_j = \alpha_{10}+\beta_{10}X_j+\gamma_{10}Z_j^{AS2014}+\omega_{10}+u_{10j} \quad (10)$$

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

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).[10] 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 appears exactly eight times in the latter specification. However, the predicted value of $\widehat{D}_j$ differs for the 4104 observations in equation (10) because random matching generates exogenous variation in the number of times household *j* is 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 qualitative results (not shown) are quite similar between these two methods. In what follows, therefore, we present only the unweighted CMP results, which do not require standard error adjustments necessary for a manual two stage least squares method like the second approach.

3.2. *Empirical results*

Table 3 shows the main linear probability model estimation results for the key variables of interest.[11] Column (1) corresponds to the result of the OLS estimator in

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

[11] The first-stage regression results as well as the coefficient estimates on suppressed control

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 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 discount rate, is strongly correlated with the respondent's IBLI uptake, with a first-stage F-statistic of 11.4 (p=0.004).

The positive impact of a peer's uptake on a respondent's willingness to provide an informal transfer is consistent with the existence of either crowding-in or free-riding exists. 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 these data of no correlation between IBLI uptake and actual interhousehold transfers (Bageant and Barrett, 2016). Moreover, the dynamic interaction analysis in Section 4.3 shows no negative impact of peer's IBLI uptake on respondent's subsequent uptake, further supporting the absence of free-riding.

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 F-statistic that is merely 1.43 (p=0.269) for D_j. Somewhat unexpectedly, the first-stage estimation result in Appendix 2 reveals that match's randomized coupon discount rate does not induce significantly greater uptake in this particular sales period.[12]

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. Indeed, there is weak 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.

Before moving on to robustness checks in the next section, other important findings

variables in the main equations are in Appendix 2.

[12] 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 discount rate for the corresponding period.

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 social and physical distance are important for informal risk sharing, presumably because they decrease communication and transaction costs (Fafchamps and Gubert, 2007; Chandrasekhar et al., 2015).

4. Robustness Checks

While we 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. Moreover, estimation that appropriately treats peers' uptake, D_j, as endogenous suffers from a weak IV problem. We now explore the robustness of our findings through a series of alternative approaches to test the validity of the existence of crowding-in effects, first treating D_j as exogeneous due to the random matching of *j* to each respondent, and then relaxing that assumption.

4.1. *Falsification test*

If the effect of network members' IBLI uptake on a respondent's willingness to make *dabare* transfers truly arises because formal insurance crowds in informal insurance, then this effect should hold only for IBLI contracts currently in force. Match *j*'s lapsed contracts should have no impact on *i*'s current willingness to transfer cattle. So a useful falsification test involves replacing D_j purchased in the fifth (August-September 2014) sales period, which were still in force at the time of the L_{ij} elicitation, with D_j values from previous sales periods, i.e., contracts that had lapsed. IBLI uptake by respondents varied over time in response to discount coupon receipt and other factors, generating within-household variation that makes such a falsification test feasible.

The results presented in Table 4 consistently show that the match's lapsed IBLI uptake has no impact on the respondent's willingness to transfer even when match's uptake is treated as exogenous. The point estimates on the lapsed D_j variables derived from any of the previous sales periods are an order of magnitude smaller than in our main results and highly statistically insignificant. This reinforces the interpretation of our main findings as indeed reflecting a positive crowding in effect of formal insurance uptake (D_j) on informal insurance (L_{ij}).

4.2. *Measurement error*

Another potential flaw is measurement error in L_{ij} or D_j.[13] Measurement errors in the dependent variable yield consistent but inefficient estimates, whereas measurement errors in the independent variable lead to inconsistent estimates.

As shown in Table 2, in 349 cases a respondent expressed willingness to transfer cattle to matches they claimed not to know. This could reflect indirect acquaintance – e.g., the respondent recognizes the match's family name but does not know the match specifically. But it could also signal measurement error, perhaps because these respondents did not understand the hypothetical transfer question and wanted to signal their altruism or respect for the traditional cultural practice of *dabare*. In such cases, respondent *i*'s responses would be plagued by measurement error. As one robustness check against such a possibility, we re-estimate using only the sub-sample households whose answers to the hypothetical transfer question do not include "Yes" to any unknown person. We estimated this sub-sample model by first treating only D_i as endogenous, and then both D_i and D_j as endogenous. For the sake of brevity, we present only the CMP results with the clustered standard errors at the study site level in Panel A, Table 5. The results do not alter our main finding that others' insurance uptake has positive impacts, but only when it is treated as exogenous.

A different approach to this prospective source of measurement error is to run the regressions separately for acquaintances and non-acquaintances. From the evidence in Tables 2 and 3, we already established that knowing the match is strongly, positively associated with the respondent's willingness to transfer cattle to the match. If claimed willingness to transfer to non-acquaintances reflects just measurement error, then splitting the sample should lead to greater precision in estimating the parameter of interest within the sub-sample of acquaintances. By contrast, if the results are equally strong among non-acquaintances and acquaintances, that may signal omitted variable bias, e.g., that IBLI uptake happened to have been stronger among a distinct sub-population among whom *dabare* is also more robust, due to omitted leader or geographic effects not controlled for adequately with site fixed effects. The results in Panel B of Table 5 show that the positive and significant relationship is observed exclusively for acquaintances. Indeed, the point estimate is greater in magnitude and more precisely estimated than in Table 3 when we

[13] Since we use an instrument for D_i, the measurement error in D_i is, if any, not problematic.

strip out the observations of non-acquaintances. If a respondent personally knows the match, that match's uptake decision is strongly, positively associated with the respondent's willingness to transfer, but if not, there is no interpersonal spillover effects. This reinforces confidence in our main findings.

Another possibility is measurement error in D_j in the sense that household *i* inaccurately perceives *j*'s IBLI uptake. Knowing the match does not imply knowing the match's actual behavior (Hogset and Barrett, 2010), especially if the behavior in question (IBLI uptake) is not easily visible and verified. Thus, actual D_j and household *i*'s perception of D_j may differ and in ways that may impact *i*'s behavior. Appendix 3 confirms that most respondents did not know their match's IBLI uptake status and of those who believed they did, many held incorrect beliefs. Out of 324 matches that respondents believed had bought IBLI, for example, only 109 (33.6%) actually bought it in the same reference period according to match *j*'s self-report.

To check for the possibility that by using actual uptake data we introduce measurement error with respect to respondents' beliefs, we replace D_j with proxy-reported peer's IBLI uptake in the latest survey, i.e., whether respondent *i* thought that match *j* bought IBLI in the sales periods prior to the survey. This question was asked only when the respondent personally knew the match in order to avoid groundless guesses.

To examine whether our inferences change due to the measurement error that might arise from the deviation of actual from perceived D_j, we re-run the regressions replacing actual (self-reported) D_j with three dummy variables that represent the combination of (*proxy-reported, self-reported*) match's IBLI uptake behavior: $D^i_{j1} = (buy, buy)$, $D^i_{j2} = (buy, not\ buy)$ and $D^i_{j3} = (not\ buy, not\ buy)$. That is, D^i_{j1} is correct perception with actual purchase, D^i_{j2} is false positive, and D^i_{j3} is correct perception with actual non-purchase.[14] The CMP results reported in Panel C of Table 5 clearly show that the match's IBLI uptake has positive impacts on the likelihood of informal transfer only when the respondent accurately knows it. The estimated positive impact of D_j on *dabare* transfers thus does not appear driven by measurement error in D_j.

4.3. *Dynamic interaction effect*

A final robustness check we implement explores the prospective dynamics of IBLI uptake.[15] 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

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[14] Since the number of observations of false negative is small, we set a reference group as false negative plus those respondents who did not know the peer's uptake status.

[15] Given that we have just established that only response to acquaintances matters, we now limit our estimation sample to relevant observations, excluding non-acquaintances.

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 et al., 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. 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 first round (IBLI 1) values of uptake were zero, as the product was not yet available, indeed it had not yet even been announced publically.

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, perhaps differentially greater point estimates than those of the other sales periods, which did not generate indemnity payouts.

The estimation results, presented in Table 6, 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. Second, 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. Third, 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, as *i* would free ride on *j*'s contract in force. Most coefficient estimates on lagged peer's uptake are statistically insignificant, except for two positive point estimates.[16] Most importantly, when we take this recursive approach to addressing the prospective

[16] 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 6'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 Appendix 4.

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.

Overall, a range of robustness checks confirm our central findings: formal index insurance uptake does not appear to crowd out informal risk sharing arrangements and may even crowd in 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. Given the existence of positive impacts and absence of crowding-out or free-riding effects, we conclude that formal insurance complements informal social arrangements that support drought risk sharing in this setting.

5. 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, we find that a household is more willing to make informal transfer to peers who take up IBLI. We confirm this result with various robustness checks. Second, respondents' IBLI uptake has no significant effect on his/her willingness to transfer to peers. Together, these first two results provide strong evidence against the hypothesis that formal insurance crowds out informal insurance in this context; if anything, it appears to crowd in customary risk sharing arrangements. 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 suggest an important complementarity between formal index insurance and informal risk sharing arrangements in mitigating drought risk in this setting.

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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	89.669
	(30.467)
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 Respondent's Willingness to Transfer

		Di as endog		Di and Dj as endog	
	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 F-statistics (p-value)					
D_i		11.44 (0.004)		5.90 (0.012)	
D_j				1.43 (0.269)	
Observations	4033	4033	4033	4033	4033

Cluster 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: Falsification test

	Willingness to transfer CMP			
	(1)	(2)	(3)	(4)
Instrumented own IBLI uptake: Di (=1)	-0.023	-0.023	-0.024	-0.024
	(0.125)	(0.125)	(0.120)	(0.120)
Match's IBLI uptake : Dj at the 4th sales period (=1)	-0.008			
	(0.014)			
Match's IBLI uptake : Dj at the 3rd sales period (=1)		0.005		
		(0.017)		
Match's IBLI uptake : Dj at the 2nd sales period (=1)			0.004	
			(0.026)	
Match's IBLI uptake : Dj at the 1st sales period (=1)				-0.003
				(0.016)

Cluster standard errors at the study site level in parentheses: *** p<0.01, ** p<0.05, * p<0.1. N=4,033

Control variables included but not reported 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, risk preference, and study site dummies.

Table 5. Robustness Checks: Measurement Errors

	Panel A		Panel B		Panel C
	Di endog	Di and Dj endog	Acquaintances	Non-acquaintances	Subjective perception and actual behavior
	CMP	CMP	CMP	CMP	CMP
	(1)	(2)	(3)	(4)	(5)
Instrumented own IBLI uptake (=1)	-0.039	-0.038	-0.064	0.021	-0.023
	(0.120)	(0.119)	(0.159)	(0.195)	(0.125)
Match's IBLI uptake (=1)	0.036***	-0.109	0.045**	0.007	
	(0.012)	(0.187)	(0.018)	(0.020)	
Correct perception with actual uptake (D1)					0.080*
					(0.049)
False positive (D2)					0.030
					(0.034)
Correct perception with actual non-uptake (D3)					0.077
					(0.048)
Observations	3042	3042	2358	1675	4033

Cluster 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: 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 6. 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)					
Other control variables						YES					
Study site fixed effects						YES					
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

Cluster 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 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)	0.931	0.913		0.912	0.917	
	(0.255)	(0.283)	0.558	(0.285)	(0.276)	0.858
Household size (person)	6.941	6.808		6.863	6.827	
	(2.626)	(2.637)	0.651	(2.718)	(2.615)	0.903
Head's Male (=1)	0.812	0.789		0.814	0.788	
	(0.393)	(0.409)	0.609	(0.391)	(0.409)	0.571
Head's Age (years)	48.396	52.274		50.480	51.766	
	(16.964)	(18.339)	0.054*	(18.634)	(18.012)	0.522
Head's Education Attainment (years)	0.356	0.561		0.657	0.487	
	(1.197)	(1.972)	0.319	(1.922)	(1.827)	0.405
Head % with no education	90.099	89.320		84.314	90.754	
	(30.016)	(30.923)	0.820	(36.547)	(29.002)	0.058*
Monthly consumption per capita (birr)	332.083	328.538		346.199	325.026	
	(163.678)	(194.208)	0.866	(194.388)	(186.943)	0.310
% income from livestock	79.839	77.004		79.946	76.970	
	(25.409)	(26.956)	0.339	(22.688)	(27.548)	0.313
Owned animals (TLU)	22.418	17.735		19.637	18.414	
	(39.912)	(24.129)	0.132	(25.103)	(28.652)	0.693
Highly risk averse (=1)	0.089	0.121		0.157	0.105	
	(0.286)	(0.327)	0.364	(0.365)	(0.306)	0.139
Moderately risk averse (=1)	0.475	0.459		0.451	0.465	
	(0.502)	(0.499)	0.766	(0.500)	(0.499)	0.804
Less risk averse (=1)	0.436	0.420		0.392	0.431	
	(0.498)	(0.494)	0.775	(0.491)	(0.496)	0.482
Joint F-test: *p* -value			0.539			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.

Appendix 2. First-stage regression results for Table 3.

	Panel A: IV		
	Di as endog	Di and Dj as endog	
	Own IBLI uptake: Di (=1)	Own IBLI uptake: Di (=1)	Match's IBLI uptake: Dj (=1)
Own discount rate (Jan-Feb 2015)	0.325***	0.325***	-0.005
	(0.093)	(0.096)	(0.0.017)
Match's discount rate (Aug-Sep 2014)		-0.010	0.092
		(0.016)	(0.055)
Know family	0.018	0.018	0.007
	(0.013)	(0.014)	(0.020)
Relative	0.004	0.004	0.024
	(0.021)	(0.022)	(0.029)
Distance	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)
Distance squared	0.003	0.003	-0.009
	(0.005)	(0.006)	(0.008)
Head's same gender	0.006	0.006	0.030*
	(0.013)	(0.013)	(0.017)
Summation			
Household size	0.000	0.000	-0.007**
	(0.002)	(0.002)	(0.002)
Head's age	0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)
Head's years of education	-0.003	-0.003	0.009*
	(0.004)	(0.004)	(0.004)
ln per capita expenditure	0.042**	0.042**	-0.003
	(0.015)	(0.015)	(0.016)
TLU	-0.000	-0.000	0.000
	(0.001)	(0.001)	(0.000)
Risk preference	0.000	0.000	0.001
	(0.003)	(0.003)	(0.003)
Difference			
Household size	-0.000	-0.000	0.007**
	(0.002)	(0.002)	(0.003)
Head's age	-0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)
Head's years of education	-0.004	-0.004	-0.006
	(0.004)	(0.005)	(0.005)
ln per capita expenditure	0.025**	0.026**	-0.005
	(0.010)	(0.010)	(0.016)
TLU	0.000	0.000	0.001
	(0.000)	(0.000)	(0.000)
Risk preference	0.002	0.002	-0.005
	(0.003)	(0.003)	(0.004)
Study site dummies	Yes	Yes	Yes
Constant	-0.781***	-0.779**	0.092
	(0.286)	(0.296)	(0.055)
Observations	4,033	4,033	4,033
R-squared	0.309	0.309	0.157

	Panel B: CMP		
	Di as endog	Di and Dj as endog	
	Own IBLI uptake: Di (=1)	Own IBLI uptake: Di (=1)	Match's IBLI uptake: Dj (=1)
Respondent i's characteristics			
Own discount rate (Jan-Feb 2015)	0.326***	0.325***	
	(0.096)	(0.096)	
Household size	0.001	0.001	
	(0.004)	(0.004)	
Head's male	0.012	0.012	
	(0.027)	(0.027)	
Head's age	-0.002	-0.002	
	(0.003)	(0.003)	
Head's age squared	0.000	0.000	
	(0.000)	(0.000)	
Head's years of education	-0.007	-0.007	
	(0.008)	(0.008)	
ln per capita expenditure	0.072***	0.072***	
	(0.025)	(0.025)	
TLU	-0.000	-0.000	
	(0.001)	(0.001)	
TLU squared	0.000	0.000	
	(0.000)	(0.000)	
Moderately risk averse (=1)	0.042	0.042	
	(0.032)	(0.032)	
Less risk averse (=1)	0.037	0.037	
	(0.035)	(0.035)	
Peer j's characteristics			
Match's discount rate (Aug-Sep 2014)			0.087
			(0.054)
Household size			-0.018***
			(0.005)
Head's male			0.035
			(0.051)
Head's age			0.005**
			(0.002)
Head's age squared			-0.000**
			(0.000)
Head's years of education			0.015
			(0.010)
ln per capita expenditure			0.000
			(0.033)
TLU			0.001
			(0.001)
TLU squared			-0.000
			(0.000)
Moderately risk averse (=1)			0.002
			(0.046)
Less risk averse (=1)			0.020
			(0.032)
Study site dummies	Yes	Yes	Yes
Constant	-0.658***	-0.657***	0.085
	(0.221)	(0.221)	(0.333)
Observations	4,033	4,033	4,033

Appendix 3. The relationships among proxy-reporting and actual IBLI uptake, and informal transfer

Panel A		
	Informal transfer	
Proxy reporting of IBLI uptake	No	Yes
Yes, uptake	81	243
No uptake	30	109
Don't know	661	1234

Panel B		
	Actual IBLI uptake by match	
Proxy reporting of IBLI uptake	No	Yes
Yes, uptake	215	109
No uptake	123	16
Don't know	1509	386

Appendix 4. The impact of others' insurance uptake on own IBLI purchase
(Linear Probability Model)

	=1 if *i* uptake IBLI
Average of peers' (*j*) uptake rate at September-October 2014	-0.131
	(0.100)
Discount premium rate for *i* at January-February 2015	0.323***
	(0.051)
Household size (# persons)	0.002
	(0.006)
Head is male (=1 if yes, =0 otherwise)	0.013
	(0.032)
Head's age	-0.002
	(0.004)
Head's age^2	0.000
	(0.000)
Head's years of education	-0.007
	(0.008)
ln per capita expenditure	0.070***
	(0.027)
TLU	-0.000
	(0.001)
TLU^2	0.000
	(0.000)
Moderate risk averse (=1)	0.039
	(0.039)
Less risk averse (=1)	0.034
	(0.039)
Constant	-0.616**
	(0.250)
Study site dummies	YES
Observations	513
R^2	0.308

Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1