The impact of interlinked insurance on risk-copying and welfare: an RCT in Ethiopia'

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Abstract

Provision of integrated insurance, creditand agricultural technologies can enormously help to promote agricultural intensification, tackle food insecurity and poverty in developing countries.Index-based insurance(IBI) is evidenced to have the potential to overcome the well-known moral hazard and adverse selection problems that often plague the development of rural financial markets.However, adoptionof IBIhas met unexpectedly low uptake and up-scaling challenges. Evidence on the extent to which interlinking IBI with credit and agricultural input can enhance the uptake and economic impacts of IBI is scant.We conducted a randomized controlled trail (RCT) with1661 smallholders in Ethiopia, randomly offering them with a standalone IBI, IBI interlinked with credit and IBI interlinked with both credit and agricultural inputs. Results indicate that the uptake of the standalone IBI is low, but interlinking IBI with credit and inputs significantly increases uptake. We estimatedthe impacts of the interlinked IBI on household consumption and investment in inputs. We find that interlinking IBI with credit and inputs has casually increased household consumption and investment in high-risk high-return inputs.We also estimate the impact of the intervention on productivity, subjective well-being and risk-copying, finding that the interlinked intervention increases land productivity and improves subjective well-being and shock-copying ability of adopters. The findings imply that increased interlinkage enhances the uptake and impact of insurance that can help to upscale agricultural risk management options for smallholders.

Keywords: IBI, InterlinkedIBI-credit-input, RCT

JEL Classification Codes: O44, Q41, D92, G22

1. Introduction

Agricultural risk and limited access to credit are key impediments to agricultural productivity and constitute a major source of poverty among smallholder farmers in developing countries. While insurance provides a market mechanism to shield the welfare of smallholders from the adverse effects of weather and seasonality-based variations, agricultural loans serve farmers to acquire and adopt high-risk high-return agricultural inputs such as improved seed varieties, fertilizer, pesticide and herbicide. Interlinking insurance with credit and agricultural technology is thus important for the mutual benefit of smallholder borrowers and lenders (Karlan et al. 2014). The benefit to the smallholderis both access to loan in order to acquire inputs that enhance productivity, and access to insurance to hedge down-side production risk. Similarly, lenders can also benefit from the interlinked insurance with credit since insured farmers possess a higher potential to repay loans, and due to the fact that default risk from lending for insured borrower farmers is lower than the risk to lend for uninsured ones (McIntosh, Sarris and Papadopoulos 2013; Farrin and Miranda 2015). This incentive thus can motivate lenders to enter markets with minimized default risk that would otherwise cannot be anticipated when the production risk that leads to default risk is not insured. In this way, insurance can help to crowd-in credit supply. And as farmers need such arrangement, it can also crowds-in credit demand. Previous studies reveal that financial market imperfections prevail among smallholders farmers in developing countries, in the form of credit and insurance rationing that impede the economic potential of the poor to surmount the critical threshold, leading to poverty traps (Boucher et al 2008; Barnett, Barrett and Skees 2008; Carter, Cheng and Sarris 2016). As an integrative solution for this, the interlinked insurance-credit-input system is a win-win strategy that forms a financial environment where insurance and credit complementarily reinforce (crowd-in) each other, and where both the borrower and the lender remain better off.

Interlinked insurance-credit-input intervention is based on the premise that lack of credit among smallholder farmers can limit their access to insurance and their potential to adopt high-risk high-return agricultural inputs. In this study, we design an innovative interlinked IBI-credit-input intervention that forms a platform that provides farmers with a sandwich of three important rural technologies:index-based insurance (IBI), IBI linked credit (ILC) and agricultural input(AI). Index-based insurance is a climate risk management strategy that can provide welfare benefits for the poor (Carter et al. 2016; Barrett 2011). It is an innovative hedging instrument that mitigates drought shocks and seasonality-based weather risks induced by climate change (Barnett et al. 2008; Chantarat et al. 2013; Skees 2008, Barrett 2011). In IBI innovation, payout is triggered when the index of a selective weather variable falls below a given threshold, signalling risk. Usually, intensity of rainfall or vegetative cover on the earth surface measured by satellite remote sensing constitutes the current generation of such an index (Skees 2008; Takahashi et al. 2016). A reliable index closely correlates with the insured asset, objectively quantifiable and publicly verifiable in order not to be manipulated by both the insurer and the insured(Skees 2008; Jensen, Mude and Barrett 2018; Barnett et al. 2008). IBI innovations are thus useful to overcome challenges that often plague the development of rural financial markets like the functioning of indemnity-based insurances for long. First, IBI delinks loss assessment from individual behaviour to overcome moral hazard problems. Second, IBI design is based on publicly verifiable data (e.g., rainfall data based on satellite measures), so it partially tackles the problem of adverse selection. Third, the use of a single index to estimate losses of a group of farms minimizes transaction costs. Hence, IBIs uniquely overcome classic incentive problems like information asymmetry and transaction costs associated with claim verification and contract enforcement in rural financial markets(Barnett et al. 2008).

The second ingredient of this innovative interlinked insurance-credit-input intervention is what we call anIBI linked credit (ILC). ILC is a bundling of index insurance and credit which works as a market-based solution to minimize downside risks and unlock credit to smallholder farmers (Gine and Yang 2009; Shee and Turvey 2012; Shee, Turvey and Woodward 2015). This mechanism provides smallholder farmers with a linked financial product that embeds within its structure an insurance protection which, when triggers, offsets loan payments due to the lender providing a risk-efficient balance between business and financial risks(Shee and Turvey 2012; Farrin and Miranda 2015). The innovation does not require farmers to pay premiums upfront and out-of-pocket, hence it removes liquidity constraints of farmers to acquire high-risk high-return inputs(Udry 1990; Clarke and Mahul 2011; Karlan et al. 2014). To target some amount of the loan to acquire these inputs, our intervention embeds agricultural input coupons (AIC) that smallholder's use to take improved seed variety, fertilizer, pesticide and/or herbicide from input suppliers in Ethiopia. AIC thus constitutes the third component of the intervention. In this way, the interlinked insurance-credit-input intervention together could combine the advantages of all the three and hence canachieve better targeting of poorer farmers. Further, through training farmer' representatives the innovation also encourages risk-rationed farmers to take up insurance, loan, financial education and extension.

This study examines the extent to whichthis innovative interlinked insurance-credit-input intervention enhances the uptake and impacts of integrated rural technologiesamong smallholders. The study is undertaken in the Rift Valley zone of Ethiopia where rainfall shocks and drought adversely affect household welfare and where the prevalence of credit and insurance rationing was evidenced(Ali and Deininger 2014; Belissa et al. 2018).[1]In the study area, given the need for an effective risk transfer mechanism, high and sustained rural technology uptake by farmers, and the need for increased investment in high-risk high-return agricultural inputs to increase productivity, it is important to assess whether the innovative interlinked insurance-credit-input intervention mechanism increases uptake and economic impacts.The rest of the paper is organized as follows. Section 2 lays out a model of insurance-linked credit and agricultural input use. Section 3 describes our intervention and randomization strategy. Section 4 presents the balancing teststo check whether the randomization has worked. Section 5 explains our estimation strategy. Section 6 presents the main results. Section 7 concludes the paper.

2. A model of insurance-linked credit and agricultural input use

An insurance-linked credit is a credit product that bundles an index-based insurance (IBI) with the repayment structure of the credit so that when the insurance triggers farmers repayment obligation is reduced. When the weather risk (e.g., variation in rainfall) worsens and crosses a predetermined trigger the insurance pays out that reduces farmers repayment burden. But if the risk is not triggered the loan must be repaid along with risk premium.

To model how an insurance-linked credit influences agricultural input demand and productivity, we start with a simplified production model with optimal choice of borrowing. If a liquidity constrained household needs q share of money to purchase the input x at a cost r then the household's marginal cost of borrowing would be $r(1+i)$ and the total debt wouldbe $qr(1+i)x$. We assume that the household has the remaining share $(1-q)$ of money in cash that (s)he

[1]Employing a direct elicitation method (DEM) to determine credit rationing status, it is determined that 38% of the sample households in Ethiopian Rift Valley zone are credit constrained.

invests to purchase the input x at a cost of r. The profit-making condition of a farm household is given by

$$\pi = Py(x\,|\,\theta) - (qr(1+i) + (1-q)r)x \tag{1}$$

where the agricultural production y comes with risk, which we define as $y(x\,|\,\theta)$. In discrete measure, we can think of the production with less risk as y_H with probability φ and the production with high risk as y_L with probability $1-\varphi$. We assume the farmers are price takers, which is realistic in the sense that our farmers are smallholder maize and wheat farmers. The optimal input choice is determined by the following first order condition

$$P\frac{\partial y(x\,|\,\theta)}{\partial x} = qr(1+i) + (1-q)r \tag{2}$$

Following Shee and Turvey (2012) if we assume a quadratic production function $y(x\,|\,\theta) = a + bx - cx^2$, the optimal input demand function can be written as

$$x^* = \frac{b}{2c} - \frac{qr(1+i) + (1-q)r}{2cP} \tag{3}$$

Now, since the credit is insurance-linked credit and the insurance is based on rainfall R, we define the insurance as a put option on uncertain rainfall (which is highly correlated with agricultural production) with a rainfall guarantee/strike of K whose payoff $= E[\max(K-R,0)]$.

By design, the insurance-linked credit requires a risk premium $i^* - i$, at which the lender is indifferent between a payout in full and a partial payout linked with rainfall shortage. This condition can be written as

$$qr(1+i)x^* = qrx^*(1+i^*) - \psi E[\max(K-R,0)] \tag{4}$$

where the hedge ratio (strike adjusted loan amount) $\psi = \frac{qrx^*}{K}$ because the farmers are required to repay only the principal amount and the financial institutions require the risk premium bundled with credit amount (insurance covers only the principal loan amount). Solving for i^* we get

$$i^* = i + \frac{E[\max(K-R),0]}{K} \tag{5}$$

To determine the optimal input demand function under insurance-linked credit situation we substitute (5) in (3) and obtain

$$x^{*}=\frac{b}{2c}-\frac{qr\left(1+i+\dfrac{E[\max(K-R,0)]}{K}\right)+(1-q)r}{2cP} \tag{6}$$

from which the following conditions can be obtained

$$\frac{\partial x^{*}}{\partial q}=-\frac{ri}{2cP}<0 \tag{7a}$$

$$\frac{\partial x^{*}}{\partial q\partial P}=\frac{ri}{2cP^{2}}>0 \tag{7b}$$

$$\frac{\partial x^{*}}{\partial K}=\frac{qr}{2cP}\left(\frac{E[\max(K-R,0)]-K\dfrac{\partial E[\max(K-R,0]}{\partial K}}{K^{2}}\right)<0 \tag{7c}$$

(7a) indicates that optimal input demand decreases with increased loan. (7b) indicates that the impact of higher credit can be offset by higher prices. From (2) the expected marginal value product can be written as

$$E[MVP]=qr(1+i(K))+(1-q)r \tag{8a}$$

Differentiating (8) with respect to K

$$\frac{\partial E(MVP)}{\partial K}=qr\frac{\partial i(K)}{\partial K}>0 \tag{8b}$$

(7c) shows that the effect of increased insurance coverage on input use is negative[2] but (8b) shows that the expected marginal value product of input use increases with the insurance coverage. This means that marginal cost increases with insurance coverage but the farmers will continue to use agricultural inputs if the gain in expected marginal product from mitigating

[2] This is negative because our embedded insurance is a put option which provides protection against declining rainfall. The term $\frac{\partial E[\max(K-R,0]}{\partial K}$ is positive and gets larger as K increases. This can be verified numerically.

downside weather risk exceeds the marginal cost of input use. In this way farmers can balance business and financial risks. This way insurance-linked credit provides protection against downside risk and encourages input use and impact agricultural productivity positively. In the empirical section we will see this effect: when farmers are offered insurance bundled with credit and agricultural inputs farmers' take-up rate, input use and consumption levels has increased.

3. Intervention and randomization strategy

3.1.Components of the intervention

Insurance: Through a local insurance company known as Oromia Insurance Company (OIC) in Ethiopia, an IBI product known as a vegetation index crop insurance (VICI)[3] was sold to the smallholders in the study area. The product is designed based on the intensity of vegetation cover or greenery on the earth's surface. Greenery level is measured by a satellite indicator known as normalized difference vegetation index (NDVI)[4]. In VICI design, NDVI is extracted at a geospatial resolution of 1 km × 1 km. The VICI product used by OIC is based on average NDVI of 16 years. NDVI reflects the already accumulated result of rain on crop growth. It is a primary measurement with no assumptions or calibrations. It is the proven standard index, in use by all early warning units globally. Actual decal NDVI data for a given period is calculated for a set of households grouped in a one crop production system (CPS) zone. The NDVI compiled for grids of 1 km × 1 kmwill then be arranged in percentile ranges from 1 to 20, 25 and 50. Based on these percentiles, benchmark values for trigger and exit index points which will be compared to the actual risk level are determined[5]. In the design of VICI, it is assumed that since uptake gradually increases, it is possible to pool more risks across areas with greater geo-spatial variations that can

[3] VICI is an improved IBI product of OIC compared to the weather index crop insurance (WICI).

[4]NDVI is measured through images obtained from a geo-satellite weather infrastructure known as GeoNetCast System. The system reads to see if the observed area contains live green vegetation or not. The data from these images are converted into digital numbers (DN-values), i.e. Integers from 0 to 255 creating the NDVI.

[5] The mechanics of the VICI product design has the following characteristics. The index is based on greenness level. The source of the satellite data is a weather infrastructure known as GeoNetCast. The system converts satellite images into digital numbers ranging from 0 to 255. It uses NDVI data for the last 16 years and reads deviation from this long-term average. In order to deal with transaction costs, it divides the geographical coverage into CPS zones. NDVI is computed for each zone at grids of 1 km × 1 km. NDVI data are usually arranged in percentiles, and payout is calculated for a decal or every 10 days period. Hence, trigger and exit thresholds are computed for CPS zones and 10-day period specific. The insurance coverage period is crop growth season specific. Payout is not crop-specific, but 1km × 1 km grid (location) specific.

help to reduce transaction costs. OIC expects nearly about one out of six households who purchased IBI may face losses. Hence, the sum to be insured per policy is given as follows:

$$S_{vici} = \frac{P}{0.15} \tag{9}$$

For each household who decides to take IBI, a premium of ETB[6] 100 per policy was paid to OIC. Payout which is a maximum of sum insured is determined according to the level of the NDVI. To explain how this works at OIC, let T, E and A represent trigger, exit and actual parametric values of the NDVI index. Then, the amount of payout in each insurance period is calculated for individual VICI buyer households as follows:

$$I_{vici} = \left(\frac{T-A}{T-E}\right)\left(\frac{P}{0.15}\right) \tag{10}$$

In determining payouts for VICI purchasers, OIC uses a linearly proportional indemnification (LPI) approach. For instance, for a single insurance with premium of ETB 100, the payout for a complete loss is 100/0.15 which is about ETB 667. Using LPI, for instance, in areas where the index indicates a 50% loss, a partial payout of about ETB 333.5 is paid to the farmers.

Credit: Smallholders were also offered with a risk contingent credit product of ETB 200in which they are not required to repay their loan if an indexed risk event occurs. The amount and repayment of this loan is contingent on the level of the risk that the households experience. Our project purchases index insurance coverage equal to the value of the loan plus interest from OIC and passes the premium costs to the borrower in the form of a higher interest rate. Households can acquire IBI from OIC and take credit from financial institutions by their own effort.

Agricultural input: Households were also offered with an agricultural input coupon (AIC) that worth ETB 300. We told them to redeem this coupon at the local input supplier offices−cooperative unions through the arrangement we made by the project. Farmers can take the proportional amounts of chemical fertilizer, improved seeds and/or herbicides or pesticides using the coupon. Similar to the IBI, the repayment of the AI loan is postponed towards shortly after harvest. All loans also bear a 1% monthly risk-free interest rate until repaid.

[6] ETB (Ethiopian Birr), 1 USD = 27 ETB

Repayment structure: The repayment structure and the farmers' burden of debt depend on the level of the risk and the amount of loss realizations that farmers face. The total maturity value of the interlinked IBI-RCC and input is ETB 600 with a maturity value of ETB 636 over six months period. Farmers were required to repay back a maximum of ETB 636 under a full rainfall with no trigger of insurance. On the other hand farmers can earn a maximum of ETB 698 in the form of payout (i.e., ETB 1334−636 = ÈTB 698) under a 100 percent trigger that implies a complete loss of their harvest. All intermittent payout values are determined as per the linearly proportional indemnification (LPI) formula.

3.2.Sample size and power of the experimental design

The sample size in this study is mainly determined by the available budget. Yet, we have some freedom in the choice of the sampling strategy. As we randomized at the level of community known as 'garee' in our study area, we have a choice between putting many garees in our sampleand then survey a small amount of households per many garees, or to sample many households per few garees and then reduce the size of garees. In terms of power, given the total sample size, an increase in the size of garees (and hence involving less households per garee) is preferable, if intra-class correlations (ICC) are positive. However, if we increase the number of garees, the survey costs will increase due to larger distances. Moreover, the more garees we use in the study, it would be more "restricted". Since the product may not be actively marketed, we need a 'control' group. Taking all these conditions into account, we decided to sample on average about 35 households from each of the 47 garees. We show belowthat this sample size enables us to detect relatively small effect sizes. Following Djimeu and Houndolo (2016)we calculate the minimum detectable effect (MDE) of the cluster randomized controlled trials with individual-level outcomes with covariates as follows:

$$MDE(\delta) = \frac{t_1+t_2}{\sqrt{p(1-p)}}\sigma_y\sqrt{\left[\rho + \frac{1-\rho}{n}\right](1-R^2)} \tag{11}$$

where MDE represents the minimum detectableeffect size; t_1 is the t-value corresponding to the desired significance level of the test; t_2 is the t-value corresponding to the desired power of the experimental design; p is the proportion of individuals assigned to the treatment group; σ_y is the standard deviation of the outcome variable; ρ is the intra-class correlation (ICC) coefficient; n is the number of individuals per cluster and R^2 is outcome variance. Table 1 presents the description and values of these parameters in the way we used them to determine the sample size and power of the experimental design.

Table 1:Parametersused in determining sample size and power of the experimental design

Parameters	Description of parameters	Value
α	Significance level	0.05
β	Power of the test	0.80
$Tail$	One-tail or two-tail test	2
t_1	T-value corresponding to the desired significance level of the test	1.96
t_2	T-value corresponding to the desired power of the experimental design	0.84
σ_y	Standard deviation of the outcome variable	0.43
J	Number of clusters of the treatment and control group	47
ρ	Intra-class correlation (ICC) coefficient	0.047
p	Proportion of individuals assigned to the treatment groups	0.25
n	Average sample size per cluster	35
R^2	Proportion of outcome variance explained by the covariates	0.079
δ	Minimum detectable effect	0.107

Note: The parameters used in determining sample size and powers of the experimental design in Table 1 are based on the assumptions of cluster randomized controlled trials with individual-level outcomes with covariates (Djimeu and Houndolo 2016). Since we are initially uncertain about the direction of the effect of the treatments on uptake, we used a two-tail test. We set the significance level at 0.05 and the desired power of the test at 0.8. The values of $t_1$and t_2(i.e., t_α and $t_{1-\beta}$) used are 1.96 and 0.84, respectively. Due to budget constraints, we decided to involve about 1660 participants constituting 47 clusters with sample size of 35 individuals per cluster. In total, we included 1661households divided into thecontrol group and three treatment arms: IBI, IBI interlinked with credit and IBI interlinked with credit and agricultural inputs. The standard deviation of the outcome variable uptake is considered as $\sigma_y = 0.43$ based on the variance $\sigma_y^2 = p(1-p)$ where p, the proportion of individuals assigned to each of the treatment group is0.25 in our data. Similarly, we considered an ICC coefficient of 0.047 and an $R^2 = 0.079$, both based on the actual data of the experiment. Based on these parameters, the estimated MDE in this study is 0.107. Several parameters are relatively standard, such as power (which we set at 80%), and significance level (which we set at 5%). Our power analysis is based on 3ie Sample size and minimum detectable effect calculator© developed in-house and available online as*3ie Sample size and minimum detectable effect calculator*©, so that readers canrun their own power analyses.

3.3.RCT experiment

We conducted a randomized controlled trial (RCT) with a randomly selected 1661 households from two kebeles in the Rift Valley zone of Ethiopia. Fromeach kebele, we randomly selected worker groups knows as '*garees*'.We invited 50 garees (35 from Desta Abjata and 15 garees from Qamo Garbi kebele) to come with lists of their members. Through kebele leaders, we arranged training at the Farmers' Training Center (FTC). From these, 47 garees have shown up on the training. We collected lists of members from all garee leaders. All households in the two kebeles were members of a *garee*, and there is no a household who has a multiple membership in different garees. We used group level randomization to randomly assign the 47 garees into one of the following four groups: Control group (T_1), standalone insurance group (T_2), interlinked insurance with credit group (T_3), and interlinked insurance with credit and agricultural input group (T_4). We preferred randomizing treatments and control at the group level rather than at the individual level to mitigate concerns about fairness. In our case if farmers in the same neighborhood area were assigned to different treatments there could have been resentment from farmers. Our RCT design is an encouragement design. The randomization was specifically undertaken as follows. First, based on random lottery basis, we kept one-fourth of the garee leaders as controls. We label the control group as group T_1. This group has got no encouragement to access insurance, credit or input from the intervention. But they can buy the standard insurance from OIC by their own. Second, we assigned the next one-fourth of the households into IBI group (T_2). Garees assigned to T_2 were those who draw the card labelled with 'IBI'. We informed group T_2 garees that their members will get ETB 100 insurance policy from OIC. In addition, like any households, members can buy insurance from OIC by their own.Thirdly, we assigned the next one-fourth of the garees into interlinked IBI with credit. Garees assigned to T_3 were those who draw the card which was labelled with 'IBI+ILC'. We informed group T_3 that their members will get ETB 100 insurance policy and ETB 200 credit through the intervention. In addition, members can also buy any amount of insurance from OIC or acquire any amount of credit from financial institutions by their own effort. Fourthly, we assigned the final one-fourth of the garees into the interlinked insurance with credit and agricultural input group. These garees were those who draw the card labelled 'IBI+ILC+AIC'. We informed group T_4 households that their members were allowed to get ETB 100 insurance policy, ETB 200 risk-contingent credit and an agricultural input coupon worth of ETB300 that can be redeemed at input suppliers' office (cooperative unions). Members of this group took fertilizer and improved seed varieties from the suppliers showing their coupon.

4. Balancing tests

Inmeasuring and interpreting the effects of treatments, various studies show that randomization ensures unbiased allocation of treatments to the study participants. However, randomization alone cannot provide the guarantee for a particular trial that the study participants in each treatment group will have similar characteristics (Schulz, Altman and Moher 2010). This is very important in light of potential non-random non-compliance.

Table 2a: Balance tests on socio-economic variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatments	Age	Gender	Education	Family size	Marital status	2015 drought	2016 drought
T_2	−0.175	0.000	0.820***	0.913***	0.024**	−0.192***	0.192***
	(0.603)	(0.023)	(0.233)	(0.207)	(0.011)	(0.021)	(0.021)
T_3	−0.059	0.010	0.222	−0.002	−0.000	−0.056***	0.049**
	(0.605)	(0.023)	(0.234)	(0.208)	(0.011)	(0.021)	(0.021)
T_4	1.189*	0.022	0.680***	0.445**	0.012	-0.031	0.031
	(0.608)	(0.023)	(0.235)	(0.209)	(0.011)	(0.021)	(0.021)
Constant (T_1)	35.764***	0.862***	3.850***	5.833***	1.000***	0.957***	0.040***
	(0.427)	(0.016)	(0.165)	(0.147)	(0.008)	(0.015)	(0.015)
$T_2 = T_3$	0.848	0.676	0.011	0.000	0.029	0.000	0.000
$T_2 = T_4$	0.025	0.348	0.550	0.026	0.294	0.000	0.000
$T_3 = T_4$	0.041	0.602	0.053	0.033	0.261	0.243	0.397
Observations	1,661	1,661	1,661	1,659	1,661	1,661	1,661
R-squared	0.004	0.001	0.010	0.016	0.004	0.054	0.057

Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Test gives p-values of Wald tests referring to groups specified after the test.

Hence, it is important to verify whether randomization resulted in similar groups in terms of observables. Such test, at best, secures unbiased treatment allocation, but not necessary balance (Tu, Shalay and Pater 2000). Since in practice following randomization, some important covariates may not be balanced between treatment groups especially when the sample size is small; it is therefore a usual practice in randomized controlled trial experiments to present baseline information on prognostic factors (Altman 1985). This practice allows for quick judgment of the success or otherwise of the randomization procedure, and as a result, provides basic information on which confidence on subsequent treatment comparison hinges. In this study, we have undertaken, balancing tests by estimating OLS models, regressing household observables on treatment group dummies and a constant (see Tables 2a and 2b below).

The constant term reflects the comparison group, and the estimated coefficients indicate whether the other groups significantly differ from the comparison group. We also examine whether there are differences between these other groups by performing Wald tests. In this regard, careful selection of covariates and baseline tests of significance to determine which covariate to include in the model are important. In Table 2a, we present regression results for some demographic variables including age (in years), gender (= 1for male; 0 for female), marital status (=1 for married; 0 for non-married), education (years of schooling), family size and drought dummies (=1 for experiencing drought in 2015 and/or 2016).

Table 2b presents similar tests for households' amount of saving, amount of outstanding loan,size of land size owned by the household, a series of farming variables capturing quantities of certain crops produced in the last cropping season (maize, haricot, teff, sorghum, wheat, and barely); a measure of total land under cultivation, and a dummy taking value 1 if the household had any formal savings. In addition, we included the variable credit rationing whether the household is credit rationed (1=for credit rationed; 0 for not credit rationed)[7] in the analysis.

[7]We used a direct elicitation method (DEM) (Boucher et al.2009) to identify the credit rationing status of each household. With this method, we can identify credit constrained households according to their decision to borrow and the lender's decision to supply credit. The credit rationing module starts by asking whether the respondent has applied for a formal loan in the past five years. If so, it asks whether the application has been accepted. Households that have not applied for a formal loan indicate their reasons for not applying. According to their responses, all households can be categorized into one of four mutually exclusive groups: credit unconstrained, quantity (or supply-side) rationed, risk rationed, and transaction cost rationed. Households that apply for formal loans and receive them are categorized as unconstrained. However, if households applied for (more) credit at the prevailing interest rate and their application was rejected, they are classified as quantity rationed. If households have not applied for a formal loan in the past five years, because the bank branch is too far from their homes or the application procedure involves too much paperwork and waiting time, we categorize them as transaction cost

Randomization seems to have worked reasonably well.In terms of balance, as compared with the comparison group, we find that the average family size is somewhat larger in T_2 and T_4 groups.This group has also achieved a relatively higher education. Households in group T_2 and T_3 were also experienced a bit more drought. We also find some other slight imbalances upon comparing the coefficients of the various treatment arms to each other. Tables 2a and 2b suggest that the randomization has worked reasonably well, especially regarding crop production (see Table 2b). Farmers of the different treatment groups produce on average the same products. There are also some imbalances. Yet these small imbalances are not a reason of concern, and do not disqualify the randomization.

In analyzing the effects of treatments in experiments, those with statistically significant difference between groups are automatically accounted for in the analysis, and those that are not significant are ignored (Meinert 2012). However, the basic argument against the afore-mentioned approach is that, since study participants are randomly allocated to treatment groups in the first instance, then, any observed difference must have been due to chance. It then appears absurd to again test whether the observed difference is purely by chance or not, which is what the test of significance does. Ignoring baseline covariate tests that have prognostic influence but not significantly different between groups remains at odds with the correctness of the use of hypothesis testing approach for covariate selection. In fact, a significant imbalance will not matter if a factor does not predict outcome; whereas, a non-significant imbalance can benefit from covariate adjustment.

rationed. If instead households do not apply for loans because they do not want to offer their land, house, or other assets as collateral that might be taken by the bank, we consider them risk rationed. Some households that are able to borrow do not apply because they do not need credit; they are also credit unconstrained. Finally, households that would have applied for loan, had they known the bank would lend to them, are another group of supply-side rationed households. We sum together the risk- and transaction cost rationed households into a group of demand constrained households; then we sum the demand constrained households and supply constrained households into a larger group of credit constrained households.

Table 2b: Balance tests on socio-economic variables

Treatments	(1) Saving	(2) Loan	(3) Rationing	(4) Land	(5) Maize	(6) Teff	(7) Sorghum	(8) Wheat	(9) Consumption
T_2	−0.073**	−0.158***	−0.039	0.267	0.325	0.002	−0.148***	0.421	33.473
	(0.032)	(0.034)	(0.028)	(0.354)	(0.649)	(0.016)	(0.049)	(0.261)	(20.722)
T_3	0.115***	0.060*	−0.009	−0.127	1.841***	0.046***	−0.148***	0.048	19.614
	(0.033)	(0.034)	(0.028)	(0.356)	(0.652)	(0.017)	(0.049)	(0.261)	(20.809)
T_4	0.124***	−0.001	−0.039	2.664***	2.773***	0.034**	−0.143***	−0.511*	32.361
	(0.033)	(0.034)	(0.028)	(0.357)	(0.655)	(0.017)	(0.049)	(0.263)	(20.912)
Constant	0.607***	0.474***	0.224***	7.767***	16.210***	0.000	0.148***	0.981***	474.297***
	(0.023)	(0.024)	(0.020)	(0.251)	(0.459)	(0.012)	(0.035)	(0.184)	(14.679)
$T_2 = T_3$	0.000	0.000	0.288	0.269	0.020	0.009	1.000	0.153	0.505
$T_2 = T_4$	0.000	0.000	0.985	0.000	0.000	0.054	0.921	0.000	0.958
$T_3 = T_4$	0.777	0.076	0.281	0.000	0.156	0.494	0.921	0.034	0.543
Observations	1,661	1,661	1,661	1,661	1,661	1,661	1,661	1,660	1,659
R-squared	0.030	0.027	0.002	0.047	0.014	0.007	0.008	0.008	0.002

Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Test gives p-values of Wald tests referring to groups specified after the test.

5. Empirical strategy

We estimate the effects of the standalone and the interlinked treatments on IBI adoption decision of the households as follows:

$$Z_{ij} = \tau_0 + \tau_1 T_1 + \tau_2 T_2 + \tau_3 T_3 + \tau_i X_{ij} + \varepsilon_{ij} \qquad (12)$$

whereas Z_{ij} represents the uptake of IBI, τ_0 represents the constant indicating IBI uptake of the control group (i.e., households who were not encouraged or not participated on promotion); the coefficientsτ_1, τ_2 and τ_3 measure the increase in uptake due to IBI, first level interlinkage and second level interlinkage, respectively. Further, T_1 is an indicator variable for assignment to treatment 1 (IBI), taking the value 1 for households assigned to treatment 1 and 0 for the others; $T_2$is an indicator variable for assignment to treatment 2 (IBI+ILC) taking the value 1 for households offered with IBI+ILC and 0 for the others;$T_3$is an indicator variable for assignment to treatment 3 (IBI+ILC+AIC) taking the value 1 for households offered with IBI+ILC +AIC and 0 for the others. Similarly,X_i is a vector of baseline characteristics or covariates that affect uptake of IBI including household demographic characteristics such as age, gender, level of education and family size; drought experiences of the household, land size, saving, indebtedness and credit rationing status of the household;and ε_i is the stochastic term capturing all unobservable factors in the data. Hence, the parameter $\tau_i$measures the effect of the different covariates on the uptake of IBI.

5.1.Impact estimation strategy

Our impact analysis focuses on assessing the welfare effects of the innovative interlinked insurance-credit-input intervention on household production and consumption behaviour. The returns to effective implementation of the innovative interlinked insurance-credit-input intervention can be expected to be substantial. By enhancing household investment in high-risk high-return production inputs, such intervention can enhance productivity, smooth consumption and improve the welfare of the smallholders. Thus we evaluate the impact of the innovative interlinked insurance-credit-input intervention on observable outcome variables including enhanced investment in high-risk high-return inputs as well as weekly consumption. We use two approaches, namely, the intent-to-treat (ITT) and the local average treatment (LATE).

5.2.Post-treatment analysis (Intent-to-treat (ITT))

In the ITT analysis, we regress the outcome variables on the randomized groups irrespective of their uptake status. Let T_1 represent the control group (i.e., households who were randomly assigned to the group whose members were not encouraged or not allowed to participate in the interlinked credit-insurance-input intervention). Note that these groups of households in principle can buy the conventional IBI from OIC by their own effort. Similarly, T_2 , T_3 , and T_4 representrandomizationdummies for groups assigned to the promoted IBI, the promoted IBI interlinked with credit and the promoted IBI interlinked with credit and input, respectively. In the first instance, we undertake the ITT analysis. Due to the RCT design, post-treatment outcomes are unbiased. The ITT compares the outcome variables in the treatment groups (i.e.,T_2,$T_3$and T_4) to the outcome variable(s) of the control group (i.e T_1). For each of the outcome variables, we estimate the ITT effects based on both the post-treatment (single) and difference-in-difference (double) outcomes.

Our ITT model specification based on single post-treatment data can be specified as follows:

$$Y_{ij} = \gamma_0 + \gamma_1 T_1 + \gamma_2 T_2 + \gamma_3 T_3 + \gamma_4 T_4 + \beta X_{ij} + \varepsilon_{ij} \tag{13}$$

where Y_{ij} represent outcome variables including value of investment in high-risk high-return agricultural inputs (i.e., value of investment in improved seed varieties, chemical fertilizer and pesticide/herbicide) as well as value of weekly foodconsumption), productivity, subjective well-being and shock-copying ability; γ_0 the constant term; T_1, T_2, T_3 and T_4 are randomization dummies as defined above taking values (=1 for households assigned to the specific group and 0 for others); X_{ij} represents household characteristics included to increase the efficiency of the model; and ε_{ij} is stochastic error term. Hence, γ_1, γ_1 , γ_3 and γ_4 measure the relative intent-to-treat effect of the conventional IBI, promoted IBI, the promoted IBI interlinked with credit and the promoted IBI interlinked with credit and input, on the outcome variables, respectively.We estimate Eq. (13) using only the single post-treatment data. Given the random assignment to the treatment, $E(\varepsilon_{ij}/T_{ij} = 0)$, so OLS estimates of $\gamma_1, \gamma_2, \gamma_3$ and γ_4 are unbiased, as long as attrition is not differential.

Further, since we have both the baseline and end-line data for some of the outcome variables, we can estimate the impact of the intervention using the difference-in-difference as follows:

$$Y_{ij} = \omega t_2 + \gamma_0 + \gamma_2 T_2 + \gamma_3 T_3 + \gamma_4 T_4 + \gamma_5 (t_2 T_2) + \gamma_6 (t_2 T_3) + \gamma_7 (t_2 T_4) + \beta_i X_{ij} + \varepsilon_{ij} \tag{14}$$

where $t_2$or *Post* (as used in the estimation) is the indicator variable for the end-line survey taking the value 1 for end-line survey and 0 for the baseline survey;γ_0, Y_{ij}, T_1; T_2, T_3 and T_4 as well as X_{ij} and $\varepsilon_{ij}$are as defined in eq. (13). Hence, γ_5, γ_6 and γ_7 are our coefficient of interest or DIDs that measure the relative intent-to-treat *overtime* effect of the three components of the intervention on the outcome variables compared to the control group. This means these coefficients measure whether the impact of T_2, T_3 and T_4 is higher than the impact of T_1 on the outcome variables. Here, we undertake Wald tests for comparing T_2 with T_3 and T_4 as well as for comparing T_3 with T_4.

5.3.Local average treatment effect (LATE)

Next, we will undertake a local average treatment effect (LATE) analysis for both the single post-treatment and difference-in-difference effects. LATE depends on the instrumental variable (IV) approach and uses the 2SLS estimator. It uses the actual uptake of a household(rather than mere assignment to treatments) from the group randomly assigned. Let T_2,T_3, and T_4 represent assignment to the treatment dummies for households assigned to the respective groups andZ_{ij}represent actual taken-up of the products: the promoted IBI, the promoted IBI interlinked with credit and the promoted IBI interlinked with credit and input, respectively. We estimate LATE based on the post-treatment data and using a two-stage least square (2SLS) as follows:

$$Z_{ij} = \tau_0 + \tau_1 T_1 + \tau_2 T_2 + \tau_3 T_3 + \tau_4 T_4 + \tau_i X_{ij} + \varepsilon_{ij} \quad (15a)$$

$$Y_{ij} = \gamma_0 + \gamma_1 \hat{Z}_{ij} + \gamma_i X_{ij} + \varepsilon_{ij} \quad (15b)$$

where Z_{ij} represents uptake (= 1 for those households who take-up after the intervention and 0 for others); γ_0, T_1; T_2, T_3 and T_4 as well as X_{ij} and $\varepsilon_{ij}$are as defined above. In eq. (15b), T_2, T_3 and T_4 serve as external instruments for uptake (Z_{ij}).

Similar to the procedures we followed in eq. (14), we can estimate LATE using difference-in-difference for the outcome variables for which we have both the baseline and end-line data as follows:

$$Z_{ij} = \tau_0 + \tau_1 T_1 + \tau_2 T_2 + \tau_3 T_3 + \tau_4 T_4 + \tau_i X_{ij} + \varepsilon_{ij} \quad (16a)$$

$$Y_{ij} = \gamma_0 + \pi t_2 + \gamma \hat{Z}_{ij} + \delta(t_2 \hat{Z}_{ij}) + \beta_1 X_{ij} + \varepsilon_{ij} \quad (16b)$$

where δ measures the DID for LATE. All variables are as defined before. Again T_2, T_3 and T_4 serve as external instruments for uptake (Z_{ij}) in eq. (16b).

6. Results

6.1.Impact on household investment in high-risk high-return inputs

Table5 presents the effects of the interlinked intervention on households' total value of investment in high-risk high-return agricultural inputs. Columns 1−4 report the ITT level effect (i.e., the average effect of being assigned to a treatment group) on investment in inputs.Based on the single post-treatment outcome, reported investments in inputs are significantly higher for the insurance interlinked with both credit and inputs. Controlling for all covariates, interlinking IBI with credit as well as interlinking IBI with both credit and agricultural inputs increasetotal investment in high-risk high-return inputs by ETB 409 and ETB 429, respectively (see Columns 1−2 in Table 5). Further, based on the DID results, the estimated ITT effect shows that interlinking IBI with both credit and input has a significant effect on household investment in high-risk high-return inputs (see Columns 3−4 in Table 5).

Table 5: Impact on household total investment in high-risk high-return inputs

Variables	ITT				LATE			
	Post treatment (single outcome)		Difference-in-difference		Post treatment (single outcome)		Difference-in-difference	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IBI	-129.700	-255.052	-30.386	-127.601				
	(254.081)	(244.389)	(246.686)	(230.588)				
IBI+ILCC	353.021	409.448*	129.276	187.030				
	(211.216)	(226.417)	(158.737)	(170.940)				
IBI+ILC+AIC	827.681***	428.594*	168.993	-213.372				
	(229.116)	(213.360)	(151.315)	(169.409)				
Post (=1 for end line; =0 for baseline)			338.229***	321.247***			904.424***	478.419***
			(24.673)	(25.314)			(172.166)	(149.881)
Post*IBI			-99.314*	-99.317*				
			(56.390)	(56.510)				
Post*(IBI+ILC)			223.745*	222.120*				
			(124.095)	(124.890)				
Post*(IBI+ILC+AIC)			658.688***	658.685***				
			(106.909)	(107.101)				
Uptake (=1 for uptakers; =0 for non-uptakers)					2,291.742***	1,490.010***	2,087.007***	564.128
					(436.605)	(393.812)	(560.853)	(492.106)
Post*uptake							-1,355.565**	124.514
							(571.276)	(498.038)
Age		17.986**		16.984**		17.431***		16.727***
		(8.565)		(7.184)		(6.112)		(3.909)
Gender		267.759		301.380		387.479**		353.617***
		(331.276)		(263.264)		(171.101)		(109.426)
Married		-390.263**		-414.194***		-368.546		-426.979**
		(185.862)		(103.782)		(296.606)		(189.690)
Education (years)		7.223		15.984		1.933		11.755
		(25.260)		(21.247)		(16.733)		(10.702)
Family size		21.327		16.995		12.480		11.951
		(22.035)		(20.407)		(16.275)		(10.409)
2015 drought		-495.271		-42.459		-318.487		17.754
		(1,309.734)		(846.263)		(462.908)		(296.047)
2016 drought		-331.381		49.972		-316.994		28.092

		(1,170.023)		(784.096)		(468.187)		(299.423)
Land size		153.722***		148.298***		153.414***		146.014***
		(36.038)		(36.697)		(9.773)		(6.250)
Saving		-455.356*		-459.012*		-441.335***		-434.625***
		(228.444)		(261.743)		(124.081)		(79.354)
Outstanding loan		-17.158		67.222		102.395		135.021**
		(165.542)		(152.591)		(102.614)		(65.626)
Credit rationed		-134.908		-64.264		-89.255		-37.743
		(172.000)		(154.895)		(133.189)		(85.179)
Constant	2,248.598***	1,201.740	1,910.369***	443.525	1,872.725***	676.647	1,399.474***	192.462
	(131.875)	(1,162.071)	(112.361)	(720.955)	(131.580)	(626.589)	(162.420)	(423.804)
Observations	1,661	1,659	3,322	3,318	1,661	1,659	3,322	3,318
R-squared	0.033	0.199	0.039	0.219		0.170		0.208

Note: The dependent variable in estimations reported in Table 5 is the total investment in high-risk high-return inputs including chemical fertilizer, improved seed variety and investments in pesticides and/or herbicides. Dependent variable is measured in Ethiopian Birr (ETB). Columns 1-2 report the post-treatment (single) effects of the intervention estimated using eq. (13), with and without controls, respectively. Results reported under columns 1-2 are estimated using OLS. Columns 3-4 report the ITTeffects of the intervention with and without controls, estimated using eq. (14), respectively. Resultsreported under columns 3-4 are estimated using difference-in-difference. Columns 5-6 report the LATE (single post-treatment effects) of the intervention with and without controls, estimated using eq. (15a & 15b), respectively. Uptake is instrumented by randomization dummies. Similarly, columns 7-8 present the IV-based LATE (difference-in-difference effects) of the intervention with and without controls, estimated using eq. (16a & 16b), respectively, where the actual uptake is again instrumented by randomization dummies. Standard errors are clustered at the *garee* level, and reported in parentheses.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Based on the single post-treatment outcome, the DID estimates show that controlling for all potential covariates, interlinking IBI with both credit and agricultural inputsincreases the investment in high-risk high-return inputs by ETB 659 (see Column 4 in Table 5).

Table 5 also reports the local average treatment effect (LATE) of the interlinked intervention on household total investment in high-risk high-return inputs. First, results presented under Column 5−6 were estimated for the single post-treatment outcome using 2SLS in which the actual uptake is instrumented by treatment dummies. Due to random treatment and low level of attrition in the data, post-treatment outcomes were unbiased. The estimated results show that, controlling for all covariates, for actual adopters, the intervention has increased total household investment in high-risk high-return inputs by ETB 1490, and this is highly significant at 1 percent level. The differential impact between ITT and LATE estimates is due to the reason that LATE estimates are for real adopters while ITT estimates are only for being assigned to treatments irrespective of the uptake status.

6.1.1. Impact on investment in chemical fertilizer

Table 6 further presents the disaggregated effect of the interlinked intervention on household investment in chemical fertilizer.Based on the difference-in-difference method of estimating the ITT effects, Column 4 in Table 6 shows thatinterlinking IBI with both credit and inputs has statistically significant effect in increasing the purchase of fertilizer at 1 percent level. Controlling for all potential covariates, interlinkagethe provision IBI with both credit and inputs increasesinvestment in chemical fertilizer by ETB 402(see Column 4 in Table 6).Table 6 also shows the LATE results of the 2SLS estimationsfor both the single post-treatment and difference-in-difference.Column 5−6indicates that the interlinked intervention has significantly increased the post-treatment investment in chemical fertilizer by ETB 595.

Table 6: Impact of interlinked insurance-credit-input on household investment in high-risk high-return input (chemical fertilizer)

Variables	ITT				LATE			
	Post treatment (single outcome)		Difference-in-difference		Post treatment (single outcome)		Difference-in-difference	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IBI	-157.752	-216.251*	-147.578	-186.544*				
	(144.904)	(127.524)	(113.107)	(101.040)				
IBI+ILC	81.211	95.378	47.954	66.434				
	(152.601)	(146.963)	(122.193)	(117.027)				
IBI+ILC+AIC	454.704***	196.700	52.770	-171.997*				
	(140.376)	(122.382)	(99.115)	(99.255)				
Post (=1 for end line; =0 for baseline)			319.100***	309.263***			670.701***	407.783***
			(18.223)	(18.465)			(87.488)	(71.920)
Post*IBI			-10.174	-10.150				
			(36.467)	(36.556)				
Post*(IBI+ILC)			33.257	31.536				
			(32.635)	(32.624)				
Post*(IBI+ILC+AIC)			401.934***	401.958***				
			(45.172)	(45.270)				
Uptake (=1 for uptakers; =0 for non-uptakers)					1,148.407***	594.968***	1,112.251***	176.793
					(236.845)	(198.739)	(285.005)	(236.136)
Post*uptake							-896.161***	17.943
							(290.301)	(238.982)
Age		11.046**		9.813**		11.100***		9.985***
		(4.285)		(3.677)		(3.084)		(1.875)
Gender		385.923***		288.367***		425.017***		296.580***
		(111.321)		(92.353)		(86.347)		(52.508)
Married		-220.570**		-211.622***		-221.576		-234.481**
		(95.392)		(78.504)		(149.683)		(91.022)
Education (years)		-4.462		2.080		-7.027		-0.514
		(9.375)		(8.041)		(8.445)		(5.135)
Family size		22.657**		17.391*		17.686**		13.944***
		(10.876)		(9.241)		(8.213)		(4.995)
2015 drought		45.691		163.426		133.068		190.548
		(534.784)		(388.867)		(233.609)		(142.057)
2016 drought		147.358		179.699		135.421		143.907

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		(486.017)		(355.423)		(236.273)		(143.677)
Land size		91.314***		80.490***		92.499***		80.052***
		(17.089)		(14.804)		(4.932)		(2.999)
Saving		-114.356		-111.956		-105.940*		-93.640**
		(105.488)		(96.011)		(62.618)		(38.078)
Outstanding loan		21.558		24.127		76.440		58.931*
		(86.586)		(72.518)		(51.785)		(31.490)
Credit rationed		0.528		16.118		17.182		25.857
		(127.595)		(111.871)		(67.214)		(40.873)
Constant	1,471.564***	138.371	1,152.464***	-81.039	1,245.615***	-121.798	832.551***	-206.472
	(85.785)	(451.664)	(68.892)	(322.939)	(71.378)	(316.211)	(82.536)	(203.361)
Observations	1,661	1,659	3,322	3,318	1,661	1,659	3,322	3,318
R-squared	0.042	0.289	0.078	0.315		0.249		0.296

Note: The dependent variable in estimations reported in Table 6 is the value of investment inchemical fertilizermeasured in Ethiopian Birr (ETB). Columns 1-2 report the post-treatment (single) effects of the intervention estimated using eq. (13), with and without controls, respectively. Results reported under columns 1-2 are estimated using OLS. Columns 3-4 report the ITTeffects of the intervention with and without controls, estimated using eq. (14), respectively. Resultsreported under columns 3-4 are estimated using difference-in-difference. Columns 5-6 report the LATE (single post-treatment effects) of the intervention with and without controls, estimated using eq. (15a & 15b), respectively. Uptake is instrumented by randomization dummies. Similarly, columns 7-8 present the IV-based LATE (difference-in-difference effects) of the intervention with and without controls, estimated using eq.(16a & 16b), respectively, where the actual uptake is again instrumented by randomization dummies. Standard errors are clustered at the *garee* level, and reported in parentheses.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

6.1.2. Impact on adoption of improved seed varieties

We further disaggregate the impact of the intervention in investment in inputs. The effect of the interlinked intervention on household investment to adopt improved seed varieties is presented in Table 7. Under Column 1−2, the ITT effects were presented for the single post-treatment outcome. Results reveal that controlling for all covariates; interlinking IBI with credit significantly increases household investment in improved seeds by ETB 314 based on the single post-treatment outcome (see Column 2 in Table 7).

Further, Column 3−4 in Table 7 continues to present the ITT effects of the intervention using the difference-in-difference method. Results show that the second level interlinkage, that is interlinking IBI with credit and inputs, has a statistically significant effect in increasing households' investment in adoption of improved seeds. Interlinking IBI with both credit and input provisions increases the investmentin improved seeds by ETB 257 and this is significant at 1 percent level (see Column 4 in Table 7).

The IV regression results estimated using the 2SLS for the LATE is reported in Table 7under columns 5−6 for the single post-treatment and under Columns 7−8 for the difference-in-difference. Estimated results show that the interlinked intervention has a statistically significant impact on investment in adoption of improved seeds based on the post-treatment outcome. The average increase in investment to purchase improved seeds is ETB 895 for the single post-treatment. The results are significant at 1 percent level.

Again it is important to note that the impact estimate based on LATE is higher than the estimate for ITT since the former is estimated for real adopters. The LATE estimates are also based on the instrumental variable (IV) regressions in which assignment to treatments are used as instrument for actual uptake. This indicates that though mere provision of IBI or provision of IBI with only credit may not be effective, intensively interlinking IBI with credit and inputs indeed causally increases investment in high-risk high-return inputs including both chemical fertilizer and improved seed variety.

Table 7: Impact of interlinked insurance-credit-input on household investment in high-risk high-return input (improved seed)

Variables	ITT				LATE			
	Post treatment (single outcome)		Difference-in-difference		Post treatment (single outcome)		Difference-in-difference	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IBI	28.052	-38.801	117.193	58.942				
	(122.397)	(134.309)	(155.091)	(149.795)				
IBI+ILC	271.810*	314.070**	81.322	120.595				
	(154.306)	(147.760)	(67.740)	(80.160)				
IBI+ILC+AIC	372.977***	231.893*	116.223	-41.375				
	(137.543)	(131.484)	(69.556)	(89.502)				
Post (=1 for end line; =0 for baseline)			19.129	11.984			233.723**	70.636
			(12.282)	(12.999)			(107.254)	(101.684)
Post*IBI			-89.140	-89.167				
			(66.957)	(67.073)				
Post*(IBI+ILC)			190.487	190.583				
			(120.889)	(121.358)				
Post*(IBI+ILC+AIC)			256.753***	256.727***				
			(87.414)	(87.563)				
Uptake (=1 for uptakers ; =0 for non-uptakers)					1,143.335***	895.043***	974.755***	387.335
					(266.312)	(262.186)	(349.396)	(333.862)
Post*uptake							-459.404	106.572
							(355.889)	(337.887)
Age		6.941		7.171*		6.331		6.743**
		(5.004)		(4.038)		(4.069)		(2.652)
Gender		-118.163		13.013		-37.538		57.037
		(300.147)		(197.745)		(113.913)		(74.238)
Married		-169.692		-202.571***		-146.970		-192.499
		(145.285)		(69.265)		(197.469)		(128.693)
Education (years)		11.685		13.904		8.960		12.270*
		(22.694)		(15.807)		(11.141)		(7.260)
Family size		-1.331		-0.396		-5.207		-1.993
		(15.024)		(12.817)		(10.836)		(7.062)
2015 drought		-540.962		-205.885		-451.555		-172.794
		(785.317)		(471.468)		(308.188)		(200.849)
2016 drought		-478.739		-129.727		-452.415		-115.815

		(694.266)		(445.965)		(311.702)		(203.139)
Land size		62.408**		67.808***		60.915***		65.962***
		(24.276)		(24.239)		(6.507)		(4.240)
Saving		-341.000**		-347.057*		-335.394***		-340.985***
		(134.460)		(175.766)		(82.608)		(53.837)
Outstanding loan		-38.716		43.095		25.955		76.090*
		(97.611)		(94.885)		(68.317)		(44.523)
Credit rationed		-135.436		-80.382		-106.437		-63.600
		(90.175)		(78.996)		(88.672)		(57.789)
Constant	777.033***	1,063.369	757.905***	524.564	627.110***	798.445*	566.923***	398.933
	(50.592)	(740.992)	(48.725)	(438.923)	(80.259)	(417.161)	(101.183)	(287.524)
Observations	1,661	1,659	3,322	3,318	1,661	1,659	3,322	3,318
R-squared	0.015	0.085	0.011	0.100		0.080		0.098

Note: The dependent variable is the value of investment in improved seed variety.Columns 1-2 report the post-treatment (single) effects of the intervention estimated using eq. (13), with and without controls, respectively. Results reported under columns 1-2 are estimated using OLS. Columns 3-4 report the ITTeffects of the intervention with and without controls, estimated using eq. (14), respectively. Resultsreported under columns 3-4 are estimated using difference-in-difference. Columns 5-6 report the LATE (single post-treatment effects) of the intervention with and without controls, estimated using eq. (15a & 15b), respectively. Uptake is instrumented by randomization dummies. Similarly, columns 7-8 present the IV based LATE (difference-in-difference effects) of the intervention with and without controls, estimated using eq. (16a & 16b), respectively, where the actual uptake is again instrumented by randomization dummies. Standard errors are clustered at the *garee* level, and reported in parentheses.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

6.2.Impact on consumption

Table8 presents results of the effect of the interlinked intervention on households' expenditure for weekly consumptions. The ITT level effects are reported under Columns 1−4. Based on the post-treatment (single) outcome, the OLS estimates show that interlinking IBI with credit increases expenditure on weekly consumption by ETB 76, while further interlinking IBI provision with credit and agricultural inputs increases household expenditures on weekly consumption by ETB 91. Both results are significant at 1 percent level after controlling for all covariates (see column 2 in Table 8).The double difference ITT estimates are also reported under Column 3−4 in Table 8. Estimated results show that all the three treatments have a statistically significant effect on household consumption (see column 4 in Table 8). Controlling for all covariates, the standalone IBI has increased weekly consumption expenditure by ETB 40. Similarly, interlinking IBI with credit increases household consumption expenditure by ETB 54, while further interlinking IBI with both credit and input increases weekly consumption expenditure by ETB 96.

Finally, the IV-based 2SLS estimations of the impacts of the intervention on consumption are presented under columns 5-8 in Table 8. LATE results reveal that the overall intervention has statistically significant impact in increasing household expenditure on consumptions. The LATE estimates based on the single post-treatment data show that the intervention has increased weekly consumption for actual adopters by ETB 292. This result is statistically significant at 1 percent level. The estimation is also based on the 2SLS that helps to control for the biases arising from time-invariant heterogeneity. Hence, it is evident that the intervention has casually increased households' weekly consumption expenditures.

Table 8: Impact of interlinked insurance-credit-input on household weekly food consumption

Variables	ITT				LATE			
	Post treatment (single outcome)		Difference-in-difference		Post treatment (single outcome)		Difference-in-difference	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IBI	73.699	27.795	33.473	-9.694				
	(47.065)	(36.152)	(44.679)	(34.475)				
IBI+ILC	74.704**	76.160**	19.614	21.498				
	(29.537)	(31.728)	(28.311)	(31.141)				
IBI+ILC+AIC	129.344**	90.710**	32.361	-3.718				
	(48.187)	(38.518)	(39.259)	(33.818)				
Post (=1 for end line; =0 for baseline)			2.453	2.260			90.661***	50.833*
			(2.906)	(3.122)			(28.448)	(26.489)
Post*IBI			40.226***	39.718***				
			(4.434)	(4.249)				
Post*(IBI+ILC)			55.090***	54.416***				
			(3.574)	(3.325)				
Post*(IBI+ILC+AIC)			96.983***	96.475***				
			(11.905)	(11.807)				
Uptake (=1 for uptakers; =0 for non-uptakers)					372.903***	292.225***	306.912***	158.944*
					(68.996)	(65.483)	(92.640)	(86.945)
Post*uptake							-146.554	-4.212
							(94.349)	(87.983)
Age		0.765		0.701		0.526		0.573
		(1.073)		(1.027)		(1.016)		(0.688)
Gender		58.765**		58.204**		85.990***		72.930***
		(26.279)		(24.056)		(28.451)		(19.267)
Married		7.004		10.496		22.659		17.473
		(56.113)		(56.033)		(49.319)		(33.390)
Education (years)		-4.749		-4.069		-4.958*		-4.316**
		(4.037)		(3.873)		(2.782)		(1.884)
Family size		26.492***		25.370***		25.869***		24.957***
		(4.110)		(3.964)		(2.706)		(1.833)
2015 drought		-118.995***		-99.224**		-88.402		-83.683
		(44.024)		(41.742)		(76.972)		(52.112)
2016 drought		-35.314		-20.101		-15.317		-11.196

		(47.361)		(45.765)		(77.850)		(52.706)
Land size		10.127**		9.431***		10.067***		9.263***
		(3.770)		(3.352)		(1.625)		(1.100)
Saving		-32.719		-30.497		-36.933*		-31.622**
		(23.087)		(22.227)		(20.632)		(13.975)
Outstanding loan		-13.929		-11.417		0.695		-2.420
		(23.357)		(21.618)		(17.063)		(11.558)
Credit rationed		-67.314*		-61.020		-59.785***		-56.459***
		(38.016)		(36.477)		(22.147)		(15.000)
Constant	476.750***	332.530***	474.297***	315.469***	442.617***	240.019**	410.691***	244.335***
	(22.336)	(69.471)	(22.193)	(66.673)	(20.793)	(104.189)	(26.850)	(74.676)
Observations	1,661	1,659	3,320	3,316	1,661	1,659	3,320	3,316
R-squared	0.019	0.132	0.018	0.128		0.128		0.123

Note: The dependent variable in estimations reported in Table 8 is the value of expenditure for weekly food consumptions measured in ETB. Columns 1-2 report the post-treatment (single) effects of the intervention estimated using eq. (13), with and without controls, respectively. Results reported under columns 1-2 are estimated using OLS. Columns 3-4 report the ITTeffects of the intervention with and without controls, estimated using eq. (14), respectively. Resultsreported under columns 3-4 are estimated using difference-in-difference. Columns 5-6 report the LATE (single post-treatment effects) of the intervention with and without controls, estimated using eq. (15a & 15b), respectively. Uptake is instrumented by randomization dummies. Similarly, columns 7-8 present the IV based LATE (difference-in-difference effects) of the intervention with and without controls, estimated using eq. (16a & 16b), respectively, where the actual uptake is again instrumented by randomization dummies. Standard errors are clustered at the *garee* level, and reported in parentheses.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

6.3.Impact on productivity

The results of the effect of the interlinked intervention on households' land productivity are presented in Table 9. The ITT level effects are reported under Columns 1−4. Based on the post-treatment (single) outcome, the OLS estimates show that interlinking IBI with credit increases land productivity 0.73. The result is significant at 5 percent level after controlling for all covariates (see column 2 in Table 9). The double difference ITT estimates are also reported under Column 3−4 in Table 9. Results show that interlinking IBI with both credit and agricultural inputs has statistically significant effect on land productivity (see column 4 in Table 9). Controlling for all covariates, interlinking IBI with both credit and input increases the productivity by 0.42.

Further,IV-based 2SLS estimations of the impacts of the intervention on productivity are presented under columns 5-8 in Table 9. LATE results reveal that the overall intervention has statistically significant impact in increasing land productivity. The LATE estimates based on the single post-treatment data show that the intervention has increased productivity by 2.1. This result is statistically significant at 1 percent level. The estimation is also based on the 2SLS that helps to control for the biases arising from time-invariant heterogeneity. Hence, it is evident that the intervention has casually increased land productivity.

Table 9: Impact of interlinked insurance-credit-input on productivity

Variables	ITT				LATE			
	Post treatment (single outcome)		Difference-in-difference		Post treatment (single outcome)		Difference-in-difference	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IBI	0.082	0.354	0.223	0.418*				
	(0.327)	(0.226)	(0.346)	(0.218)				
IBI+ILC	0.720	0.730**	0.560	0.550*				
	(0.438)	(0.316)	(0.416)	(0.294)				
IBI+ILC+AIC	0.124	0.570*	-0.293	0.137				
	(0.390)	(0.327)	(0.363)	(0.285)				
Post (=1 for end line; =0 for baseline)			-0.138***	-0.145***			0.106	0.593***
			(0.016)	(0.017)			(0.155)	(0.154)
Post*IBI			-0.141	-0.141				
			(0.183)	(0.183)				
Post*(IBI+ILC)			0.160	0.159				
			(0.098)	(0.099)				
Post*(IBI+ILC+AIC)			0.417***	0.417***				
			(0.046)	(0.046)				
Uptake (=1 for uptakers; =0 for non-uptakers)					1.021***	2.091***	0.699	2.596***
					(0.371)	(0.367)	(0.504)	(0.504)
Post*uptake							-0.498	-2.281***
							(0.513)	(0.511)
Age		0.009		0.007		0.007		0.005
		(0.006)		(0.005)		(0.006)		(0.004)
Gender		0.270		0.218		0.480***		0.375***
		(0.187)		(0.177)		(0.159)		(0.112)
Married		0.375		0.284		0.491*		0.367*
		(0.689)		(0.599)		(0.276)		(0.194)
Education (years)		-0.002		0.001		-0.004		-0.001
		(0.031)		(0.029)		(0.016)		(0.011)
Family size		0.009		0.010		0.006		0.010
		(0.021)		(0.020)		(0.015)		(0.011)
2015 drought		0.127		0.082		0.309		0.185
		(0.614)		(0.583)		(0.431)		(0.303)
2016 drought		-0.578		-0.220		-0.421		-0.101

		(0.643)		(0.594)		(0.436)		(0.307)
Land size		-0.148***		-0.146***		-0.152***		-0.151***
		(0.025)		(0.024)		(0.009)		(0.006)
Saving		-0.305		-0.301		-0.330***		-0.314***
		(0.191)		(0.187)		(0.116)		(0.081)
Outstanding loan		0.749***		0.758***		0.852***		0.827***
		(0.224)		(0.213)		(0.096)		(0.067)
Credit rationed		0.361		0.363*		0.422***		0.410***
		(0.218)		(0.208)		(0.124)		(0.087)
Constant	2.417***	2.242***	2.555***	2.574***	2.366***	1.692***	2.487***	1.895***
	(0.224)	(0.724)	(0.229)	(0.703)	(0.112)	(0.584)	(0.146)	(0.434)
Observations	1,661	1,659	3,322	3,318	1,661	1,659	3,322	3,318
R-squared	0.026	0.261	0.029	0.259		0.047		0.034

Note: The dependent variable in estimations reported in Table 9is the productivity measured as the ration of yield per land size. Columns 1-2 report the post-treatment (single) effects of the intervention estimated using eq. (13), with and without controls, respectively. Results reported under columns 1-2 are estimated using OLS. Columns 3-4 report the ITTeffects of the intervention with and without controls, estimated using eq. (14), respectively. Resultsreported under columns 3-4 are estimated using difference-in-difference. Columns 5-6 report the LATE (single post-treatment effects) of the intervention with and without controls, estimated using eq. (15a & 15b), respectively. Uptake is instrumented by randomization dummies. Similarly, columns 7-8 present the IV based LATE (difference-in-difference effects) of the intervention with and without controls, estimated using eq. (16a & 16b), respectively, where the actual uptake is again instrumented by randomization dummies. Standard errors are clustered at the *garee* level, and reported in parentheses.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

6.4. Impact on subjective well-being (SWB)

Table10 presents results of the effect of the interlinked intervention on households' subjective well-being estimated using ordered logit regressions. Column 1−2 presents the ITT level effects with and without control variables, respectively. Estimated results show the effect of interlinking in log-odd ratios. We find that the interlinked insurance improves SWB. Based on the post-treatment (single) outcome, the ordered logit estimates show that IBI uptake increases the log-odds of reporting higher SWB by 0.69. While the results of the ordered logit regressions are concise and more convenient for presentation purposes, their interpretation may not be straight forward. Rather, it requires exponentiation. By exponentiation, this means that IBI buyers are 1.99 ($\approx e^{0.69}$)times more likely to report higher SWB than lower SWB.This shows that uptake of IBI has a strong positive effect on SWB, presumably because insurance coverage reduces risk exposure for risk-averse buyers. Table 10 also shows that interlinking IBI with credit increasesthe log-odds of reporting higher SWB by1.23, while further interlinking IBI provision with credit and agricultural inputs increasesthe log-odds of reporting higher SWB by 2. All results are significant at 1 percent level after controlling for all covariates (see column 2 in Table 10).

Further, the IV-based 2SLS estimations of the impacts of the intervention on SWB were presented under columns 3-4 in Table 10. LATE results reveal that the overall intervention has statistically significant impact in increasing households' SWB. The LATE estimates based on the single post-treatment data show that the intervention has increased the log-odds of reporting higher SWB by 4.11. Exponentiating this, we find that participants of the interlinked intervention are by far more likely to report higher SWB than reporting lower SWB.

Since randomized treatment dummies were used as instruments for the potentially endogenous uptake of IBI, the coefficients on IBI, IBI+ILC and IBI+ILC+AIC measures the causal effects of insurance, insurance interlinked with credit and insurance interlinked with both credit and inputs, respectively, on SWB. This result is statistically significant at 1 percent level. The estimation is also based on the 2SLS that helps to control for the biases arising from time-invariant heterogeneity. Hence, it is evident that the intervention has casually increased households' subjective well-being.

Table 10: Impact of interlinked insurance-credit-input on SWB

Variables	ITT		LATE	
	Post treatment (single outcome)		Post treatment (single outcome)	
	(1)	(2)	(5)	(6)
IBI	0.698***	0.690***		
	(0.109)	(0.123)		
IBI+ILC	1.239***	1.232***		
	(0.163)	(0.162)		
IBI+ILC+AIC	2.006***	2.024***		
	(0.169)	(0.167)		
Uptake (=1 for uptakers; =0 for non-uptakers)			4.085***	4.111***
			(0.249)	(0.259)
Age		0.002		-0.001
		(0.006)		(0.004)
Gender		-0.423***		0.082
		(0.148)		(0.112)
Married		-0.319		-0.030
		(0.344)		(0.195)
Education (years)		0.006		0.009
		(0.020)		(0.011)
Family size		0.008		-0.006
		(0.018)		(0.011)
2015 drought		-0.984**		-0.105
		(0.429)		(0.304)
2016 drought		-0.965*		-0.275
		(0.496)		(0.307)
Land size		-0.009		0.000
		(0.013)		(0.006)
Saving		0.211		-0.003
		(0.157)		(0.081)
Outstanding loan		-0.134		0.085
		(0.132)		(0.067)
Credit rationed		-0.121		0.006
		(0.168)		(0.087)
Constant			0.996***	1.060**
			(0.075)	(0.411)
Observations	1,661	1,659	1,661	1,659
R-squared			0.256	0.250

Note: The dependent variable in estimations reported in Table 10 is the households' subjective well-being (SWB). SWB for individual household is measured on an ordinal scale basis following self-reported or stated perceptions about their well-being. Respondent households rated their well-being status on a Likert scale ranging from 1=very bad to 5= very good, responding to the question "On a scale ranging from 1−5, how do you rate your current well-being status?" Columns 1-2 report the post-treatment (single) effects of the intervention estimated using eq. (13), with and without controls, respectively. Results reported under columns 1-2 are estimated using Ordered Logit. Columns 1-2 report the ITT (single post-treatment effects) of the intervention with and without controls, estimated using eq. (13). Similarly, Columns 3-4 report the LATE (single post-treatment effects) of the intervention with and without controls, estimated using eq. (15a & 15b), respectively.Uptake is instrumented by randomization dummies. Standard errors are clustered at the *garee* level, and reported in parentheses.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

6.5. Impact on shock-copying ability (ScA)

The results of the effect of the interlinked intervention on households' shock-copying ability are presented in Table 11. The results are estimated using ordered logit regressions and presented inlog-odd ratios. Column 1−2 presents the ITT level effects with and without control variables, respectively. Results show that the interlinked insurance improves ScA.

Based on the post-treatment (single) outcome, the ordered logit estimates show that IBI uptake increases the log-odds of reporting higher ScA by 0.989. As we did in Section 6.5, interpretations of the ordered logit results require exponentiation. The above result thus shows that IBI buyers are 2.7 ($\approx e^{0.989}$) times more likely to report higher shock-copying ability than lower SCA. Consistent with our expectations, uptake of IBI has a strong positive effect on SCA of the households, presumably because insurance coverage reduces risk exposure for risk-averse buyers. Table 11 also shows that interlinking IBI with credit increases the log-odds of reporting higher ScA by 1.27, while further interlinking IBI provision with credit and agricultural inputs increases the log-odds of reporting higher SWB by 2.19. All these results are significant at 1 percent level after controlling for all covariates (see column 2 in Table 11).

The IV-based 2SLS estimations of the impacts of the interlinked intervention on ScA were presented under columns 3-4 in Table 11. LATE results reveal that the overall intervention has statistically significant impact on increasing households' ScA. The LATE estimates based on the single post-treatment data show that the intervention has increased the log-odds of reporting higher ScA by 2.79. Exponentiating this, we find that participants of the interlinked intervention are by far more likely to report higher ScA than reporting lower ScA.

As we explained in Section 6.5, since randomized treatment dummies were used as instruments for the potentially endogenous uptake of IBI, the coefficients on IBI, IBI+ILC and IBI+ILC+AIC measures the causal effects the three components of the intervention on ScA. This result is statistically significant at 1 percent level. The estimation is also based on the 2SLS that helps to control for the biases arising from time-invariant heterogeneity. Hence, it is evident that the insurance-credit-input interlinked intervention has casually increased households' shock-copying ability.

Table 11: Impact of interlinked insurance-credit-input on shock-copying ability

Variables	ITT		LATE	
	Post treatment (single outcome)		Post treatment (single outcome)	
	(1)	(2)	(5)	(6)
IBI	1.003***	0.989***		
	(0.116)	(0.134)		
IBI+ILC	1.284***	1.269***		
	(0.175)	(0.190)		
IBI+ILC+AIC	2.180***	2.185***		
	(0.159)	(0.168)		
Uptake (=1 for uptakers; =0 for non-uptakers)			2.775***	2.785***
			(0.180)	(0.186)
Age		0.011*		0.001
		(0.006)		(0.003)
Gender		-0.563***		0.041
		(0.168)		(0.081)
Married		-0.517*		-0.060
		(0.301)		(0.140)
Education (years)		0.027		0.012
		(0.026)		(0.008)
Family size		-0.003		-0.008
		(0.019)		(0.008)
2015 drought		-0.869*		-0.067
		(0.505)		(0.218)
2016 drought		-0.941*		-0.193
		(0.542)		(0.221)
Land size		-0.008		0.002
		(0.015)		(0.005)
Saving		0.204		-0.038
		(0.192)		(0.059)
Outstanding loan		-0.195		0.037
		(0.147)		(0.048)
Credit rationed		-0.084		0.006
		(0.165)		(0.063)
Constant			0.864***	0.923***
			(0.054)	(0.296)
Observations	1,661	1,659	1,661	1,659
R-squared			0.305	0.303

Note: The dependent variable in estimations reported in Table 11 is the household's shock-copying ability. Shock-copying ability is measured for households on ordinal scale of the respondents' stated perception of their copying ability on a Likert scale ranging from 1=very weak to 4= very strong. It's the answer to the question "In general, how do you rate your ability to cope up with shocks?" Columns 1-2 report the post-treatment (single) effects of the intervention estimated using eq. (13), with and without controls, respectively. Results reported under columns 1-2 are estimated using Ordered Logit. Columns 1-2 report the ITT (single post-treatment effects) of the intervention with and without controls, estimated using eq. (13). Similarly, Columns 3-4 report the LATE (single post-treatment effects) of the intervention with and without controls, estimated using eq. (15a & 15b), respectively. Uptake is instrumented by randomization dummies. Standard errors are clustered at the garee level, and reported in parentheses.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

7. Conclusion

Index-based insurance is increasingly recognized as a pro-poor climate risk management strategy. Overcoming the classic information asymmetry problems that often plague the functioning of rural financial markets, IBIs have a remarkable potential to improve welfare. However, the uptake of IBI remains quite low at micro-level. Practical understanding on the extent to which interlinking IBI with credit and inputs can enhance the uptake and impacts of insurance is important, but yet unexplored, particularly to inform policy aimed at improving rural financial markets and adoption of productivity enhancing high-risk high-return inputs. To improve our understanding in this regard, we conducted an RCT in which we exogenously vary the provision of the standalone IBI, IBI interlinked with credit and IBI interlinked with both credit and agricultural inputs among smallholders.The experiment is undertakenin the Ethiopian Rift Valley zone. The results of the experiment indicate that the uptake of IBI alone is very low amounting 8.8 percent of the total potential demand, but interlinking IBI with credit significantly increases uptake. Further interlinking IBI with both credit and agricultural input even further increases the uptake of IBI. Adopters of IBI can expect that insurance payout and increase in productivity due to intensive use of inputs, can increase their farm profitability, after repaying loans. As a result, their demand for insurance, credit and agricultural inputs can increase simultaneously.

We estimated the causal impacts of the interlinked insurance-credit-input system on household weekly food consumption and investment in high-risk high-return agricultural inputs, using the intent-to-treat (ITT) and local average treatment effect (LATE) for both the single post-treatment and the double difference outcomes. We employed OLS, IV regressions in which actual uptake is instrumented by assignment to treatments and double differencing to overcome biases arising from time-invariant heterogeneity in estimating LATEs. First, impact estimations from the ITT effects indicate that interlinking IBI with both credit and agricultural inputs, increases household total investment in high-risk high-return inputs by ETB 429 and ETB 659, for the single and double difference outcomes, respectively. Further, IV-based 2SLS LATE estimation results show that, the insurance-credit-input intervention has increased total investment in high-risk high-return inputs by ETB 1490, based on the single post-treatment outcome for actual adopters. Then, second, we disaggregated the total impacts of the interlinked intervention on household investment on inputs into effects on investment in chemical fertilizer and improved seed varieties. Estimated ITT effects show that interlinking IBI with both credit and inputs increases investment in chemical fertilizer by ETB 402, for the double difference outcome. IV-based 2SLS LATE estimations also show that the interlinked intervention has increased investment in chemical fertilizer by ETB 595, for the single post-treatment outcome. Similarly, OLS-based ITT estimates indicate that interlinking IBI with credit increases household investment in improved seeds by ETB 314 and ETB 257, for the post-treatment and double difference outcomes, respectively. The IV-based 2SLS LATE estimations also show that the interlinked intervention has investment in adoption of improved seeds by ETB 895, for the single post-treatment. Third, we estimated the impact of the interlinked intervention on household weekly food consumption expenditure. From the OLS-based ITT effect estimations, we find that, for the single post-treatment outcome, interlinking IBI with credit and with both credit and inputs increases weekly consumption by ETB 76 and ETB 91, respectively. In addition, using the difference-in-difference method, estimated ITT effects show that the standalone IBI, IBI interlinked with credit and IBI interlinked with both credit and inputs, have increased the level of consumption by ETB 40, ETB 54 and ETB 96, respectively. Finally, the IV-based 2SLS LATE estimations show that the intervention has increased weekly consumptionfor actual adopters by ETB 292.With respect to productivity, OLS-based ITT effect estimationsshow that interlinking IBI with credit increases land productivity 0.73. The double difference ITT estimates also show that controlling for all covariates, interlinking IBI with both credit and input increases the productivity by 0.42.Further,IV-based 2SLS estimations of the impacts of the intervention on productivity show that the intervention has increased productivity by 2.1, for the single post-treatment data.Lastly, we estimated the impact of the interlinked intervention on two qualitative welfare outcomes: subjective well-being and shock-copying ability. The ordered logit estimates show that uptake of IBI, IBI interlinked with credit and IBI interlinked with both credit and inputs significantly increase the log-odds of reporting higher SWB. These interventions also increase the households' shock-copying ability. Further, IV-based 2SLS estimations reveal that the interlinked intervention increasesthe log-odds of reporting higher subjective well-being and shock-copying ability by 4.11 and 2.79, respectively.

We find that the estimated impacts are justifiable for various reasons. Due to random treatment and low level of attrition in our data, the post-treatment outcomes were unbiased. In addition, the double differencing techniques are helpful to account for potential biases that may arise from time-invariant heterogeneity. Our LATE estimates are also based on the instrumental variable (IV) regressions in which assignment to treatments are used as instrument for actual uptake. The higher welfare impacts we estimated using LATE as compared with ITT are in line with theory, and this is due to the reason that LATE stand for real adopters while ITT estimates are for only being assigned to treatment irrespective of the uptake status. In general, our results point that insurance, credit and agricultural inputs can complement each other, and IBI-credit-input interlinkage can enlarge welfare improvement space of smallholders in developing countries. To successfully meet the risk management needs of smallholders who are usually credit constrained it is important to innovate and develop interlinked financial services that bear enhanced uptake and economic impacts. Previously, insurance, credit, and agricultural inputs were often offered independently of each other but their uptake and impacts are limited. This study, however, evidences that interlinking insurance, credit and inputs together could combine the advantages of all three and hence can enhance the uptake and impacts significantly.The policy-relevant message from this study is that integrating insurance, credit and agricultural inputs can help to upscale agricultural risk management options and improve welfare for smallholders.

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