Does Index-based insurance improve household welfare? Empirical evidence based on panel data in south-eastern Ethiopia

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Abstract

Evidence on the welfare impacts of index-based insurance (IBI) is scant. We use two-round panel data on households who had access to adopt IBI in the Rift-valley zone of south-eastern Ethiopia. Difference-in-difference method with fixed-effect estimation technique is used to reduce potential program placement and individual self-selection biases arising from time-invariant unobserved heterogeneity. Results reveal that adoption of IBI indeed causally increased the level of consumption and investment in high-risk high-return agricultural inputs. Accounting for the intensity of adoption through a flexible model specification, results suggest that repeated adoption of IBI has cumulative lasting effect on these outcomes.

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Introduction

Increased interest is seen in recent years in studying index-based insurance (IBI) as a potential drought insurance technology that constitute a pro-poor climate risk management strategy. However, evidence on the welfare impacts of this innovation is scant. In the absence of data on repeated observations, identification and measurement problems create the difficulty to assess the impact of an intervention implemented in a natural experimental setting. Impact estimations that rely on simple comparison of outcomes of participants with that of non-participants often suffer from selection biases. In technology adoption, one form such bias is self-selection bias that arises from the tendency of adopters to engage in adoption decisions being motivated by their own unique characteristics like individual-specific entrepreneurial spirit. Self-selection bias results in biased impact estimates since unmeasured individual attributes of adopters may simultaneously affect participation in adoption and the outcome of adoption. The other form of selection bias is program placement bias that occurs due to the fact that interventions are usually undertaken on non-random basis. In IBI intervention, insurance firms may choose villages for implementation based upon characteristics that may not be observable to the researcher. Randomized experimental designs that create exogenous variations in sub-groups of treated and controlled units may help to overcome the problems of selection biases. However, randomization alone is not a necessary and sufficient condition to capture the complete impact of an intervention. Karlan and Goldberg (2007) explain that since interventions often take long time to establish lasting effects, impact estimates based on one period of experimental or quasi-experimental data meant to randomize over potential sources of selection may not reflect the full effect of an intervention. The timing and duration of exposures is important in assessing the impact of interventions (King and Behrman 2009). Various studies (Coleman 1999; Pitt and Khandker 1998) depend on cross-sectional data analysis techniques, and exploit program-specific designs or innovative quasi-experimental survey methods to generate control and treatment groups. Estimation techniques like Heckman correction and instrumental variable (IV) are also used to control for selection biases stemming from unobserved heterogeneity. However, these procedures impose distributional and functional form assumptions. Furthermore, identifying a valid instrument that determines the treatment status but not the outcome variable, remains an empirical challenge in IV regressions. The IV approach also ignores interactions between the treatment variable and other covariates as it assumes that the treatment variable has only a parallel shift effect (intercept effect but not slope effect) with respect to the outcome variable. Hence, estimating unbiased impact of an intervention that may accrued overtime, requires a panel dataset with appropriate panel estimators that can deal with individual, village or intervention-specific time-invariant heterogeneity. The double differencing approach with fixed-effect estimates provide a better alternative to control for time-invariant unobservable heterogeneity that may cofound with impacts (Khandker 2005; Copestake, Bhalotra and Johnson 2001; Tedeschi 2008). While such challenges of impact evaluation as a missing data problem were commonly addressed in almost all recent non-experimental studies, the focus in this piece of work is, however, to investigate whether adoption of IBI can provide for welfare enhancements at household level.

This study uses a two-round panel data and difference-in-difference method with fixed-effect estimation techniques to assess the impact of IBI adoption on two household welfare indicators: consumption and investment in high-risk high-return inputs. Household consumption is measured in terms of per capita weekly food consumption, annual non-food and total consumption expenditures. Risk-taking behaviour of the household is also measured in terms of total investment in high-risk high-return inputs with three components: investment in chemical fertilizer, improved seed variety and pesticides/herbicides. In estimating the impact of IBI adoption, first, we employed the fixed-effect estimator that accounts for time-invariant unobservables in difference-in-difference impact estimation framework. Then, we estimated the impacts using a flexible random effect model that takes adoption cycles into account, In both cases, results indicate that adoption of IBI has significant welfare improvement effects in terms of consumption and investment in high-risk high-return agricultural inputs at household level. In particular, from the flexible model specification, we find that the impact of IBI adoption on consumption and investment in high-risk high-return inputs increases with the frequency of IBI adoption. However, the estimated effects of IBI adoption were not monotonously increasing with increase in adoption cycles.

Remaining sections of the paper are organized as follows. Section 2 presents review studies on the impacts of IBI adoption. IBI implementation in the study area, survey design and data collection procedures are explained in Section 3. Section 4 details our impact identification strategy addressing the origins of selection bias in impact evaluation and the difference-in-difference techniques of accounting for it. Section 5 presents estimation results starting with test results for identifying the existence of selection bias and mechanisms of accounting for it. Section 6 concludes the paper.

Review of microinsurance impact studies

Barnett et al. (2008) explains that the economic returns to adoption of IBIs are potentially broad and substantial. These can be in terms of inducing households to make more prudential investments, providing better management for consumption risk, crowding-in finance for ancillary investment and enhancing local adaptation to climate change. Corroborating this, various recent evidences also show that where effectively implemented, IBIs have welfare improvement impacts. Karlan et al. (2014) identified that lack of access to insurance is the limiting factor to investment for maize farmers in Ghana. The study revealed that smallholders who purchased IBI have a 13% more investment in agricultural inputs than others. Fuchs and Wolff (2011) evaluated the impact of insurance against catastrophic drought on corn yields, area cultivated in corn, and per capita income and expenditures of smallholders in Mexico. Results indicate that where coverage is available corn yield was increased by 8% with gains in income and expenditures. This implies that adoption of insurance induces ex-ante risk management responses. Mobarak and Rosenzweig (2013) used a randomized experiment where IBI is offered to Indian cultivators, finding that IBI helps cultivators reduce self-insurance and switch to riskier, higher-yield production techniques. Similarly, in Cai et al. (2015) it was found that insurance for sows significantly increased farmers' tendency to raise sows in south-western China, where sow production is a risky production activity with potentially large returns. In another experiment, Cai (2016) demonstrated weather insurance induces tobacco farmers in China to increase the land devoted to this risky crop by 20%. This finding implies reduced diversification among tobacco farmers, consistent with less self-insurance. Using an experimental game method, Vargas-Hill and Viceisza (2013) indicated that insurance induces farmers in rural Ethiopia to take greater, yet profitable risks, by increasing (theoretical) purchase of chemical fertilizer. An important inference from all these evidences is that adoption of IBI can enhance prudential investment risk-taking behaviour among farm households in developing counties.

IBIs are also evidenced to have impacts on ex-post shock coping. Recent works provide compelling evidences in this regard. Janzen and Carter (2013) explained that access to an index-based livestock insurance (IBLI) in northern Kenya helped pastoral households to smooth their asset and consumption that constitute the two key dimensions of self-insurance. The study revealed that after the intervention, poor pastoral households are less likely to destabilize their consumption in response to drought, while those who are better off are less likely to have to compromise their accumulated assets. Insured households are observed to be less dependent on food aid and other forms of assistance, which indicates their better ability to cope with shocks. The impact of the IBLI intervention was also selective based on wealth position of the pastoral households. Studies also show that adoption of IBI enhances household access to other financial markets. In an experimental game conducted in China, Cheng (2014) studies the effect of offering IBI to risk-rationed households. The study reports that more than half of the risk-rationed farmers decided to apply for credit when IBI is available to them. Similarly, Giné and Yang (2009) studied an intervention where maize farmers in Malawi were offered with a choice between a loan and a loan plus insurance. The study found, however, that demand for the basic loan was 13% higher than that for the insured loan. In Carter, Cheng and Sarris (2016), it was argued that a stand-alone insurance product does not provide additional benefits to farmers who have low collateral. The reason is that, if no formal insurance is available, only farmers with high collateral may choose not to borrow, because they do not want to put their collateral at risk. Cai (2012) also finds that insurance causes households to decrease savings by more than 30%, pointing that households were building up extra precautionary savings in order to better smooth consumption in the case of a shock. Belissa, Lensink and Anne (2018) examined the impact of IBI on credit rationing in Ethiopia, and found that insurance coverage is positively related to credit use. The study then goes on to examine if the change is associated with reduced demand side or supply-side rationing, finding that the changes are due to differences in supply-side rationing.

Adoption of IBIs were also evidenced to reinforce the functioning of the extant social insurance mechanisms. Mobarak and Rosenzweig (2012) show that in rural India existence of informal risk-sharing networks among members of a sub-caste increases demand for IBI when informal risk-sharing covers idiosyncratic losses. Belissa et al. (2018) examined whether uptake of IBI can be enhanced by postponing the premium payment towards shortly after harvest, and by marketing the product through the extant social insurance institutions known as iddirs in Ethiopia. The study found that coaxing the market-based IBI with the predominant social insurance increases uptake, as compared with selling such insurance through conventional statutory channels like state-owned cooperatives. On the other hand, Fuchs and Rodriguez-Chamussy (2011) analysed the impact of insurance payouts on voter behaviour in the 2006 presidential election in Mexico. Using a regression discontinuity design with electoral section as a unit of analysis, the study questions whether insurance payouts received by farmers in the electoral section in 2005 have affected voting behaviour in favour of the incumbent political party in the subsequent 2006 election. The study find that disaster relief buys votes. The incumbent party is estimated to have gained 8% more votes where payouts had been made prior to the election. The overall conclusion of this review of evidences is that, where available and affordable, adoption of IBI does work for the intended purposes. It helps to achieve more effective shock coping and less costly risk management. The outcome can be more growth and less poverty.

Study context, IBI intervention, survey design and data

Context

This study is undertaken in the central Rift Valley zone of the Oromia regional state in south-eastern Ethiopia. The Rift Valley zone is a semi-arid plain plateau area with a low-land agro-ecology. The pattern and intensity of rainfall exhibits considerable spatial and temporal variation, with a bimodal type of distribution. The area receives very low level of annual average rainfall. Rainfall seasons are from May to August and during October and November. Moisture stress and drought frequently causes devastating crop failure, rampant livestock mortality and herd collapse. Major droughts in the area include the 2015-16 drought which followed the historical trend of droughts during 1973-74, 1983-84, 1991-92, 1999-2000, 2005-06 and 2011-12 (Dercon 2004). Households in the area are smallholder subsistence farmers[1] who mainly produce maize and wheat. They often face drought-induced income shocks that translate into erratic consumption patterns. Their ex-post shock coping mechanisms include reducing frequency of meals per day, distress livestock sells, reducing farm investment on chemical fertilizer and improved seed varieties, forcing pupils to withdraw from school for casual labour, renting land and family labour for local landlords and wage employment on floriculture farms of foreign investors. Future drought shock predictions in Ethiopia are pessimistic with expected rise in temperature from 23.08 to 26.92°C (Hulme et al. 2001). As a result, the wide crop-livestock mixed farming system in arid and semi-arid areas like the Rift Valley zone were projected to transform into extensive systems to respond to the risks of climate change (Meinke and Stone 2005; Thornton et al. 2010). Hence, innovative drought risk management mechanisms like adoption of drought insurances were highly recommended for farm households in the area. A large proportion of the smallholders in the study area have no access to formal financial service. They also do not have access to non-traditional[2] risk coping mechanisms. In this context, adoption of IBIs to manage weather-related shocks has been initiated in Ethiopia since 2006.[3] The overall coverage of microinsurance service is however quite low in Ethiopia. For instance, the Microinsurance Network (2015) reported that on average, only 1.9% of the rural population are covered with microinsurance

[1] In Ethiopia, the agricultural sector accounts on average about 42% of the GDP, employs about 85% of the rural labour force and contributes around 90% of the total export earnings.

[2] However, the functioning of indigenous social institutions is actively remarkable. Burial societies (iddirs) provide social insurance services when households lose their bread winners like death of household heads or draft oxen.

[3] The World Bank with the Ethiopian Insurance Company (EIC) implemented the first microinsurance product in southern Ethiopia in 2006. The intervention was targeted to members of cooperatives, and only 28 maize grower farmers have purchased the product. It was discontinued after a year. The main challenge identified was lack of weather stations to extract quality data with a reasonable level of resolution. World Food Program (WFP) also initiated a drought insurance for famine as a meso-level coverage in 2006 (Meze-Hausken et al. 2009). The drought index monitored by AXA Re based on data from 26 weather stations under the National Meteorology Agency (NMA) of Ethiopia did not trigger any payout for the year 2006/07. In addition, Nyala Insurance Company (NIC) implemented IBI for haricot bean production with its initial pilot in Boset district in eastern Ethiopia in 2009 (Meheret 2009). The product was designed based on Water Requirement Satisfaction Index, and piloted through a cooperative union. Though planned to reach smallholders in more drought-prone areas, it was interrupted in anticipation of greater involvement of the Ethiopian government. In addition, Oxfam America in collaboration with Africa Insurance Company (AIC) has widely implemented IBI for crops to manage the risks of drought in Tigray region of northern Ethiopia in 2009 (Dinku et al. 2009). The product designed was based on Rainfall Estimate (RFE) within 10 km^2 grid was implemented as insurance-for-work programme in exchange of labour for environmental works. Though the intervention has increased uptake, it entailed high premium and high basis risk.

products, and this figure is very small even when compared to other African countries such as Kenya (6%), Uganda (6.7%), Ghana (29.6%) and South Africa (64%).

IBI in the study area

In 2013, Japan International Cooperation Agency (JICA) and Oromia Insurance Company (OIC) jointly implemented IBI for crops in the Rift Valley zone of Ethiopia to improve the resilience of households in the face of climate change. The IBI scheme was implemented in five districts including Boset, Bora, Ilfata, Adamitullu-Jido-Kombolcha (AJK) and Arsi Negele. Before the initial intervention in 2013, Oromia Insurance Company (OIC), JICA that provides the financial support and Ethiopian Ministry of Agriculture that provides the technical support for the intervention have discussed and identified districts in which drought shocks are common in Ethiopia. Most of these districts are located in the Rift Valley zone. The partners then held a focus group discussion (FGD) with selective representative farmers from each kebele within each of the selected districts. Based on this discussion, many kebeles which have severe drought experience in the past were identified. However, then, it was found that the financial support that JICA allotted for the 2013 weather index insurance intervention was not adequate to cover all the identified drought-prone kebeles. Therefore, the partners randomly considered some kebeles for the first intervention in 2013 and rank-filed the remaining kebeles to be considered in subsequent interventions. IBI is often marketed and sold twice per year during April and during September, months preceding the two rainy seasons, to provide coverage against losses during the seedling and flowering stages of crop growth, respectively. Major targeted stable food crops to be insured include maize, wheat, barley and teff. However, payout is not crop-specific. Two IBI products were implemented in the study area. These are termed as weather index crop insurance (WICI) and vegetation index crop insurance (VICI). WICI was the initial IBI product co-implemented by JICA and OIC. It was designed by CelsiusPro Ag and Swiss Re, using satellite rainfall data with 10 km^2 resolution for the period 1983–2012. OIC used this product during the period 2013-2014[4]. In WICI, payout is determined according to the level of rainfall measured at the meteorological station nearest to the focal kebele for a specified period.

Since 2015, however, OIC further collaborated with the University of Twente in Netherlands and Kifiya Financial Technology in Ethiopia to design the VICI. VICI is a modified product with better quality compared to the WICI. VICI 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 differential vegetation index (NDVI)[5]. In VICI design, NDVI is extracted at a geospatial resolution of 1km^2. The current 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^2 will then be arranged in

[4] The underwriting procedure includes awareness creation, grid selection, enrolment and sells.

[5] 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.

percentile ranges from 1 to 20, 25 and 50. Based on this percentiles, benchmark values for trigger and exit index points which will be compared to the actual risk level are determined[6].

Survey design and data

Data used in this study were collected from smallholders in the Rift-valley zone in south-eastern Ethiopian. A two-round survey with two-year intervals (2015–2017) was administered on 1143 randomly selected IBI-adopter and non-adopter households. Recruitment of households included in these two surveys was worked out as follows. First, we selected three districts, namely Bora, AJK and Arsi Negele, out of the five districts where OIC implemented IBI. Second, we identified a random sample of kebeles within the three districts[7], including those kebeles covered by IBI as well as those that OIC did not cover. Finally, sample households were randomly drawn from all these selected kebeles. In the first round of survey that we conducted during January-April, 2015, data were collected from a total of 1143 households, out of which 461 were adopters and 682 were non-adopters of IBIs, over the period 2013-14. The dataset covers information on household, village and IBI intervention, including household demographic characteristics, investment in agricultural inputs, consumption, use of financial services as well as village infrastructure and access to markets. The same questionnaire used in the baseline survey was also administered in the end-line survey. The questionnaires did take about 3 hours per interview[8]. Respondent attrition was minimal. Only four households who were considered during the baseline were not covered during the end line survey. This study is thus based on a balanced panel of 1139 households, of which 596 were adopters and 543 were non-adopters, during the second survey observation. Over the two survey periods, adoption or treatment status was changed in subsequent years, with some households joining IBI adoption and others dropping out. In addition, uptake payout data were collected from OIC, and cross-checked with the responses of the households in the survey. An advantage of these data in studying the impact of IBI is that the baseline observation in 2015 coincides with the massive expansion of IBI in villages that were rank-filed during the initial two years of intervention, to be considered in the subsequent intervention periods. This enables us to identify the impact of IBI adoption using 2015 as baseline information for both adopters and non-borrowers.

Moreover, there is little reason to believe that OIC's expansion to other villages has been systematic and endogenous to village outcomes. In principle, if a kebele is considered for IBI implementation,

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[6] In summary, the mechanics of 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 a grids of 1km^2. 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 1 km^2 grid (location) specific.

[7] A district on average holds about 20 kebeles, some of which may be covered in OIC intervention while others not.

[8] We conducted the surveys partly at the Farmers' Training Centre (FTC) and partly at their home. The surveys were undertaken by experienced enumerators many years of experience on working with rural households, and collecting socio-economic data. The uptake and payout data were authentically obtained from OIC and, so, there was no need to carry out surveys to obtain these data. In addition to the quantitative analyses, we conducted Focus group discussions (FGDs) and in-depth stakeholder interviews to better understanding, and to triangulate our quantitative results.

all residents in that kebele were eligible to buy IBI. However, households may have self-selected into IBI adoption, and participation can be endogenous at the individual level, which we explicitly tackle in the empirical analysis. We measure the impact of IBI adoption on two welfare indicators: household consumption and investment in high-risk high-return inputs. Both set of variables are continuous in nature. Household consumption is an aggregate of selected food and non-food consumption. Food items consumed both from own sources and from purchases over a period of one week were included[9]. Necessary adjustments are made to make measured items and units. To minimize measurement error from heterogeneity in age among household members, per capita consumption is used. Table 1 presents explanation of variables included in this study.

Identification strategy

The main parameter of interest in estimating the causal effect of an intervention is quantifying the impact of a treatment on the outcomes of a treated unit (Heckman 1998). In this section, we quantify the impact of IBI adoption on household welfare indicators using the difference-in-difference method. We start by identifying the origins of selection bias and the difference-in-difference techniques of testing the existence of self-selection and program placement biases. Consider a generic model for evaluating the impact of a treatment on welfare outcomes of a treated household as:

$$H_{it} = P_{it}\gamma + X_{it}\beta + V_j\varphi + M_i\alpha + \mu_{it} \tag{1}$$

where the welfare outcome variable H_{it} of household i at time t is determined by participation in IBI adoption P_{it}, vectors of observable characteristics X_{it}, time-invariant observable village and household-specific characteristics, V_j and M_i, respectively, and the stochastic error term μ_{it}. Participation of households in IBI adoption, in turn, depends on a set of observable characteristics Z_{it} and unobservable characteristics W_{it} in such a way that: $P_{it} = W_i\phi + Z_{it}\psi + \varepsilon_{it}$. In an estimation procedure, Z_{it} can be included in X_{it}. Selection bias arises when the unobservables M_i and the error term μ_{it} in the welfare outcome equation correlate with the unobservables W_i and the error term ε_{it} in the IBI adoption participation equation. In this study, there are two potential sources of bias that can cause non-zero correlations among these variables. First, households that participated in adoption of IBI might have been motivated to do so by their own unobservable characteristics like entrepreneurial spirit. Thus, entrepreneurial ability as unobservable attribute might have simultaneously influenced IBI uptake decision as well as the welfare outcomes of the household. This is, however, a testable hypothesis as considered in various studies (Heckman and Hotz 1989; Tedeschi 2008; Berhane and Gardebroek 2011). In this study, we investigate the existence of selection bias by testing the hypothesis that the baseline level of welfare for households who eventually become (late) adopters, dropouts and persistent adopters at the end-line survey, are not statistically different from that of non-adopters (i.e., who never adopted till the end line survey). We identify this through a specification that regresses the 2015 consumption and investment in high-risk high-return inputs over dummy variables for late adopters, dropouts and persistent adopters as follows:

$$H_{i,15} = \tau_1 + \tau_2 X_{i,15} + \tau_3 Late_{i,17} + \tau_4 Dropout_{i,17} + \tau_5 Persistent_{i,17} + \tau_6 R_{i,15} + \in_i \tag{2}$$

[9] Food items include food grains, fruits, vegetables, milk and milk products, beef, meat and meat products, cooking oil, salt, and coffee, tea, and other leisure drinks. Non-food items include clothing and footwear, gas and fuel, schooling, health, family events, and household durables. Note that the recall period for estimating food items consumed was 7 days, and for non-food items like cloths was a year.

where $H_{i,15}$ represent the baseline level of welfare; $Late_{i,17}$, $Dropout_{i,17}$, and $Persistent_{i,17}$ represent dummies for households who eventually become late adopters, dropouts and persistent adopters, respectively, between the baseline and the end line survey. $R_{i,15}$ is a dummy indicator for the existence of IBI service in the kebele until the baseline survey. Inclusion of this variable in eq. (2) enables us to test for the existence of program placement bias. Specifically, we infer that if τ_6 is statistically different from zero, there is a program placement bias in IBI implementation process that should be taken into account in estimating the impacts of IBI. Hence, in all our impact estimates, we included the variable, $R_{i,15} = 1$, for households in the treatment region before the baseline survey, and 0, for others. In addition, the use of panel data techniques by itself reduces self-selection and program placement biases through creating correlations between selection decisions and unobservables. Panel data models including the fixed and random effect models are preferred to reduce selection problems through within and between transformations. The fixed-effect estimator in difference-in-difference setting provides a consistent estimate of eq. (1) on the assumption that all unobservables that influence the outcome of interest are time-invariant, since these unobservables can be removed by a within transformation. To clarify this, eq. (1) can be rewritten in the form of a two-period model as follows.

$$H_{it} = P_{it}\gamma + X_{it}\beta + V_j\varphi + M_i\alpha + R_{it}\tau + \mu_{it} \quad (3)$$

$$H_{it+1} = P_{it+1}\gamma + X_{it+1}\beta + V_j\varphi + M_i\alpha + R_{it+1}\tau + \mu_{it+1} \quad (4)$$

Then, through first-differencing, a consistent estimate of γ that measures the effect of the treatment can be obtained by eliminating the time-invariant village and household-specific unobservables that can potentially bias γ. Hence, the standard fixed-effect model that can result from first-differencing the above systems of equations is given as:

$$\ddot{H}_{it} = \ddot{P}_{it}\gamma + \ddot{X}_{it}\beta + \ddot{R}_{it}\tau + \ddot{\mu}_{it} \quad (5)$$

where $\ddot{H}_{it} = H_{it+1} - H_{it}$; $\ddot{X}_{it} = X_{it+1} - X_{it}$; $\ddot{R}_{it} = R_{it+1} - R_{it}$ and $\ddot{\mu}_{it} = \mu_{it+1} - \mu_{it.}$ The estimate of γ is unbiased since time-invariant unmeasured attributes were dropout. Note that estimating equation (5) provides the effect of the program and the effect of the time trend individually. However, it is not feasible to estimate the effect of the treatment on the treated as an interaction effect. A more feasible specification can be explained as:

$$H_{it} = \varphi P_{it} + \phi t_t + \gamma(P_{it}t_t) + \tau R_{it} + \alpha X_{it} + \varepsilon_{it} \quad (6)$$

where γ in this case is the co-efficient of the interaction term as unbiased measure of the effect of the treatment on the treated. Obviously, since P_{it} is a dummy for participation in IBI adoption, the specification in eq. (6) imposes the restriction that IBI adoption in each cycle bears the same impact. In other words, it cannot account for the effects from the intensity or frequency of IBI adoption. Initial adoption may, however, entail lasting effect on welfare levels, which can alter the effect of adoption in subsequent periods. Repeated cycles of IBI adoption can also result in higher level of impact. Wooldridge (2002) suggested a more flexible specification that allows intervention indicators to reflect the frequency or intensity of adoption in each year. Such model was employed in recent impact evaluation studies (e.g., Berhane and Gardebroek 2011). Our specification accounts for the impacts of repeated cycles of IBI adoption by replacing P_{it}, t_t and $P_{it}t_t$ in eq. (6) with a series of adoption frequency for each cycle of adoption for which the household has been in the program as follows:

$$H_{it} = \gamma_1 P_{1,it} + \cdots + \gamma_k P_{k,it} + \tau R_{it} + \alpha X_{it} + \varepsilon_{it} \tag{7}$$

where $P_{1,it} = 1$, if individual household i has been adopting IBI for exactly j cycles during period t or $P_{1,it} = 0$, otherwise. Similarly, k is the maximum number of years the household was observed to have been adopting IBI.

Result and discussions

Characteristics of the sample households

Summary statistics for IBI-adopter and non-adopter households on some selective pre-intervention characteristics as well as outcome variables is presented in Table 2. IBI-adopter and non-adopter households had statistically significant differences in terms of their demographic characteristics including age and gender of the household head as well as their family size and level of education. Compared to non-adopters, IBI-adopters were more aged, female-headed, more educated and had larger family size. In terms of production assets, adopter and non-adopter households have statistically significant differences in land size and livestock assets measured in standard tropical livestock units (TLU). IBI-adopters had larger land and livestock sizes compared to non-adopter households. Our descriptive statistic results also show that IBI-adopters travel more distance to access markets and, also make more frequent contact with extension agents as compared with non-adopter households. With regard to the outcome variables, Table 2 shows that, compared to non-adopters, IBI-adopters have made much more investment in total high-risk high-return agricultural inputs. On disaggregated terms, adopters have made much more investment in chemical fertilizer, improved seed varieties and pesticide/herbicides as compared with non-adopters. Similarly, there are statistically significant differences between IBI-adopter and non-adopter households in their per capita consumption.

Table 1: Variable type and definition

Variables	Variable type and definition
Age	Continuous, age of the household head in years
Gender	Dummy, gender of the household head, 1= male headed 0 = female headed
Education (years)	Continuous, household head's level of education in years of schooling
Family size	Continuous, number of household members in the family
Dependency ratio	Continuous, ratio of dependents[1] in a family to family size
Land size in *qarxi*	Continuous, household's land holding, measured in a local unit called qarxi, where 1 qarxi = 0.25 hectares
Livestock size (*TLU*)	Continuous, number of livestock owned by a household measured in standard tropical livestock units (TLU)[2]
Distance from market	Continuous, distance from household's residence to market measured in walking hours
Extension contact	Dummy, equal to 1 for households that frequently make contact with extension agents; 0 for others
Investment in fertilizer	Continuous, value of household's investment in fertilizer in ETB
Investment in improved seed	Continuous, value of household's investment in improved seed varieties in ETB
Investment in pesticides/herbicides	Continuous, value of household's investment in pesticides/herbicides in ETB
Total investment in high-risk high-return inputs	Continuous, value of household's total investment in high-risk high-return agricultural inputs in ETB
Per capita food consumption expenditure	Continuous, value of household's annual per capita food consumption expenditure in ETB
Per capita non-food consumption expenditure	Continuous, value of household's annual per capita non-food consumption expenditure in ETB
Per capita total consumption expenditure	Continuous, value of household's total annual per capita food consumption expenditure in ETB

[1] Family dependents were counted as household members with age less than 15 years (preschool children) and those with age greater than 65

[2] 1 TLU = 1*cow or ox = 0.75*heifer = 0.34*calf = 1.1*horse = 0.7*donkey = 0.13*adult sheep or goat = 0.013*chicken (Storck *et al.* 1991)

Table 2: Summary statistics of IBI adopter and non-adopter households

Variables	Full sample		Adopters		Non-adopters		Difference in means		t-value
	Mean	SD	Mean	SD	Mean	SD	Mean	SD [a]	
Age	39.56	12.40	40.24	11.32	38.81	13.45	-1.43	0.59	-2.76***
Gender	0.84	0.37	0.86	0.35	0.82	0.38	-0.03	0.02	-2.23***
Education (years)	2.39	1.15	2.51	1.13	2.25	1.16	-0.26	0.05	-5.38***
Family size	7.00	2.81	7.52	2.93	6.44	2.54	-1.08	0.12	-9.32***
Dependency ratio	0.50	0.20	0.50	0.20	0.50	0.21	0.00	0.00	0.05
Land size in *qarxi*	8.86	8.96	9.84	10.78	7.79	6.21	2.05	0.37	-5.50***
Livestock size (TLU)	9.62	8.03	10.57	8.91	8.58	6.79	-1.99	0.33	-5.95***
Distance from market	1.72	1.03	1.79	1.10	1.64	0.95	-0.15	0.04	-3.56***
Extension contact	0.95	0.22	0.96	0.19	0.93	0.25	-0.03	0.01	-3.45***
Investment in fertilizer	2266.69	2813.17	2891.50	3232.74	1580.88	2058.42	-1310.62	114.79	-11.42***
Investment in improved seed	1823.31	3889.97	2470.16	4913.72	1113.32	2070.83	-1356.84	160.72	-8.44***
Investment in pesticides	241.72	1351.08	338.36	1822.57	135.65	403.81	-202.71	56.53	-3.59***
Total investment in high-risk high-return inputs	4331.72	5748.36	5700.03	7037.67	2829.86	3265.57	-2870.17	233.57	-12.29***
Per capita food consumption expenditure	93.07	184.04	103.07	186.23	82.09	181.05	-20.98	7.70	-2.72***
Per capita non-food consumption expenditure	1808.37	4071.95	1865.45	4566.56	1745.72	3449.41	-119.72	170.83	-0.70
Per capita total consumption expenditure	6254.101	10631.6	6846.92	10781.86	5603.41	10430.41	-1243.51	445.32	-2.79***

*** p<0.01, ** p<0.05, * p<0.1.

[a] $SD_{difference\ in\ means} = \sqrt{\frac{SD_1^2}{N_1} + \frac{SD_2^2}{N_2}}$

As shown in Table 2, on average, IBI-adopters have higher per capita weekly food consumption and higher per capita total annual consumption than IBI non-adopters. However, the difference in per capita non-food consumption between IBI-adopters and non-adopters is not statistically significant. In general, IBI-adopter and non-adopter households were observed to have statistically meaningful differences on certain pre-intervention characteristics as well as outcome variables. Differences in pre-intervention characteristics between the two groups necessitates to control for these variables in our subsequent impact estimate regressions. Similarly, the observed differences in outcome variables provides us with indications about the impacts.

Impacts of IBI adoption

Table 3 reports descriptive statistics results on mean changes in welfare outcome indicators of the sample households over the IBI adoption period. Results presented in Table 3 are based on manual computation of the difference-in-difference in outcomes of IBI-adopter and non-adopter household. First, we calculate difference-in-difference values for total amount of investment in high-risk high-return inputs. Then, we did similar computations for the disaggregate values, separately, for the investment made in chemical fertilizer, improved seed varieties, and pesticides/herbicides. In a similar way, we compute the difference-in-difference values for weekly per capita food consumption, per capita annual non-food and total consumption expenditures. Results depicted under column 5 in Table 3 were derived as the first difference, D_1, by subtracting the end-line welfare indicator values from the baseline for non-adopter households. Results under column 5 thus indicate the change in investment in high-risk high-return inputs as well as consumption for non-adopter households over the two periods (i.e., year 2015 and year 2017), indicated by $t = 1$ and $t = 2$, respectively. Similarly, in the second step, results reported under column 6 in Table 3 were derived as the second difference, D_2, by subtracting the end line welfare outcome values from the baseline, for adopter households. D_2 indicates the change in investment in high-risk high-return inputs as well as consumption for IBI-adopters over the two periods. Thirdly, results presented under column 7 in Table 3, were derived as the difference-in-difference, DD, by subtracting the second difference D_2 from the first difference D_1.

The difference-in-difference results shown under column 7 in Table 3 indicate that, on average, IBI-adopter households have made about ETB 3165 higher investment in high-risk high-return agricultural inputs than non-adopter households. On disaggregated terms, average investment made to purchase chemical fertilizer, improved seed variety and pesticide/herbicide were higher, for IBI-adopter households than non-adopters, by ETB 986, ETB 2085 and ETB 94, respectively. Table 3 also shows that IBI-adopter households have made ETB 37 higher weekly per capita food consumption expenditure than non-adopters. IBI-adopter households have also made ETB 2008 higher annual per capita total expenditure than non-adopters. In general, on average, IBI-adopter households have higher welfare indicator values than non-adopters for the average values of most of the outcome variables considered. Estimated results presented in Table 3 are, however, simple measures of descriptive statistics. As such these results are not adequate to causally infer the impact of IBI adoption on the welfare of adopter households. Hence, more rigorous estimates that take the role of other covariates into account were provided and discussed in the subsequent sections.

Table 3: Descriptive statistics on mean changes in welfare outcome indicators over IBI adoption period

Welfare outcome indicators	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	$t = 1$		$t = 2$				
	Adopters (a)	Non-adopters (b)	Adopters (c)	Non-adopters (d)	$D_1 = d - b$	$D_2 = c - a$	$DD = D_2 - D_1$
High-risk high-return investment							
Investment in fertilizer	2338.97	1521.33	3444.03	1640.43	119.10	1105.06	985.96
Investment in improved seed	904.30	590.11	4036.02	1636.54	1046.43	3131.72	2085.29
Investment in pesticides	266.17	110.31	410.55	160.99	50.68	144.38	93.70
Total investment in high-risk high-return inputs	3509.45	2221.75	7890.60	3437.96	1216.21	4381.16	3164.94
Per capita consumption expenditure							
Per capita food consumption expenditure	40.72	38.44	165.42	125.74	87.30	124.70	37.40
Per capita non-food consumption expenditure	1561.34	1348.29	2169.56	2143.16	794.87	608.22	-186.65
Per capita total consumption expenditure	3715.47	3475.98	9978.38	7730.85	4254.87	6263	2008.04

Estimation results

This section presents estimation results based on the models outlined earlier, with selection bias test results being presented first. The test is carried out by estimating equation (2) using OLS estimator for the 2015 investment in high-risk high-return inputs and consumption outcomes. Test results for the existence of self-selection and program placement biases are presented in Table 4. The F-statistic from the OLS results in Table 4 reveal that the null hypothesis that all parameters of interest are simultaneously equal to zero is rejected at a 1% significance level for all outcome variables, except for the variable investment in herbicides/pesticides. In order to assess the prevalence of self-selection bias, the most important results from this regression are the parameter estimates for late-adopters, dropouts and persistent-adopters. The insignificance of the dummy variable, *late-adopters*, for most of the variables, indicate that self-selection is not a serious problem in this data. Insignificant values shows that those who eventually become adopters in 2017 had no statistically different levels of consumption or investment in high-risk high-return inputs in 2015. However, Table 4 shows that the null hypothesis that there was no significant difference between persistent-adopters and non-adopters ($H_o: \tau_5 = 0$) is rejected at the 5% level of significance for the outcome variables: total investment in high-risk high-return inputs and chemical fertilizer, though not rejected for the rest of the welfare indicator outcome variables. Thus, controlling for dropouts, we do not find that those who eventually became IBI-adopters in 2017 had higher investment in high-risk high-return inputs or higher per capita consumption expenditure than those who never adopted IBI. However, we find certain evidence that those who persistently adopted IBI had higher investment in high-risk high-return inputs than those who never adopted IBI. Hence, our impact analysis must account for potential bias due to self-selection. With regard to the program placement bias, test results in Table 4 show that the parameter estimates of the proxy variable 'region' are statistically significant, for the total value of investments in high-risk high-return inputs, chemical fertilizer and improved seed varieties as well as per capita weekly food consumption and total annual consumption expenditures at 1% level of significance. This test results point that to a certain extent there is program placement bias in adoption of IBI that should be taken into account in estimating the robust impact of IBI adoption on household welfare. Moreover, as detailed in the subsequent section, our use of the panel data techniques of impact estimation, helps to reduce the potential biases due to self-selection and program placement thereby removing time-invariant variation.

Table 4: Test results for selection bias using baseline data

Variables	(1) Total value of high-risk high-return inputs	(2) Value of fertilizers used	(3) Value of improved seed used	(4) Value of pesticide used	(5) Per capita total consumption expenditure	(6) Per capita food consumption expenditure	(7) Per capita non-food consumption expenditure
Intercept	-1095.47	-378.82	-338.39	-378.25	6046.17***	57.28***	2836.18***
	(1057.90)	(821.66)	(344.01)	(333.82)	(937.36)	(7.71)	(569.94)
Late adopters	77.53	106.72	7.24	-36.43	471.82*	7.12**	173.64
	(349.23)	(260.58)	(131.56)	(43.11)	(278.23)	(3.06)	(140.27)
Dropouts	1096.01***	704.98***	226.19*	164.84	182.03	2.74	171.11
	(303.57)	(173.06)	(124.25)	(150.92)	(298.53)	(2.05)	(145.64)
Persistent adopters	1183.74**	891.80**	84.37	207.57	534.42	1.27	547.60
	(507.15)	(308.38)	(144.56)	(232.61)	(476.63)	(2.96)	(333.27)
Region	-910.16***	-559.19***	-397.13***	46.16	727.90***	9.00***	130.11
	(265.42)	(196.43)	(116.21)	(38.44)	(362.47)	(2.20)	(133.12)
Age	3.30	3.26	1.24	-1.19	-24.26***	-0.19**	-11.91**
	(9.37)	(6.53)	(4.63)	(2.43)	(8.20)	(0.07)	(5.35)
Gender	-174.12	-277.34	126.25	-23.02	267.13	-1.96	237.23*
	(259.50)	(223.08)	(106.64)	(47.95)	(289.67)	(3.32)	(132.41)
Education (years)	681.48***	489.38***	186.94***	5.16	317.88***	3.33***	128.80*
	(115.93)	(94.48)	(49.30)	(14.67)	(79.57)	(0.79)	(67.96)
Family size	209.59***	92.78**	96.33***	20.48	-400.11***	-4.09***	-152.09***
	(55.40)	(40.43)	(27.19)	(20.64)	(59.45)	(0.37)	(25.07)
Dependency ratio	1048.60	620.72	91.31	336.56	-3725.05***	-33.66***	-1289.85**
	(682.37)	(444.11)	(231.89)	(301.02)	(1104.07)	(6.49)	(469.03)
Land size in *qarxi*	142.15***	83.53***	35.74***	22.88	17.69	0.15	9.16
	(33.00)	(16.12)	(10.05)	(17.99)	(13.17)	(0.13)	(8.28)
Livestock size (TLU)	9.41	3.59	15.42**	-9.60	76.51***	0.47***	36.26***
	(19.11)	(12.44)	(7.70)	(9.22)	(25.55)	(0.13)	(10.18)
Distance from market	-282.65	-158.91*	-83.81***	-39.93	71.91	2.77***	-41.65
	(122.45)	(86.47)	(44.84)	(39.02)	(126.44)	(1.00)	(65.92)
Extension contact	-695.08	-461.86	-365.65	132.43*	67.16	5.16	-338.18
	(752.03)	(601.23)	(244.52)	(76.47)	(390.62)	(3.11)	(308.06)
District 1	1942.19***	1595.73***	-75.52	421.98	658.94	8.18**	290.67
	(663.16)	(376.41)	(126.57)	(391.43)	(498.75)	(3.46)	(290.21)
District 2	110.35	98.15	48.75	-36.56	404.57	5.47***	-49.74
	(263.23)	(199.13)	(113.78)	(44.30)	(287.54)	(2.89)	(179.07)
R^2	0.1681	0.1592	0.1447	0.0326	0.1816	0.2669	0.1255
$F(15, 91)$	9.28***	12.69***	8.29***	0.84	15.42***	16.45***	9.18***
Observations	1139	1139	1139	1139	1139	1139	1139

*** p<0.01, ** p<0.05, * p<0.1. Robust standards errors indicated in brackets are clustered at iddir level

Table 5 reports the results of household fixed-effect estimates of the model explained in equation (6) for investment in high-risk high-return inputs. For total investment in high-risk high-return inputs, chemical fertilizer, improved seed variety and herbicide/pesticide, Table 5 provides a separate estimate of the impact of IBI adoption with and without controlling for potential covariates. Note that regression results in Table 5, which are estimated without including controls variables are equivalent with the descriptive impact estimates presented in column 7 of Table 3. Since we are primarily interested to measure the welfare impact of IBI adoption, we include only household observables that may systematically correlate with selection. Moreover, since time-invariant characteristics are removed by the within transformation, only time-varying variables were included in our estimation procedure. Demographic characteristics including age, gender and level of education of the household head as well as family size and dependency ratio are considered. Land size and size of the livestock owned by the household were also controlled for, since these productive assets may also affect the level of the outcome indicators. In anticipation that OIC might have targeted accessible villages in terms of infrastructure, we included proxies, distance from the market and frequency of extension contact, as two sets of controls. Moreover, we controlled for spatial variations by including district dummies in the regressions.

In Table 5, the significance of the *F*-test statistics for all outcome indicators, under both with and without covariate regressions, reveal that not all parameters are jointly equal to zero at the 1% level of significance. The results based on the fixed effect estimator under column (1) show that adoption of IBI has a significant positive effect on household investment in high-risk high-return agricultural inputs for adopters as compared with non-adopters. Table 5 also shows that after controlling for potential selection on unobservables, household investment in high-risk high-return agricultural inputs has increased by ETB 3176 for IBI-adopters, compared to non-adopters. Under columns (4) and (6), the results in Table 5 also show that IBI-adopters have made ETB 994 and ETB 2088, higher investment in chemical fertilizer and improved seed, respectively, than non-adopters. In Table 5, we also observe that certain control variables, namely education of the household, land size and number of livestock owned by the household have statistically significant positive effect on investment in total high-risk high-return inputs and chemical fertilizer. Land size and livestock ownership also have significant impact on investment in improved seed varieties.

Table 6 presents the results of household fixed-effect estimates of the model explained in equation (6) for per capita consumption outcomes. The F-test statistic is significant for all the estimations reported under columns (1) to (6), for the three consumption indicators (i.e., per capita food, non-food and total consumption) with and without covariate controls. Hence, the hypothesis that all estimated parameters are jointly equal to zero is rejected at 1% level of significance. Under column (2), Table 6 shows that after controlling for all covariates weekly per capita food consumption is ETB 37.74 higher for IBI-adopters than non-adopters. Similarly, per capita annual consumption is also ETB 2027 higher for IBI-adopters than non-adopters.

Table 5: Impacts of IBI adoption on household production behaviour

Variables	(1) Total value of high-risk high-return inputs	(2) Total value of high-risk high-return inputs	(3) Value of fertilizers used	(4) Value of fertilizers used	(5) Value of improved seed	(6) Value of improved seed	(7) Value of pesticides used	(8) Value of pesticides used
Uptake of IBI (Treatment)	1211.99***	635.82**	801.88***	558.03***	258.75**	-38.98	151.36	116.77
	(312.03)	(272.54)	(180.96)	(166.97)	(107.97)	(111.48)	(147.04)	(130.31)
Year	1216.21***	1222.22***	119.10	108.05	1046.43***	1058.01***	50.68*	56.17**
	(207.20)	(212.57)	(134.66)	(138.14)	(138.71)	(139.48)	(22.12)	(22.01)
Treatment * year	3169.11***	3176.32***	987.64***	993.93***	2087.59***	2088.21***	93.88	94.18
	(361.03)	(368.09)	(191.94)	(192.17)	(316.13)	(322.24)	(152.77)	(151.52)
Region		114.33		-193.71		195.39		112.65**
		(265.94)		(186.20)		(160.21)		(44.04)
Age		-5.01		1.29		-4.20		-2.10
		(11.36)		(5.01)		(8.61)		(2.13)
Gender		-91.37		-122.63		70.27		-39.01
		(256.09)		(133.94)		(214.78)		(33.67)
Education (years)		384.09***		296.74***		72.39		14.97
		(107.98)		(58.42)		(77.41)		(15.92)
Family size		151.29***		41.61		97.75**		11.93
		(54.11)		(27.70)		(47.39)		(14.40)
Dependency ratio		639.11		258.01		215.15		165.96
		(694.05)		(275.69)		(492.22)		(143.24)
Land size in *qarxi*		92.80***		45.70***		35.23*		11.87
		(32.75)		(11.10)		(17.81)		(9.66)
Livestock size (TLU)		74.51***		52.20***		26.58**		-4.28
		(16.18)		(8.87)		(10.47)		(5.33)
Distance from market		-156.00		-72.20		-61.27		-22.53
		(87.85)		(50.79)		(50.66)		(21.31)
Extension contact		-348.39		-418.09		18.34		51.36
		(427.68)		(369.17)		(202.96)		(49.27)
District 1		439.58*		912.14***		-690.67*		218.12
		(538.81)		(217.45)		(350.02)		(263.72)
District 2		-590.21		182.58		-641.43*		-131.36**
		473.88)		(140.53)		(384.98)		(45.20)
Constant	2260.28***	-174.71	1529.14***	52.77	618.52***	-162.89	112.62***	-64.60
	(170.19)	(1034.56)	(101.72)	(487.72)	(94.73)	(746.26)	(36.58)	(231.90)
R^2 within	0.14	0.1971	0.0736	0.1536	0.1249	0.1463	0.0067	0.0246
$F(15,91)$	104.43***	35.24***	50.81***	21.67***	56.74***	18.71***	40.16***	8.87***
Observations	2278	2278	2278	2278	2278	2278	2278	2278

*** p<0.01, ** p<0.05, * p<0.1. Standard deviations are in brackets. Standards errors are clustered at iddir level

Table 6: Impacts of IBI adoption on per capita consumption

Variables	(1) Per capita food consumption expenditure	(2) Per capita food consumption expenditure	(3) Per capita non-food consumption expenditure	(4) Per capita non-food consumption expenditure	(5) Per capita total consumption expenditure	(6) Per capita total consumption expenditure
Uptake of IBI (Treatment)	-0.17	8.32**	127.77	331.71**	62.55	606.48*
	(2.50)	(3.56)	(131.96)	(149.64)	(283.08)	(318.41)
Year	87.30***	87.85***	794.86***	829.79***	4254.87***	4315.41***
	(12.29)	(12.66)	(181.56)	(182.04)	(690.72)	(707.64)
Treatment * year	37.54**	37.74**	-185.30	-182.04	2015.62**	2026.97**
	(16.90)	(16.94)	(324.33)	(323.50)	(922.89)	(924.32)
Region		25.72**		484.58*		1741.29**
		(11.68)		(275.09)		(681.40)
Age		-0.43		-20.90**		-37.91
		(0.40)		(9.10)		(22.98)
Gender		1.75		270.11		382.57
		(15.73)		(271.72)		(901.99)
Education (years)		0.77		247.99**		247.60
		(3.35)		(93.1850		(203.48)
Family size		-11.41***		-297.19***		-798.11***
		(1.66)		(58.01)		(106.32)
Dependency ratio		-79.55**		-3154.95***		-6485.32***
		(28.74)		(941.59)		(1748.14)
Land size in *qarxi*		0.58		8.93		34.65
		(0.44)		(12.98)		(28.60)
Livestock size (TLU)		0.44		23.47**		58.22**
		(0.39)		(9.94)		(28.01)
Distance from market		-2.12		-30.53		-133.55
		(2.76)		(81.26)		(175.69)
Extension contact		-9.74		-317.19		-513.29
		(17.86)		(362.14)		(944.17)
District 1		-0.36		426.83		438.99
		(15.90)		(558.50)		(994.97)
District 2		-27.94*		-197.60		-1236.27
		(14.32)		(435.75)		(852.27)
Constant	39.69***	172.00***	1392.56***	4691.22***	3566.58***	12013.72***
	(4.230	(30.40)	(92.94)	(1221.68)	(282.19)	(2016.33)
R^2 within	0.0939	0.1527	0.0078	0.1169	0.0705	0.1642
$F(15,91)$	54.28***	21.40***	9.96***	10.63***	43.26***	19.44***
Observations	2278	2278	2278	2278	2278	2278

*** p<0.01, ** p<0.05, * p<0.1 Robust standards errors clustered at iddir level are in brackets

An important follow-up question that we address in the next section is the extent to which the impact of IBI adoption varies with the intensity and frequency of repeated adoption. This issue is analysed using the flexible random effect model explained in equation (7) in section 3. Instead of mere participation in IBI adoption, in this case, indicators were assigned, for the number of times each household has been involved in IBI adoption process. Table 7 provides the estimated results of this model for the first category of outcome indicators related to investment in high-risk high-return inputs. Again note that the double-differencing estimation procedure reduces the risk of selection bias. Results in Table 7 show once again that IBI adoption has a significant impact on household investment in high-risk high-return agricultural inputs. Interestingly, Table 7 shows that the magnitude of the impact of IBI adoption increases with increase in frequency (years) of adoption. Column 2 reports that controlling for all covariates, adoption of IBI for two years have increased total investment in high-risk high-return agricultural inputs by ETB 2214. The effect is statistically insignificant for one-year of IBI adoption. However, three- and four-years of adoption of IBI have increased total investment in high-risk high-return inputs by ETB 4376 and ETB 2583 with ($p = 0.00$) and ($p = 0.01$), respectively. For individual input components, two-, three- and four-years adoption of IBI were observed to increase investment in chemical fertilizers, by ETB 622, ETB 1259 and ETB 1170, respectively, and this is significant at 1% level. Similarly, at 1% significance level, two- and three-years of IBI adoption has increased investment in improved seed varieties by ETB 1525 and ETB 2573, respectively. Table 7 also shows that four-years of IBI adoption has increased investment in improved seed varieties by ETB 1048 at 5% level of significance. Results under column 8 of Table 7 also show that after controlling for all covariates, investment in pesticides/herbicides has increased by ETB 545 and ETB 366, for three- and four-years of IBI adoption, respectively.

The impact of repeated IBI adoption on per capita consumption, estimated using the flexible random effect model is presented in Table 8. Overall results reveal that the impact of IBI adoption on household per capita food, non-food and total consumption increases with years, even though the increase is not uniformly monotonous. Specifically, compared with non-adopters, weekly per capita food consumption has significantly increased by ETB 62 and ETB 61, for two- and three-years of adoption, respectively, after controlling for all covariates. These effects are statistically significant at 1% level. Column 6 of Table 8 also reveals that two- and three-years adoption of IBI has increased household per capita total consumption expenditure by ETB 3310 and 2971, respectively at 1% level of significance. The effect of IBI adoption for four-years on total consumption expenditure is statistically insignificant ($p = 0.17$). This can be due to non-monotonic patterns of increase in household consumption with increase in income. As their income increases, households may spend more on basic consumption items (e.g., food) at first, followed by investment in high-risk high-return inputs (e.g., purchase of chemical fertilizer, improved seed varieties and use of pesticide/herbicides). And, then, households may invest in other consumption items (e.g., health, education, clothing). Hence, it is likely that the cumulative effect of repeated adoption of IBI on household welfare is in line with the usual spending patterns of households as their income increases.

Table 7: Results of flexible random effect model on the impact of intensity of IBI adoption on production

Variables	(1) Total value of high-risk high-return inputs	(2) Total value of high-risk high-return inputs	(3) Value of fertilizers used	(4) Value of fertilizers used	(5) Value of improved seed	(6) Value of improved seed	(7) Value of pesticides used	(8) Value of pesticides used
One-year adoption	-119.42	-491.69	-7.63	-92.83	-72.95	-307.40*	-76.17**	-95.56
	(316.17)	(309.87)	(220.74)	(211.75)	(172.57)	(179.26)	(32.49)	(38.70)
Two-years adoption	2653.79***	2214.28***	784.91***	621.89***	1782.14***	1525.21***	92.70**	67.47
	(305.09)	(319.56)	(132.53)	(133.29)	(260.50)	(264.65)	(46.13)	(55.19)
Three-years adoption	4882.10***	4375.65***	1632.87***	1259.31***	2677.14***	2573.71***	574.93**	544.82**
	(1044.20)	(960.16)	(305.89)	(254.60)	(667.48)	(652.51)	(283.27)	(241.89)
Four-years adoption	3233.90***	2583.34**	1591.68**	1170.74*	1286.58**	1048.03**	346.24***	365.57***
	(1055.91)	(987.11)	(670.90)	(650.86)	(555.41)	(512.43)	(98.99)	(97.56)
Region	-115.77	220.88	-200.81	-218.92*	-97.99	-97.68	155.07***	93.57***
	(190.09)	(190.81)	(121.95)	(131.85)	(120.81)	(132.79)	(38.02)	(31.55)
Age		12.20		4.89		8.01		-0.71
		(11.17)		(4.80)		(8.97)		(1.98)
Gender		-149.40		-133.74		10.29		-25.83
		(259.53)		(136.37)		(199.90)		(31.99)
Education (years)		447.01***		335.16***		100.89		10.91
		(105.41)		(55.42)		(76.63)		(10.55)
Family size		122.02**		51.33*		63.08		7.74
		(52.27)		(28.80)		(47.50)		(12.22)
Dependency ratio		1050.15		287.65		592.17		168.83
		(672.97)		(278.42)		(519.42)		(30.66)
Land size in *qarxi*		92.26**		48.02**		32.04*		12.20
		(33.22)		(11.57)		(17.69)		(10.17)
Livestock size (TLU)		87.94***		60.46**		30.96***		-3.57
		(16.69)		(9.77)		(10.24)		(4.69)
Distance from market		-178.48*		-78.97		-75.95		-23.00
		(91.59)		(52.42)		(53.49)		(21.17)
Extension contact		-303.19		-345.84		-41.91		82.07
		(453.24)		(368.35)		(199.44)		(63.60)
District 1		13.67		840.81***		-930.26**		97.52
		(455.41)		(220.51)		(356.16)		(165.79)
District 2		-789.66*		127.98		764.85**		-156.78***
		(411.18)		(140.15)		(331.57)		(40.12)
Constant	3495.22***	383.44***	2130.87***	32.06	1339.36***	376.70	74.18***	-15.54
	(182.01)	(889.24)	(118.18)	(479.57)	(107.09)	(649.41)	(13.99)	(165.57)
R^2 within	0.0667	0.1294	0.0325	0.1210	0.0505	0.0742	0.0134	0.0296
Wald χ^2	103.86***	352.40***	65.55***	232.27***	61.39***	175.75***	105.84***	135.64***
Observations	2278	2278	2278	2278	2278	2278	2278	2278

*** p<0.01, ** p<0.05, * p<0.1. Standard deviations are in brackets. Standards errors are clustered at iddir level

Table 8: Results of flexible random trend model on the impact of intensity of IBI adoption on per capita consumption

Variables	(1) Per capita weekly food consumption expenditure	(2) Per capita weekly food consumption expenditure	(3) Per capita non-food consumption expenditure	(4) Per capita non-food consumption expenditure	(5) Per capita total consumption expenditure	(6) Per capita total consumption expenditure
One year adoption	-26.16***	-17.12**	-121.91	111.21	-1185.06*	-552.36
	(5.88)	(6.48)	(118.62)	(149.51)	(340.22)	(386.81)
Two years adoption	51.72***	61.62***	371.94	601.87*	2643.88**	3310.17***
	(16.95)	(15.89)	(373.83)	(340.50)	(971.61)	(898.15)
Three years adoption	47.29**	61.18***	-335.73	-29.91	2081.32**	2971.28***
	(18.97)	(17.66)	(232.84)	(232.700	(1046.93)	(957.93)
Four years adoption	9.16	18.19	403.58	502.27	1314.74	1677.25
	(14.77)	(13.80)	(9.190	(645.52)	(1390.12)	(1229.33)
Region	31.91***	6.61	885.00***	336.47*	2279.42***	696.03
	(7.78)	(10.27)	(124.19)	(186.26)	(454.65)	(580.69)
Age		0.19		-15.59*		-7.40
		(0.39)		(8.58)		(21.82)
Gender		-3.25		115.50		40.53
		(15.41)		(254.70)		(869.30)
Education (years)		3.97		288.08***		417.94**
		(3.40)		(92.88)		(201.13)
Family size		-12.46***		-293.08***		-841.87***
		(1.74)		(52.68)		(105.72)
Dependency ratio		-64.06**		-2862.98***		-5607.02***
		(27.00)		(817.19)		(1616.28)
Land size in *qarxi*		0.18		7.63		18.77
		(0.38)		(11.54)		(24.39)
Livestock size (TLU)		0.91***		29.36***		84.39***
		(0.32)		(9.470		(23.84)
Distance from market		-0.95		3.22		-41.57
		(2.800		(80.930		(170.2)
Extension contact		-17.50		-385.29		-894.76
		(16.98)		(338.28)		(908.86)
District 1		1.30		550.91		541.86
		(14.810		(578.92)		(948.66)
District 2		-28.08		-117.16		-1202.82
		(14.30)		(431.80)		(848.09)
Constant	58.83***	204.19***	1119.05***	4726.84***	4017.35***	13494.95***
	(3.75)	(31.49)	(76.96)	(1174.84)	(243.71)	(1999.20)
R^2 within	0.0262	0.0775	0.0099	0.1105***	0.0244	0.1067
Wald χ^2	105.02***	352.74***	60.78***	186.87	98.85***	322.17***
Observations	2278	2278	2278	2278	2278	2278

*** p<0.01, ** p<0.05, * p<0.1 Robust standards errors clustered at iddir level are in brackets

Table 9 presents p-values for one-sided t-test to understand the extent to which marginal impacts of IBI adoption increase per cycle of adoption. The test is performed as post-estimation parameter test for the equality of the parameters of four-, three-, two- and one-year adoption of IBI, for all individual outcome indicators. As shown in Table 9, the results are mixed. Testing the null hypothesis for the total investment in high-risk high-return inputs, $\gamma_k = \gamma_l$ (with $l < k$) versus the alternative hypothesis $\gamma_k > \gamma_l$, we find that the null hypothesis cannot be rejected at a 5% significance level for four-years of adoption, compared to three or two-years of adoption (p-values are 0.24 for $\gamma_4 = \gamma_3$ and 0.72 for $\gamma_4 = \gamma_2$). But the estimated p-value is 0.003 for $\gamma_4 = \gamma_1$. These test results reveal that four-years of IBI adoption does not lead to a statistically significant increase in investment in high-risk high-return inputs as compared with three- or two-years of adoption, but it leads to significant increase in investment in high-risk high-return inputs, compared with one-year adoption. Similarly, three-years of IBI adoption has led to a statistically significant increase in investment in high-risk high-return inputs over two years ($p = 0.025$*).* Three years of adoption has led to a statistically significant increase in investment in high-risk high-return inputs over one-year adoption (see that $p = 0.00$ for $\gamma_3 = \gamma_1$ with respect to total investment in high-risk high-return inputs). Two-years of IBI adoption has also led to a statistically significant increase in investment in high-risk high-return inputs than one-year adoption with $p = 0.00$ for $\gamma_2 = \gamma_1$.

Table 9: Post-estimation parameter test for monotonicity of the estimated impacts overtime

Variables	$\boldsymbol{\gamma_4 = \gamma_3}$	$\boldsymbol{\gamma_4 = \gamma_2}$	$\boldsymbol{\gamma_4 = \gamma_1}$	$\boldsymbol{\gamma_3 = \gamma_2}$	$\boldsymbol{\gamma_3 = \gamma_1}$	$\boldsymbol{\gamma_2 = \gamma_1}$
High-risk high-return investment	0.239	0.720	0.003	0.025	0.000	0.000
Investment in fertilizer	0.898	0.409	0.067	0.020	0.000	0.002
Investment in improved seed	0.109	0.417	0.013	0.095	0.000	0.000
Investment in pesticides	0.490	0.006	0.000	0.098	0.018	0.000
Per capita food consumption	0.024	0.007	0.027	0.984	0.000	0.000
Per capita non-food consumption	0.971	0.548	0.076	0.065	0.000	0.000
Per capita total consumption	0.293	0.186	0.078	0.772	0.001	0.000

Standards errors are clustered at iddir level. Indicated values are p-values of one-side t-test

Four-years adoption does not provide a statistically significant increase in investment in fertilizer than three-years adoption ($p = 0.90$), two-years adoption ($p = 0.41$) or one-year adoption ($p = 0.07$), at 5% significance level. But three-years adoption has led to higher purchase of chemical fertilizer than two-years as well as one-year of adoption. The same is true for two-years adoption over one-year adoption of IBI. With regard to investment in improved seed varieties, Table 9 shows that four-years IBI adoption has no statistically significant increase than three- or two-years of adoption. But three-years adoption has led to a statistically significant increase in investment in improved seed varieties than one-year adoption. Two-year adoption of IBI were also led to a statistically significant increase in investment in improved seed than one-year adoption at the 1% level of significance. Considering the impact of IBI adoption on consumption expenditure per repeated adoptions, Table 9 shows that four-years adoption has led to a statistically significant increase in per capita weekly food consumption than three-years adoption ($p = 0.02$), two-year adoption ($p = 0.01$) and one-year adoption ($p = 0.03$), respectively. Three- and two-years of IBI adoption has also led to a statistically significant increase in per capita weekly food consumption than one-year adoption at 1% level of significance. At 5% level of significance, four-years of IBI adoption does not led to a statistically significant increase in per capita total consumption than any of the three-, two or one-year of adoptions (p-values are 0.29; 0.19 and 0.08, for $\gamma_4 = \gamma_3$, $\gamma_4 = \gamma_2$

and $\gamma_4 = \gamma_1$, respectively). The non-monotonic increase in household consumption and investment in inputs over the IBI adoption period, for instance, among four-year versus three-, two- or one-year adoption is not surprising, because households eventually shift attention from investing in high-risk high-return inputs, or consumption to other activities. Compared with the average impact obtained from the individual-specific fixed effect model, the results in table 7 and 8 support the hypothesis that IBI adoption has a lasting impact over time. While the impact of one-time adoption is not significant in most of the cases, having adopted multiple times leads to higher levels of consumption and investment in high-risk high-return inputs. The increase in level of consumption can be from direct collection of payouts, or it can be reinforced due to increased investment in high-return inputs, that can increase productivity.

Conclusion

This study evaluates the impact of IBI adoption on two categories of welfare indicators: household consumption and investment in high-risk high-return inputs. Self-selection and program placement biases often complicate causal attributions of welfare improvements to rural interventions. This study employs difference-in-difference techniques to deal with these potential selection biases. The fixed-effect equivalent of the difference-in-difference model helps to reduce selections based on time-invariant unobservables. This study used a two-round panel data collected from smallholders in the Rift-valley zone of south-eastern Ethiopia. IBI adoption in the area has lasted four years before this study, so that lasting welfare effects can be established. The analysis in this article starts with tests of self-selection and program placement biases. The tests indicated that biases prevail due to systematic program placement and self-selection of participants. Our analysis, thus, accounted for potential selection bias through double differencing, and through including control variables for the order of program placement and individual self-selections. The results indicate that adoption of IBI has significantly increased both investment in high-risk high-return inputs and per capita household consumption expenditure, which are important indicators of welfare in the study area. The random effect model with flexible specification that takes the frequency of adoption into account has shown that investment in high-risk high-return inputs as well as per capita household consumption were increased with the frequency of IBI adoption. One-time adoption had no significant impact on these outcome indicators. Repeated cycles of adoption, however, do matter to achieve significant welfare impacts from IBI adoption. Even though there are certain deviations between the results of the fixed-effect and random-effect models, due to different assumptions, specifications, and estimation techniques, both strongly suggest that adoption of IBI in this specific Rift-valley zone of east Africa has been useful in terms of the measured household welfare outcomes. These findings have a number of implications. First, they show that the effect of IBI adoption on household welfare can be multidimensional and may not be fully captured by just a single household welfare outcome indicator. Moreover, estimated impacts are not monotonous over time. Second, the results also indicate that the effect of IBI adoption lasts longer than one or two years. It takes time before the effect of IBI adoption on consumption or investment in high-risk high-return inputs is fully materialized. Therefore, impact estimates that rely on a single household welfare indicator and focus only on one cycle of IBI adoption may underestimate the potential welfare gains that can be achieved overtime. Future research must focus on more robust specifications that incorporate temporal as well as multidimensional effects of IBI adoption on household welfare. Finally, an important implication of these results for microinsurance firms like OIC is that IBI-adopter households should be encouraged not only to participate in adoption once, but also to sustain their uptake by renewing the IBI policy, and by remaining for longer periods in adoption phase in order to realize the full potentials of welfare gains.

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