THE IMPACT OF COMMERCIAL RAINFALL INDEX INSURANCE: EXPERIMENTAL EVIDENCE FROM ETHIOPIA

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Abstract

We present the results of an experiment introducing commercial rainfall index insurance into drought-prone farming cooperatives in Amhara Region, Ethiopia. We provided a market-priced rainfall deficit insurance product through producer cooperatives, and test a number of potential ways to kick-start private demand. Take-up of the insurance at market prices is very low, between 0.5% and 3% across seasons. When we use a randomized experiment to distribute small free insurance contracts to farmers, 39% of subsidized individuals enroll but this fails to stimulate input use, yields, or income, and nor does it enhance demand in subsequent seasons. A training and promotion on the product improves uptake and willingness to pay, but also does not improve farming outcomes. We conclude with a case study of our efforts to interlink index insurance with credit for agricultural inputs.

Keywords: Index Insurance, Randomized Experiments, Ethiopian Agriculture

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1. INTRODUCTION

Smallholder farmers are beset by an interlocking set of market failures, and when credit and insurance markets are missing, farmers can become trapped in a low-investment equilibrium (Rosenzweig & Binswanger 1993). Production risk likely hampers farmers' adoption of higher yielding technologies (Dercon, 2005), particularly when these shocks are covariate (Townsend, 1994, Conning and Udry, 2007). Risk-driven reluctance to invest in inputs such as fertilizer and improved seeds may be partially responsible for the fact that Africa has not undergone a 'green revolution' (Skees and Collier 2007). This set of insights has raised hopes that weather index insurance (WII) could succeed as a commercial product and provide substantial welfare improvements for risk-exposed populations (Barnett and Mahul, 2007). Unfortunately, while several recent studies have shown dramatic effects of index insurance on producer behavior when provided at subsidized prices (e.g., Mobarak and Rosenzweig 2013, Karlan et al., 2014, Elabed and Carter 2014, Cai et al. 2016), weather index insurance has struggled as a commercial product (JPAL, 2016; de Janvry et al. 2017) and has seen low demand at market prices (Giné & Yang 2009, Cole et al 2013).

Even if micro-insurance looks unlikely to enjoy the same immediate commercial success as micro-credit, important questions remain as to how risk protection can best be used to improve welfare. A standard model of the utility benefits of insurance is based on the smoothing of consumption, typically *ex post* to the shock. A special case for insurance can be made if in addition it has an *ex ante* effect on the decisions of producers, allowing them to increase productivity as they are protected from risk. To achieve this potential first-order improvement in welfare farmers must be directly informed of the protection they will receive, and so marketing insurance directly to individuals appears particularly attractive. Absent this response, integrating index insurance protection into the activities of higher-level entities such as banks (Farrin and Miranda 2015; Mishra and Miranda 2017) or local governments (de Janvry et al. 2016) is likely a more straightforward way to achieve *ex post* risk coverage. This study is designed to examine whether individually-marketed index insurance can create this supply response, experimenting with subsidies and training to examine their effect on demand and agricultural decisions.

Two distinct cases can be made for using subsidies to provide individual insurance at sub-market prices. The first is a business argument: given the unfamiliarity of WII products as well as the demonstrated dynamic impacts of subsidies (Hill et al. 2016) and payouts (Cole et al. 2014, Stein 2016), temporary subsidies can build a path to a sustainable long-term market (Cai et al. 2016). Second, linkages between credit and risk imply that subsidies provided in the form of risk reduction may have a larger effect than a cost-equivalent lump-sum transfer (Karlan et al., 2014; Jensen et al. 2014). Numerous recent papers have shown that subsidized insurance can indeed induce farmers into growing the higher-returning, more weather-exposed crops covered by the insurance (Mobarak and Rosenzweig 2012; Cai 2016; Cole et al. 2017), suggesting that risk poses a first-order drag on agricultural productivity that can be alleviated with subsidized WII. The effects of training on insurance are more inconclusive, with recent experimental studies finding either weak (Cole et al. 2013) or short-term (Hill et al. 2016) effects on demand. In this paper we present the results of an experiment that attempts to address these questions by cross-randomizing cost vouchers and an informational promotion of rainfall insurance among cooperativized Ethiopian farmers. We also present a case study of a product that addresses interlinked credit and insurance market failures by offering farmers loans explicitly backed by the insurance policy.

Our study, called EPIICA (Ethiopian Project on Interlinking Insurance with Credit in Agriculture), set out to build a private-sector driven rainfall index insurance product linked with seasonal credit and to field it among drought-exposed farmers in Ethiopia's Amhara State. We addressed the credibility problem by working with the largest private insurance company (Nyala Insurance) and the largest private lender (Dashen Bank) in the country. Our partners identified market-friendly, drought-exposed villages with potential for increased productivity, and developed a product that uses Ethiopia's robust agricultural cooperative system to aggregate demand for rainfall insurance. The product was designed to cover the cost of purchased inputs to farming, namely fertilizers and improved seeds. We conducted three consecutive years of standalone weather index insurance sales (Table A1 in the appendix), during which we experimented with vouchers that provided small amounts of free insurance at the farmer level, re-randomizing each year so as to understand the dynamic demand effects of subsidies. A randomized marketing exercise conducted at the time of baseline tests the importance of farmer comprehension of the details of an index insurance product. Four rounds of panel surveys at the household and plot level allow us to analyze the impact of the insurance vouchers on welfare outcomes such as input use, productivity, and income.

Our results paint a mostly negative picture of the prospects for standalone index insurance. Despite well-known commercial partners and the selection of high-potential regions of the country, uptake in the absence of subsidies was very low, between 0.5% and 2% of cooperative membership at commercial prices (~130% of actuarially fair). The subsidies for insurance uptake had a very strong effect on coverage rates (~40% of those offered vouchers registered to be insured); yet similar to Takahashi et al. (2016) we find that subsidies have no dynamic effect on private demand. Where our results differ from prior studies such as Karlan et al. (2014) is that we then find no downstream effect of subsidized risk protection on agricultural practices or income. More promisingly, we do find that the simple and inexpensive insurance promotion we conducted at baseline had a lasting effect on demand, and is the only intervention we tested that led to meaningful willingness to invest one's own money in premium purchases.

Relative to the literature, the surprising result from our study is the lack of impacts on producer behavior even when the insurance is provided for free. We explore four dimensions that might explain this unresponsiveness. First, we provided only relatively small insurance subsidies to farmers (average premium voucher value of $20, maximum payout implied of $80). We have an almost perfect ability to predict the sum insured with the voucher amount, and even when we examine the slope effect or the impact at high voucher amounts, there is no indication of an input use response. Second, we examine whether we have a strong effect restricted to the sub-population of individuals risk-rationed at baseline. While these individuals are more likely to take up insurance when offered a voucher and invest less overall in inputs, there is no differential impact of the treatment on the risk rationed. Third, could the indirect marketing strategy pursued by the insurer to lower administrative costs and improve commercially viability, be responsible? We use a survey of comprehension in the fourth round to examine this, and find that while a very high fraction of the treatment sample knew of and had considered the product, detailed understanding of the contract was lacking and very few had direct contact with the insurer. Finally, we discuss contextual features that may have caused the product to struggle in Ethiopia in particular, but find no heterogeneity in uptake based on cooperative membership or exposure to the main government safety net program.

Overall, our study shows that the psychology of protection from risk among smallholder producers is not easily created. Despite the backing of a well-known national brand, near universal awareness of the program, and excellent understanding of the subsidies, input investment behavior did not change in a first-order sense. Our analysis of channels leaves the poor comprehension of the details of the insurance and the indirect relationship with the insurer as prime suspects. Even the fact that our training had strongly significant effects on uptake of the insurance reinforces the critical role of familiarity. In light of these results it appears that serious investment is required to create the *ex ante* response to risk protection, and absent this it may make more sense to focus attention on meso-level entities who can provide *ex post* risk protection with fewer transactions costs.

In the final section of the paper we provide a case study of the one cooperative in which we were able to successfully field the interlinked insurance-backed credit product. Our intended study design added to the standalone WII insurance an 'interlinked' product that offered agricultural loans on the condition that the farmer would purchase rainfall insurance to cover the loan. This has the potential to unlock substantial credit demand by reducing risk-related obstacles to investment. Theoretically, Carter et. al. (2016) showed that when covariate risk is a large share of total risk faced by farmers, and when there is lack of credit, such a product would be demanded by small farmers, and would increase input use and output. However, several other recent studies found that bundling insurance with a credit contract actually *decreased* demand for fertilizer relative to a standalone credit product (Giné and Yang, 2009, Banerjee et. al. 2014). The most likely explanation for these results seems to be that farmers were interested mostly in credit, and anything that increased, albeit in the short-term, its the cost (such as insurance premiums) would lower credit demand and the linked insurance. In the end, the interlinked product proved a substantial challenge to field for reasons that speak to the core issue of the reassignment of agricultural risk within the credit system. Nonetheless, where we were successful in fielding this product a substantial improvement in credit access and input use appears to have resulted.

The paper is organized as follows: Section 2 describes the setting, research design and data collection strategy for the study, as well as the construction of the index and basic statistics on demand. Section 3 analyzes the impact of insurance vouchers on farmer behavior, Section 4 investigates four reasons for the low impact of our intervention, and Section 5 presents the case study of the cooperative where we fielded the interlinked product. Section 6 concludes.

2. STUDY CONTEXT.

2.1. Inputs and Credit in Ethiopian Smallholder Agriculture.

Ethiopia is a very poor country by world per capita GDP standards (it ranks 209 out of 228 countries listed by the World Bank). Nevertheless, it has been growing quickly in the last few years (on average 10.2 percent per annum during 2010-14). Agriculture is the main productive sector of the Ethiopian economy. Most of Ethiopian agricultural production takes place under rain-fed conditions and is subject to considerable weather variation. Dercon and Christiaensen (2011) found that fertilizer use in Ethiopia, while profitable, is risky, and that the lack of insurance against risk leads to low input use and inefficient production choices. These results provided the motivation for the project reported in this paper. Risk aversion is well accepted as a factor inhibiting the adoption of fundamentally new technology such as chemical fertilizers and improved seeds. In addition to production risk, Dercon and Christiaensen (2011) showed that ex-ante consumption risk could also affect fertilizer use. Similarly Lamb (2003) showed that risk avoidance in the face of incomplete insurance may be key in understanding limited fertilizer use.

Given the transformative effect of agricultural technology on yields, risk-driven barriers to adoption present an obvious potential poverty trap (Christiaensen and Demery, 2007), and hold out the possibility that risk-protecting products such as index insurance may not only provide second-order benefits through variance reduction, but first-order benefits through enhanced willingness to take profitable risks among producers. Despite large secular increases in fertilizer use and household income over time, overall input use and productivity remain low.[1] Per hectare fertilizer consumption is less than one fifth of that of other developing countries (Morris, et. al. 2007). Dercon and Hill (2009) report that fertilizer intensity per hectare of fertilized area has not increased between 1997/98 and 2007/08, despite the apparent doubling of total fertilizer sales

[1] A host of demand and supply side factors have been invoked to explain the limited adoption of fertilizer in Ethiopia including limited knowledge and education (Asfaw and Admassie, 2004, Yu et. al. 2011), risk preferences, credit constraints (Croppenstedt, Demeke and Meschi, 2003), irregular rainfall (Alem et. al. 2008), limited profitability of fertilizer use (Dadi, Burton, and Ozanne, 2004), lack of market access (Abrar, Morrissey, and Rayner, 2004), incomplete markets (Zerfu and Larson, 2010), inefficiency of input use (Yu et. al. 2011), as well as limited or untimely availability of the inputs themselves.

during the same period. This can be largely explained by a heavy reliance on extensification, via expansion in the cultivated area.

The absence of productive credit may be an important reason for the low use of improved inputs and hence lower productivity among African rural smallholders. Many studies have found that small farmers in developing countries are credit constrained and as a consequence use few purchased inputs (Besley, 1995; Conning and Udry, 2007).[2] On the supply side, banks may find it very risky and expensive to provide credit to rural smallholders, thus rationing the supply of credit or making available contracts that maybe too expensive or too demanding on collateral. On the demand side, even in situations where credit is available farmers may find it too risky to borrow (Boucher et al. 2008). Recently Abate et al. (2015) showed that access to institutional finance has significant positive impacts on both the adoption and extent of technology use in Ethiopian agriculture. They also showed that cooperatives have a greater impact on technology adoption than do Micro Finance Institutions (MFIs). The high cost of credit also adds to the cost of fertilizer. According to the Ethiopian Rural Household Survey 1994-99 (ERHS), in 1999 71 percent of those purchasing fertilizer used formal seasonal credit provided via parastatals, and the implicit median interest rate was calculated at 57 percent (Dercon and Christiaensen, 2011).

The system of input distribution and financing in Ethiopia has evolved over time, but consistently features a dominant role for state-led actors. While thriving private output markets exist, the government is responsible for fertilizer imports, and the cooperative sector is used as the primary delivery vehicle for most improved inputs. In Amhara region, the Cooperative Unions handle the wholesale demand aggregation function, pre-ordering inputs through a public enterprise (AISE), and also serving as a conduit to financing through the publicly owned Commercial Bank of Ethiopia. Our fieldwork found that the financing chain in recent years has been subject to a game of chicken, whereby the government announces early in the season that fertilizer would be distributed on a cash-only basis, but once it becomes clear that this would result in low demand and excess stocks of fertilizer they injected public-sector credit later in the season. Regional governments are required to underwrite the risks of smallholder input financing, and have been eager to find ways of shedding this risk, as any farmer arrears have to

[2] Other recent analyses, however, emphasize the poor quality of soils that make adoption unprofitable (Marenya and Barrett, 2009), or the heterogeneity of farmer profitability of fertilizer use (Suri, 2011).

be paid by the state budget. Cooperatives, local government agents, and local agricultural extension workers have been pressed into service to assist with the collection of the debts used to finance inputs, so individual borrowers take on substantial risk when they finance inputs obtained through the cooperatives.

The Ethiopian context, then, is an ideal one in which to pilot index insurance from the perspective of exposure to risk, and several recent studies have been conducted in the country (e.g. Hill and Viceisza 2010; Berhane et. al. 2014; Dercon, et. al. 2014; Oxfam's HARITA project described in OXFAM 2014; and McIntosh et al. 2016). However, from the institutional perspective Ethiopia is a challenging environment in which to introduce a novel and complex private-sector financial service. Rigidity of input supply chains, strong government-led approach to both credit and fertilizers, and the presence of the massive Productive Safety Nets Program (PSNP)[3] in Amhara Region all reinforce the central role that the government plays in agriculture and risk management. The high climate variability and its central role in driving risks in Ethiopian agriculture makes reinsurers reluctant to provide coverage, and increases the price of reinsurance. Under the current system, most financial drought risk is absorbed by regional governments, who also must provide loan guarantees for all publicly-backed credit moving through the state input financing system. All of this implies that the public sector plays a driving role in absorbing agricultural risk, and while these entities may be eager to spread these risks more broadly it is likely to be a challenging context in which to engineer private-sector institutional means to do so.

2.2. Index Design.

We now describe the insurance product in more detail. The starting point was the powerful village-level cooperative system, through which most inputs in the Ethiopian smallholder supply chain move. Recognizing the importance of local networks as means for spreading information (Cai et al. 2015) and sharing the remaining risks in the face of an imperfect index (Mobarak and Rosenzweig 2013; de Janvry et al. 2014; Dercon et al. 2014), the cooperatives provide a potentially promising avenue through which to offer index insurance products. Given the potential trust problems with asking cooperatives to smooth risk (McIntosh

[3] Productive Safety Net Program (PSNP) is a national program, started in 2005, that helps vulnerable families to ensure minimum nutrition.

et al. 2018) and the importance of making sure that the input decision-maker is directly aware of risk protection, we elected to market the product through the cooperative network but to designate specific individual buyers as the explicit beneficiaries of the insurance policies.

The focus of the project was to improve input use and therefore the insurance product was conceptualized as insuring the value of purchased inputs rather than trying to cover the variation in the value of output. The input purchases of an average-sized farm in Amhara operating at the input intensity recommended by the Ministry of Agriculture would be roughly 4,000 birr ($278). We worked with an agronomic expert to build a crop water requirement model for every village and every crop for which we sought to offer insurance, namely the major food and cash crops, teff, wheat, sorghum, and maize. The contract took a standard 'trigger and exit' form, whereby the payout was a linear function of rainfall below the point at which the agronomic model began to predict agricultural losses (trigger) and then made a complete payout on the contract once the cumulative rainfall fell below the critical (exit) value for that location and crop.

The farmer could choose to purchase whatever multiple or fraction of this contract he/she wished. The contract stipulated a graduated payment based on a rainfall index, which was based on rainfall observed at a station nearby (within 20 km). We focus on the productive *meher* season (the rainy months of June, July, and August), within which each crop's growing cycle was divided in three segments (early, growth, and maturation). A maximum and minimum rainfall level were specified for each period, relating to the zero and full damage to the crop respectively, and the maximum payout in each period was specified, depending on the estimated value of the crop that accrues in each period.. The actual payout in each period was estimated as an increasing share of the rainfall deficit from the maximum in the given period, so as to be zero for rainfall above the maximum (trigger), and 100 percent of the period amount, if rainfall was below the minimum threshold (exit). We used rainfall data covering a period of at most 40 years, obtained from the National Meteorological Agency (NMA) of Ethiopia. The definition of periods and trigger and exit points was based on agrometeorological models of crop water requirements developed at the Food and Agriculture Organization (FAO) of the United Nations, and was specified so as to give a full crop failure approximately once every 10 years. The entry and exit probability values are selected as 0.2 and 0.1, respectively indicating some payout every 5 years and full payout every 10.

Premium costings were accordingly based on the average payout to farmers, had the system been operated historically over a long time period. Since the number of years with precipitation data is relatively short (normally 30 years, but actually shorter in many cases because of missing data), a classical stochastic weather generator (M&Rfi) was used to generate 500 years of data with the same statistical properties as the years actually observed.[4] The premia thus calculated were different in different zones, given their different rainfall probability profiles. The crop water requirement model is built to measure stress on the crop, and hence actuarially fair prices are higher in drier locations, and so our product was expensive where it was most valuable and inexpensive where it triggered less often. The final market premium price was augmented by 25 percent above the actuarially fair premium to provide the commercial margin for Nyala in offering the product (usually called "loading"). The first sales year (summer of 2012) was characterized by a widespread drought, and the insurance triggered in 42% of the study villages for at least one crop. The average payout in the first year was $27, the minimum $3 and the maximum $121; payout rates ranged from 3% to 75% of the sum insured. In the second sales year (summer 2013) the index triggered for only 13% of villages in at least one crop, with smaller payouts averaging $9. Given an average input expenditure of $172 on average across all inputs, the observed payouts based on the voucher experiment cover just under 20% of the value of inputs.

How well does the index correlate with our survey-measured agricultural yields? To examine this question we take the primary yield outcome (a normalized index of yields across all crops grown by a farmer) and examine the variation in this variable across villages and across years.[5] We present two ways of thinking about the quality of the index. First, in Appendix Table A2, we show a simple two-by-two table of the village/years in which the index would have paid out versus whether our survey-based measure of yields indicates a shock occurred (as measured

[4] M&Rfi includes a Markov chain (where sequences of dry/dry, dry/wet, wet/dry and wet/wet periods are taken into account), that models rainfall using a Gamma distribution to take account of the positive skew in the rainfall distribution (many low values with some high values) as well as an autoregressive model to take into account pseudo-cycles (Dubrovský 1997; Dubrovský et al 2000, 2004).

[5] The yield index includes the five main crops grown and insured in Amhara: teff, maize, wheat, barley, and sorghum. For each crop we calculate a mean and standard deviations of yields per hectare based on the control group distributions in the pre-treatment rounds. We then Z-score the yields for each crop by subtracting the mean and dividing by the standard deviation for each crop, and take the average value of this normalized metric for all farmers who grow the crop in that village/year. This measures how many standard deviations from the average yield per hectare are farmers across all crops in a village/year.

by normalized yield outcomes that are in the bottom 20% of the distribution). This shows that of the 79 village/years in which the survey indicates no yield shock, we have index payouts (false positives) in 21% of them. For the 19 village/years in which the survey does indicate a yield shock, the index pays out just over half of the time.

Second, Appendix Figure A1 plots the Cumulative Density Function of normalized yields for village/years in which the index did and did not pay out. In the left-hand figure we perform a placebo test by analysing survey-based yields in the year before insurance sales started as a function of whether there would be payouts in the future. Here we should see the distributions of yields very similar, and indeed we do. In the right-hand panel we then plot the same distributions for the subsequent year in which the index does pay out. In this year, the distribution of yields in villages where the index does not pay out first-order stochastic dominates the distribution where it does, and there is a particularly pronounced mass of farmers with yields far below average where payouts occur. A Kolmogorov-Smirnov test shows the CDFs in the right-hand panel to be significantly different at the 99% level. This pair of graphics illustrates that payouts and yields are strongly contemporaneously negatively correlated and do not appear to suffer from any spurious correlation. Hence, we conclude that our index was meaningfully correlated with yields, although the correspondence is not perfect and is stronger in the second sales season (when there were fewer payouts) than in the first season (when there were many).

2.3. Study Design.

The identification of the original 120 study villages (kebeles) in Amhara region was conducted by Nyala Insurance, who selected regions that were generally drought-exposed, within 20 km of a rainfall station, and that they believed to be good markets for weather insurance with well-functioning village cooperatives (see Appendix Figures 2A and B for maps of the study area). The selection of the Kebeles was purposive, and designed on the basis of informed opinion of Nyala as to where in the Amhara region the market for WII has best potential.[6]

Within study villages we then randomly sampled 18 households from within the membership of the village agricultural cooperative, and 2 households from the non-cooperative

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[6] The selected zones in Amhara are North Shewa, West Gojam, South Wello, and North Wello

farming population of the village.[7] Appendix Table A3 provides summary statistics by district from the baseline, underlining the extremely low education levels among household heads prevalent in our sample.[8] We conducted a second, shorter baseline survey in 2012 collecting the primary agricultural outcomes only so as to provide a more powerful estimate of pre-treatment outcomes, as advocated in Bruhn and McKenzie (2009). The baseline data were used to conduct a village-level randomization stratified on average fertilizer use and blocked on region, that assigned villages into three arms of 40 villages each: standalone villages in which only WII was offered, interlinked villages that offered both standalone WII and insured loans, and a control. In both types of treatment villages we conducted household-randomized experiments giving vouchers for small amounts of free insurance coverage, and insurance training.

Unfortunately, several implementation problems altered the intended design. First, the reinsurer proved unwilling to underwrite the product in a subset of weather stations that had fewer than 30 years of valid data. Taking the entire study sample that reported being drought exposed in the baseline survey as well as being within twenty kilometers of one of the nine re-insurable weather stations, the reduction of the eligible weather stations left us with a sample of only 49 villages in which we could reasonably field the insurance product. Second, delays in the implementation of the credit side meant that it was only possible to offer the interlinked product after the experimental phase of the project had ended (see Section 6 for a non-experimental description of our interlinked treatment), and so all insurance studied in the main tables for the paper is standalone. The effective research design for the village-level experiment therefore compares the 34 villages in which standalone insurance was offered to the 15 experimental control villages in which it was not. Figure 1 indicates the original and eventual study sample.

Fortunately, the experiment also features two dimensions that were randomized at the household level. These individual experiments provide us with a source of variation which is still experimental (being blocked by village or subsequent to attrition) and relatively high-

[7] Because fertilizers are procured exclusively through primary cooperatives and their upper level zonal Cooperative Unions (CUs) in Ethiopia, it was anticipated that cooperative households may display a higher propensity to uptake additional fertilizer if risk concerns can be ameliorated. We focus the analysis in this paper on the cooperative membership only.

[8] From appendix Table A2 it can be seen that 51 percent of household heads have no education, only 21.5 percent received any formal education, among whom the average years of schooling is 4.8 years. A full 56.5 percent of household heads cannot read or write.

powered statistically. First, at the time of the baseline survey we selected a random subset of cooperative farmers to receive a promotion and training in the way that the index works. Because we were concerned that the indirect marketing techniques used by Nyala might generate only an imperfect understanding of the product (they trained village level agricultural extension officials to work with village cooperative leaders to promote the product to farmers) we employed the EEA to conduct these randomized promotions as a part of the baseline activity. EEA was hired using research funds to give an extended promotion of the product, including conducting a 15-minute presentation of the details of the product for a randomly selected six households per kebele, discussing and answering any questions the household had about the way the product works, and leaving behind a flyer indicating how Nyala would use rainfall at the nearest station to determine payouts, and which historical years the product would have paid out in that village.

Secondly, in all years where rainfall insurance was offered, a random subset of households in treatment villages received vouchers that they could use to obtain some insurance coverage for free. The size of the free insurance voucher was designed to be 10, 30, and 50 percent of the amount of insurance premium that the farmer would need to cover inputs for the average sized farm (calculated as a function of average land size and the recommended input usage in Amhara). Each type of voucher was offered to 5 farmers in each kebele. Given 34 treatment kebeles, a total of 170 farmers received each type of voucher for a total of 510 farmers out of the 680 farmers surveyed in the treatment kebeles. In sales season 2, vouchers were re-randomized at the household level. Finally in early 2016 a small group of 120 households in the only kebele which successfully fielded the interlinked credit-insurance product were surveyed. Table 1 presents the final design of the household-randomized component of the study in the 49 villages that were panel tracked as a part of the survey. Appendix Table A4 shows how core study outcomes evolved across time; despite impressive overall improvements in input use the study sample has very high levels of food insecurity and farm size fragmentation is occurring quickly.

2.4. Attrition and Balance.

As described above, the study is subject to two kinds of attrition. The first is at the village level, where we lost locations to the study due to (a) lack of exposure to drought and (b) rainfall stations that were not re-insurable. We lost almost 60% of our sample due to these sources of village-level attrition. Within the panel sample of villages, we then attempted to track all study households in these villages, and achieved a four-round tracking rate of 92.5%. Tracking this sample of 20 households each in 49 villages, we conducted two post-treatment household and cooperative-level surveys, one in 2014 and one in 2015. Table A5 analyzes the extent to which the two types of attrition from the study are correlated with treatment status. First, in columns 1 and 2 we examine whether the rate of attrition is differential across village-level treatment status, and find that both the original standalone and interlinked arms were lost to the study at rates from 7.5 to 12 percentage points lower than the control. These differences are not significant. Given the high and potentially differential attrition subsequent to randomization, this village-level component (whether insurance offered) cannot be considered cleanly experimental.

Despite the village level attrition the farmer-level randomization should still be clean because the training was blocked by village, and vouchers were blocked and randomized subsequent to village-level attrition. Columns (3) and (4) of Table A5 show that household-level attrition is low overall (9% in the control) and not significantly different across those who receive the voucher or training treatments. Appendix Table A6 examines the balance of the attrited sample on baseline covariates (Panel A) and baseline outcomes (Panel B) and finds the panel experimental sample to be well balanced overall (only 5 comparisons out of 80 significant at 5% level). Based on this analysis of attrition and balance we place the emphasis of our analysis on the individually-randomized treatments.

2.5. Analysis of uptake of insurance .

As presented in McIntosh et al. (2013), we conducted a stated willingness to pay study at baseline, which indicated that almost 50% of the study sample would be willing to purchase standalone index insurance at actuarially fair prices. Table 2 illustrates that this stated WTP radically overestimated the actual demand for the product. The table shows simple summary statistics of the rate of take-up and the average sum insured across the various years and arms of the study. The study featured only one-sided non-compliance, meaning that there were no individuals who received insurance in the control villages at any point in the study. Distressingly, there were no surveyed households who *ever* purchased insurance in the absence of being given a voucher and/or promotion to do so (although voucherless farmers outside the study sample did so). It was typically the case that individuals 'purchased' mainly the amount provided by the voucher and rarely put their own money into insurance premium payments. Overall, the fraction of the total membership of treatment cooperatives (including those not sampled to be surveyed) who purchased insurance in the absence of any voucher is roughly .5%. This makes it clear that the insurance, as priced to reflect all costs, and as promoted through the indirect marketing strategies used by Nyala, was simply not viable as a commercial product. The average premium paid towards insurance in treatment villages during the first sales season without the training was \$3.77, and the training added another \$3.75 on to this figure (Table 2, third row). However, roughly 95% of this value came from the vouchers rather than from true willingness to pay in farmers' own money, and so and what we study here is almost exclusively the decision to accept free insurance, and the impact of receiving it. In the second sales season both willingness to accept the vouchers and the amount paid from farmers' own pockets rose, but because the voucher amounts were smaller the overall sum insured fell. Consequently, as shown in Appendix Figure A3 the average sum insured at the individual level is an almost linear function of the voucher amount, a feature we exploit in Section 4.2. The sum insured was slightly higher in the first sales season than the second because the voucher amounts were larger. Non-cooperative members take up standalone insurance at about 2/3rds the rate of coop members.

The more optimistic perspective on Table 2 is that the two individually randomized components of our insurance promotion had very strong effects on the probability that a farmer is actually covered by the insurance. In both seasons the vouchers have an effect significant well above the 99% level on insurance uptake. The EEA-administered training and promotion is also independently effective; conditional on already having received a voucher, also receiving the training has an effect on uptake in the first sales season and roughly doubles the premiums paid by farmers. In the second sales year the additional effect of the training had faded but trained farmers were putting \$1.46 more of their own money into premiums, significant at the 10% level. Hence, while our study suffered from multiple forms of attrition at the village level and did not generate mass-scale uptake, we have delivered a statistically relatively well-powered experiment in what happens to farm outcomes when farmers are covered by WII. We now proceed to analyze the impact of these individually-randomized components on farm-level outcomes.

3. IMPACT ANALYSIS.

3.1. Intention to Treat Effect of Free Insurance Vouchers.

We can use the experiment to track out the impact of being insured on the primary objective outcomes of the study: use of fertilizer, use of improved seeds and credit, agricultural yields, and household income. The best powered regression specification is the standard panel fixed effects difference-in-differences (DID), namely:

$$y_{ict} = \alpha_i + \delta_t + \beta_1 T_{ct} + \beta_2 V_{ict} + \varepsilon_{ict} \quad (1)$$

Here α_i is an individual fixed effect; δ_t is a fixed effect for each of the post-treatment survey waves; T_{ct} is a panel treatment dummy that switches on in treatment villages for rounds 3 and 4 (pooling together the standalone and interlinked arms since only standalone insurance was sold); and V_{ict} is a panel dummy variable indicating that the individual was treated in that season (received the voucher, the training, or both, in subsequent tables). We cluster standard errors at the village level to account the village-level design effect. Given this specification, β_1 is the impact of being in a treated village and not receiving a voucher, and β_2 is the ITT impact of receiving a voucher. Based on the uptake analysis, we expect β_1 to be zero and we are primarily interested in the sign and magnitudes of β_2.

Table 3 presents the analysis of the insurance vouchers. We begin in Columns 1 and 2 with the analysis of uptake, presenting a binary outcome for any insurance coverage (which in this case means accepting the free insurance policy) as well as a continuous outcome for the sum insured in that season. We then examine a core set of agricultural outcomes, including all of the farming activity of the household and not only the crops covered by the insurance policy. The primary outcome for the study was fertilizer use, which we study using three variables: a dummy for whether the household used any chemical fertilizer at all during the *meher* season covered by the insurance (column 3), the number of kilograms of fertilizer used (column 4), and the number of crops on which fertilizer was used (column 5). We then examine other inputs, including a dummy for any improved seeds (column 6), any credit that was specifically taken for input purchases (column 7), and the total value of all inputs used (column 8). Finally, we broaden the lens from inputs to study an index of agricultural yields across all five major study crops (maize, wheat, barley, sorghum, and maize; each crop yield is z-scored by subtracting the mean yield off and dividing by the standard deviation, and we take the average of this z-score across all crops grown by a given household in column 9), total household income (column 10), and the total area of land cultivated by the household (column 11).

We focus our estimation on the row for 'Any Voucher' (β_2), which generates a very large first-stage effect on uptake of the insurance. Troublingly, none of the three variables describing the use of fertilizer are significantly affected by the provision of insurance vouchers, despite the fact that 39% of the individuals in this group are insured. The use of any chemical fertilizer rises by a mere 3 percentage points off a base of 63%, and this effect has a t-statistic below one. The number of kilograms of chemical fertilizer used actually falls by 4.6, or 4% of the baseline mean, and the number of crops on which fertilizer is used sees a similar small decline. Turning to the use of improved seeds we see an apparently impressive increase of 8 percentage points, but a more careful examination shows that this is relative to a *decline* of 18 percentage points in treatment villages for individuals who do not receive vouchers (both effects significant at 10% level). Credit use does not increase, and the area cultivated declines slowly over time at the same rate in the treatment and the control. (Note that given an overall uptake rate of ~40%, our estimate of the Treatment on the Treated for receiving a voucher would simply be the ITT estimates presented times ~2.5). As we would expect when inputs to the agricultural process remain unchanged, yields are similar across groups and household income remains unchanged by the receipt of the free insurance voucher. Additional analysis (not shown) indicates that there was no effect of insurance coverage on farmers' decisions to plant or dedicate inputs to teff, the specific crop covered by more than 70% of the insurance sold.

Against this discouraging picture of the impact of index insurance vouchers, it is important to highlight the remarkable improvements apparent in the round dummies. The use of chemical fertilizer surges overall during the course of the 2012 (R3) and 2013 (R4) seasons: the fraction of farmers using fertilizers rises from a control average of 64% in 2010-2011 to 79% in 2012 and 75% in 2013. The average number of kilograms used in the control goes from 118 to 130 in 2012 and 2013. So it is important to recognize that lack of insurance impacts is measured during an interval when the target outcomes of the study were strongly improving overall.

Does the lack of impact arise from a lack of statistical power? Clearly the village-level insurance experiment in our reduced sample is underpowered. Examination of the standard errors on the individual-level voucher experiment, however, suggests that power is not a major concern.

Multiplying the standard errors on the impact coefficients in the first row by 1.96, we would be able to detect an increase of at least 8% in fertilizer use, and 7.5% in improved seeds or credit. For fertilizer, this represents a standardized effect size of .18, represented as a fraction of the standard deviation of this variable in the baseline control. This is a relatively small standardized minimum detectable effect, indicating that the voucher-level experiment is adequately powered. Further, we see negative coefficients on two of the three variables describing the primary fertilizer outcome, and we clearly have plenty of power to detect the cross-period changes in input use and incomes. Hence, while the final study sample is less well-powered than the original design intended, we can conclude that at least the individual-level voucher treatment did not result in any substantial improvement in input use or incomes during the period of the study relative to the absence of a voucher.

3.2. The Impact of the Training Exercise.

We next present an analysis that looks at the impact of the training administered by the survey firm at baseline, as well as how the training may interact with receipt of a voucher. Because this analysis combines an intervention randomized at baseline (training) with one randomized across rounds (vouchers) we examine the first and second sales season separately, using an analysis of covariance (ANCOVA) specification to absorb the variation explained by the lagged dependent variable. This specification is :

$$(2) \qquad y_{ict} = \gamma_0 + \gamma_1 T_{ct} + \gamma_2 V_{ict} + \gamma_3 P_{ict} + \gamma_4 (V_{ict} * P_{ict}) + \rho y_{ic1} + \varepsilon_{ict}\,.$$

In this specification P_{ict} is a dummy variable for treatment prior to the outcome observation in survey year *t*; γ_2 gives the impact of vouchers in the absence of the training, γ_3 gives the effect of training in the absence of a voucher (since there was no adoption of insurance in the study sample in the absence of a voucher, we expect this term not to be significant), and γ_4 measures whether the impact of the voucher experiment is different for those trained directly in how the insurance works.

Table 4 presents the results of the ANCOVA analysis, interacting vouchers with trainings for each sales year separately. The first row of this table examines the impact of voucher in the subsample that did not get trained, and the third row the additional impact of vouchers for those who were trained. Beginning with take-up, we see that in the first sales season there was a strong interactive effect of the two treatments (the trained are 11 percentage points more likely to use vouchers) but that this effect of the training had faded by the second sales season. The bottom row shows the coefficient on the lagged dependent variable; most of our outcomes with the exception of 'total value of inputs used' have strong positive autocorrelation. In terms of impacts, in neither sales season and for no combination of treatments do we see any clear pattern of improved agricultural or welfare outcomes. We might have hoped to find that once the insurance was better understood as a result of the training exercise, the receipt of free vouchers would induce a behavioral response among farmers. Unfortunately there is no evidence of this, suggesting that while the training exercise had some commercial logic (increased own purchase of insurance as well as uptake of vouchers), there is no welfare benefit induced by the combination of training and vouchers.

3.3. The Dynamic Effect of Subsidies.

The preceding subsection illustrated that demand for the product was vanishingly small in the absence of subsidies, but that vouchers for insurance created strong variation in the rate at which individuals were successfully issued insurance policies by Nyala. Given the now well-recognized problems of demand in launching a new index insurance product, however, a business case for subsidies can still be made if they have strong enough effects on subsequent demand, and positive dynamic effects of subsidies have been suggested in the context of learning about the new probability distribution of farmer income (Cai et al., 2016). Our study provides a very simple environment in which to pose this question, because we independently randomized subsidies in the first and second sales seasons (measured using survey waves 3 and 4, respectively), meaning that we can examine the impact of first-season subsidies T_{ic1} and second-season subsidies T_{ic2} for individual *i* in cluster *c* on second-season demand D_{ic2}, using the cross-sectional regression

$$D_{ic2} = \beta_0 + \delta_1 T_{ic1} + \delta_2 T_{ic2} + \varepsilon_{ic2} \ . \qquad (3)$$

Standard errors in this regression are clustered at the village level to reflect the design effect. This regression, as all of the main regressions in the paper, is estimated using only the sample of cooperative members. This first pass at this question is provided in Column 1 of Table 5, which analyzes the impact of subsidies in the first and second year on the uptake of insurance in the second year. The first clear result from this table is that the vouchers have strong contemporaneous effects on the probability that an individual is protected by insurance; uptake is elevated by 36 percentage points in the presence of subsidies, and the sum insured is driven strongly by the amount of the voucher in that season. The dynamic subsidy argument requires that subsidies create more permanent demand, however, and when we look at the effect of season 1 vouchers we find no effect that has persisted into season 2. The point estimate on this term is very small (3.8 pp) and far from significant. Column 2 shows that the voucher amount has no effect on uptake either concurrently or subsequently. The impact of the voucher amount on the sum insured displays a similar pattern; strong contemporaneous effects but no detectable dynamic effect at all.

One mechanism through which the literature has suggested sales can be built is that insurance becomes credible once agents see payouts being made (Cole et al. 2014; Cai et al 2016). To test this, we define a dummy variable W_{ic1} based on the rainfall realizations at the local rainfall station and the main crops that a farmer grows, to indicate that they would have received a payout in the first sales season had they decided to purchase insurance (the variable is constructed in the same way for the treatment and control). We then interact this variable with the (randomized) season 1 vouchers.

(4) $$D_{ic2} = \beta_0 + \delta_1 T_{ic1} + \delta_2 T_{ic2} + \eta_1 W_{ic1} + \eta_2 (T_{ic1} * W_{ic1}) + \varepsilon_{ic2}$$

The uninteracted term therefore tests whether having payouts made in your village for your crop to others around you induces subsequent demand, and the interaction term tests whether this effect is bigger for those who received vouchers, roughly 40% of whom actually received insurance payments. Column 3 shows that neither of these terms is significant, indicating that neither the presence of historical payouts nor the receipt of subsidies in combination with payouts leads to increases in demand. Columns 5-8 show similar results on the sum insured; while this outcome responds more strongly to the contemporaneous voucher amount, there is still no impact of lagged voucher receipts. As a final test we calculate the payout amount that an individual would have received based on location, baseline crop, and voucher amount, and in Columns (4) and (8) examine whether this local payout rate drives future sales; again, it does not.

Consequently, this relatively well-powered demand experiment provides no evidence that temporary subsidies resulted in a durable improvement in demand for index insurance.

4. UNPACKING THE LACK OF IMPACT.

We now seek to understand why our study delivered not only low demand for commercial insurance, but such small impacts on agricultural outcomes even when insurance was provided for free. We consider four explanations in turn.

4.1. Impacts of the Sum Insured, using Voucher Amounts as an Instrument.

A first possibility is that many of the vouchers we distributed were simply too small to affect behavior. To investigate this, we pursue two approaches. The first of these is to exploit the fact that the randomly assigned voucher amounts generate very strong experimental variation in the sum insured. Appendix Figure A3 illustrates this relationship, showing the almost perfectly linear relationship between the voucher amount and the sum insured, and illustrating that at the highest voucher amounts the sum insured is nearly \$80, representing 55% of the total value of inputs observed in the control group, which is equal to \$147. We can use this to estimate a slope effect that is sensitive to the sum insured, instrumenting for the (endogenous) sum insured with the randomly assigned voucher amounts. This analysis is presented in appendix Table A7. Panel A of Table A7 Column 1 shows the first stage of the IV regression, indicating that the instrument has a t-statistic of 4, and the IV regression should be well powered to detect impacts. Columns 2-9 present these instrumented impacts on the battery of primary outcomes, illustrating that even when we translate impacts into the marginal effect of the sum insured, we are unable to detect any significant changes in inputs or agricultural productivity as a result of the insurance.

A second way of tackling this question is to isolate the impact of receiving the largest voucher amounts. We do this by defining a separate dummy variable that indicates being treated with the largest voucher amounts, which we characterize as the vouchers for 300, 400, and 500 Ethiopian birr (\$20.83, \$27.78, and \$34.72). Panel B of appendix Table A7 includes separate dummies for the large transfer amounts and the smaller transfer amounts, and at the bottom of the table we present an F-test of the difference between the two impacts. As in Panel A, there is no indication that the study provided any transfers that generate significant impacts; neither of the two voucher dummies is significant and nor is the difference between them, for any of the primary outcomes in the study. While we cannot, of course, speak to the impacts that would have been observed had we distributed larger vouchers, we find no indication that even the largest subsidies in our study had any meaningful impact on investment behavior. This is suggestive that the overall lack of impacts is not arising simply from small average subsidies.

4.2. Differential Impacts by Risk Rationing Status.

A second explanation for low uptake and small impacts would be that only a small share of the farmers in our sample were constrained in their input investment behavior by the presence of risk. In this case, potentially large impacts in the risk-constrained group could be swamped in the Intention to Treat analysis by the small numbers of such individuals. This suggests a heterogeneity analysis where we distinguish the analysis of uptake and impacts by whether individuals were risk constrained at baseline. This is a sub-group analysis that we anticipated in our earliest project documents, and we carefully classified the credit rationing status of households at baseline using the approach outlined in Boucher et al. (2008). Standard agricultural investment models such as Bardhan and Udry (1999), Boucher et al. (2008), and Carter et al. (2016) would all predict that the first-order impacts of insurance on expanding the willingness to borrow and invest in inputs will be strongest in the risk-constrained group. We use a set of survey questions on the access to and use of input credit to classify farmers into four constraint categories: unconstrained (currently using credit), quantity rationed (would not be able to access credit), price rationed (could get loan but find it too expensive), and risk rationed (could get loan and would be able to cover interest costs on average, but unwilling to bear the risk of possible default). Overall, we classify 54.6% of our sample as unconstrained, 18.8% as quantity constrained, 6.8% as price constrained, and 19.8% as risk constrained.

Appendix Table A8 shows the results of this interaction analysis. In order to present the raw effects of the credit rationing status of the household we run this regression as a Difference in difference (DID) without household-level fixed effects. As in previous tables, Columns 1 and 2 show the uptake variables (dummy for purchased insurance and the sum insured), and in neither case does credit rationing status drive uptake, either in absolute terms or in interaction with the receipt of a voucher. When we look at the study outcomes in Columns 3-10, we see that those who were classified as credit rationed at baseline have sharply lower input investment overall: they are 9 percentage points less likely to use any fertilizer than the unconstrained, they use 42

Kgs less fertilizer and use it on roughly a third the number of crops, and total input expenditures among the risk constrained are only about 2/3[rds] of those of the unconstrained. Despite these large cross-sectional differences, there are no signs of significant differential impacts of the provision of vouchers on the risk constrained. Hence, we uncover no evidence that the lack of demand and impact in our sample is masking large impacts among the subset of individuals whose input investment decisions are most constrained by the presence of risk.

4.3. Comprehension and Trust of the Index Insurance Product.

In the round 4 endline, we conducted a survey in treatment villages that asked household heads to describe what they knew of the Nyala index insurance product, how they had learned about it, and what their experiences with the product had been. In terms of awareness of the product and understanding of the voucher distribution, farmers appear to have been properly sensitized. 88.3% of surveyed cooperative members in treatment villages reported having received information about the insurance, although Nyala's strategy of indirect marketing can be clearly seen in their sources of information: 81% reported EEA[9] (the survey entity) as their primary source of information about the product, 8.3% the cooperative leadership, and only 5.4% reported getting information directly from a Nyala representative.[10] A promotional brochure that was developed by Nyala and distributed by EEA had been seen by 56% of households, and 73% of households reported understanding the product 'very well' (9%) or 'partially' (64%). Recall of the voucher amounts that had been distributed was excellent; of the 370 cooperative members randomly assigned to receive vouchers in the second sales season, only 6 incorrectly recalled their voucher amounts. So overall the promotional materials for the insurance and the voucher subsidies were distributed on the ground in a manner that closely conformed to the research design, and awareness of the product was high.

When we dig into the details of the product, however, and the extent of trust that farmers felt in the index and the company, the picture is less rosy. When asked the factual question as to the event that would trigger a payout, only 6.4% of respondents correctly indicated deficit rainfall at the closest rainfall monitoring station. While 23% indicated that they simply did not know, the most common answer (44%) was that payouts would be triggered by crop losses on the

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[9] EEA is the Ethiopian Economic Association, which functions as a research organization.
[10] The remainder reported other sources such as village officials, friends and relatives, etc.

respondent's farm. This indicates that the plurality of cooperative farmers in fact believed that they were being offered indemnity insurance. This certainly points to major potential problems for the insurer in the event of uncovered losses, but it cannot serve as an explanation for low demand or meager impacts because it suggests that farmers believed the product to be closer to a form of perfect insurance than was in fact the case, and were not being dissuaded by the presence of basis risk. Despite this confusion about the index structure, the 22 farmers who reported having purchased insurance and faced an insurable loss all reported having been contacted by Nyala to arrange the payout, and 17 of them had actually received the money by the time of the R4 survey. Thus, it does not appear that disagreements over payouts among the insured, or any actual failure to honor the contact by Nyala, were to blame. Nonetheless, by the time of the endline survey households that received the promotion are no more knowledgeable about the insurance product than households in the same village who had not received the promotion.

Overall, it appears that there were meaningful holes in understanding of the product. A relatively cost-effective promotion of the product led to a meaningful increase in both acceptance of the vouchers and out-of-pocket expenses, but demand among those who received the promotion faded over time, and they did not understand the product better by endline. We conclude that distrust and misunderstanding were prevalent causes of low demand, but that at least in the short term demand can be improved with a more intensive client education and promotion plan.

4.4. State-Led Agriculture and Rigidity in the Input Provision Process.

The heavy state role in the agricultural sector could inhibit demand for a market priced product, particularly given the importance and visibility of the government's massive safety net program (PSNP) in the study area (Duru 2016). Even when free insurance is provided, as was done with our vouchers, the rigidities of the Ethiopian input provision process may inhibit the input usage response at the farmer level. The long planning times involved in the input procurement process, the egalitarian ethos of the cooperatives, and the lack of outside options for purchasing may all limit the ability of individual farmers to adapt their input usage decisions easily. Preliminary evidence that this is the case is provided by the fact that the intra-cluster correlation on the use of chemical fertilizer within the control group across all four rounds is .66, indicating that only one third of the variation in the key input for this study is found within-village.

A simple way of understanding whether the cooperative supply chain generates an inflexible input response is to incorporate into our analysis the two non-cooperative households that were included in both the survey and the voucher treatment in each village. In Appendix Table A9 we conduct analysis of heterogeneity using pooled OLS, including a dummy for the non-coop members, the panel voucher treatment dummy, and the interaction between these two terms. This interaction asks whether a disproportionate response to the treatment is visible in the non-coop members, as might be the case if they had differential access to more flexible sources of inputs or input financing and hence were able to respond more fully to being insured. These results clearly indicate that non-members have significantly *lower* use of chemical fertilizer overall, and despite relatively low power there is no evidence of a disproportionate response to the treatment among the non-cooperative members. Indeed, most of the point estimates are negative, suggesting that if anything those farmers with access to the cooperative supply chain are in fact *more* responsive to the treatment.[11]

Finally, we can examine whether the baseline presence of PSNP in the village or receipt of PSNP at the household level has depressed demand for private insurance. By good luck our sample is relatively well stratified by PSNP access (40% of panel study villages were eligible for PSNP, and within these villages 31% of study households were direct beneficiaries at baseline). This analysis (presented in Appendix Table A10, Panel A) shows that among cooperative members there is no evidence that PSNP depresses uptake of the vouchers; if anything the point estimates are *positive* and of moderate magnitude, in the range of 5-7 percentage points.[12] In Panel B we show the differential impacts of having vouchers for those who also are covered by PSNP. In row 3 of this table we see the outstanding household-level poverty targeting of PSNP: eligible households are less likely to use every kind of measured input at the 99% level and have

[11] If the question of rigidity was a driving one, we might also expect that farmers would have a strong preference to receive credit and insurance earlier in the season, since this would give them more time to adjust input usage. When we look in the data we do not see responses consistent with this; only 3.4% of respondents say that they would have been more interested in the interlinked product if it could deliver credit in a more timely way. Only 9% of respondents say that the timeliness of the insurance offer was the major issue.

[12] This results stands in contrast to Duru (2015), who using the same survey find a depressive effect of PSNP. That analysis includes non-cooperative members and weights the whole sample to be representative of study villages, meaning that the 2 such individuals sampled in each village have a strong effect on the results.

agricultural yields that are just under .2 SD lower than the ineligible. Despite the huge disparity between the eligible and ineligible groups overall, however, there is no solid evidence of differential impacts of insurance vouchers. While the PSNP eligible group do increase 'any use' of fertilizer when given vouchers relative to the voucher-treated ineligibles (significant at 95%), the value of fertilizers used is actually somewhat lower, and use of credit is significantly lower. Total value of inputs used, yields, and household income all see insignificant differential impacts. Therefore PSNP coverage drives neither uptake nor impacts of formal insurance.

So, while it is certainly possible that the marginal effects of free insurance on input use would have been stronger in a more demand-driven market environment, we do not find evidence that the cooperative structure, supply chain rigidity, or public safety nets in the Ethiopian environment are directly responsible for the weak connection between being insured and farmer-level outcomes. Overall, the most compelling argument we find for the lack of impact of the product on farmer behavior seems to be the indirect relationship of the insurer to the farmers and the imperfect informational environment that existed on the ground. In the conclusion we discuss the implication of these results for the design of programs that seek to provide subsidized risk protection to farmers.

5. DESCRIPTIVE ANALYSIS OF THE IMPACT OF THE INTERLINKED PRODUCT.

An additional contribution of the EPIICA project was the emphasis on interlinking WII with a credit product, thereby creating a contingent loan for risk-exposed farmers. We conclude our analysis by providing a brief description of our experiences in this endeavor. Under our design, village-level cooperatives would aggregate demand for the interlinked loans, pass it on to the Unions, who would in turn sign a single loan contract with Dashen Bank for all their sub-cooperatives, collateralized by the productive assets of the Union. This solution represented a potentially fundamental reshaping of the way in which agricultural risk was apportioned in the credit system, and many parties were eager to see this happen. The Amhara state government was seeking a way to lessen their exposure to the agricultural credit portfolio, and Dashen was willing to enter the agricultural credit market because the protection provided by the insurance.

In the end, however, fielding the interlinking proved to be a substantial logistical challenge. The current public credit system makes state governments explicitly liable for the agricultural lending portfolio, permitting the use of extension agents, police, and other officials in the debt collection process.[13] Recognizing the exposure it faced as a private sector entity newly entering a market previously so dominated by the state, Dashen insisted that the Cooperative Unions (CUs) have a stake in the successful recovery of loans through the interlinked product via collateralizing the loans with CU assets. Unions are cooperatively managed entities with a somewhat ambiguous legal status, and this requirement to use their own assets to leverage credit for members was new and difficult to negotiate. This tension suggests a fundamental issue in the process of interlinking: despite the fact that the index insurance product itself generates little moral hazard, an interlinked loan may still have substantial non-weather related residual moral hazard. This makes decisions about where this risk sits very consequential. While the ambiguous legal status of the Unions may be localized to the Ethiopian context, it is a fundamental challenge to reallocate weather-driven default risk in a way that retains the incentive compatibility of repayment in the absence of a weather shock. Given the design of our financial product, the collateralization of the assets of the quasi-public CUs turned out to be the place where this non-weather driven risk became concentrated and consequently proved one of the most difficult steps to negotiate.

All five Cooperative Unions in the project implementation area attended workshops intended to facilitate the understanding and development of the interlinked product, as well as providing financial documents to Dashen that let the bank begin to assess their creditworthiness and begin the contracting mechanism. Only one Union (Merkeb) was ultimately able to clear the various hurdles required to receive loans from Dashen, and they ended up providing interlinked loans to a single village, Feres Wega. Credit did not reach the famers in this village until after the fourth and final round of the full household survey. We did nonetheless design a special survey instrument that was fielded in Feres Wega in a fifth round (in early 2016), including all 20 of the panel individuals that we had previously been tracking and 100 additional randomly sampled households among those who took up the interlinked product. The product was sold in late 2014 and we conducted this fifth survey in early 2016. In that year (2014) the interlinked

[13] The largest parastatal lender in the study credit system at present is the Amhara Credit and Savings Institution (ACSI); some of the means applied by ACSI to compel repayment has led to discontent amongst the customers.

product was offered to all farmers in the stated village, and not only to those in the panel. In total, 254 farmers obtained interlinked insurance and credit through the project in that year.

We can use pre-treatment data to explore the differences between FW and the other Kebeles in our sample, which may account for the ability and willingness of the cooperative and its members to successfully put in place the interlinked product. Household heads in FW are younger and more educated than the average household head in the other kebeles. They are also less food insecure as a smaller share indicate that their income is not enough for even food. Farmers in FW own and cultivate smaller amounts of land, but use inorganic fertilizers and chemicals in larger proportions than in other kebeles. They produce the major crops, teff, barley, wheat, maize, and millet, in higher proportions than the rest. They also have lower income and expenditure per capita. All these taken into account suggest farm households who are headed by younger and more educated farmers, but who cultivate smaller amounts of land with higher input intensity, which, nevertheless, does not provide them with higher income than the other farmers in the sample. These attributes suggest that the farmers in FW maybe more exposed to weather risk, and be more credit constrained than the rest of the farmers in the sample, and these a-priori imply more openness to the interlinked product.

Of the 20 households in the panel from FW, 15 had received at some point a voucher towards the premium, and 11 bought interlinked insurance for an uptake rate of 55 percent. Of those in the panel who received a voucher but did not buy any insurance, the majority (4 out of 6) indicated lack of understanding of the product as the major reason they did not buy. The other 100 surveyed households were outside the original study sample and did not receive any voucher. Of the 120 farmers surveyed, 107 indicated that they received information about the insurance product offered by Nyala. And 94 (out of 105 respondents) indicated that they understood the product well or partially. A very high share (108 out of 120) of farmers among the respondents applied for the Dashen loan covered by weather index insurance. Of the 108 who applied for the loan 105 received the full amount they applied, while the other 3 stated that they received less than what they applied for. The average amount of loan applied for was 1675 birr, with 75 percent of the applicants asking 1400 birr and only 4 applicants asking for 3000 or more birr. Of the 108 farmers who received the loan, 45 knew that the insurance premium was included in the price of the loan. All 108 applicants applied via the local cooperatives and 105 out of the 108 felt that the system of getting bank loans through the cooperatives was appropriate.

We have no counterfactual for the changes measured in Round 5 in Feres Wega, both because this cooperative is clearly endogenously selected in a number of dimensions, and because we did not conduct panel surveys in other villages in this round. We nonetheless conduct an exploratory analysis as to the possible effects of interlinked insurance and credit on input use by simply asking respondents in this village about the changes in their farming behavior that occurred after they received an interlinked loan. Appendix Table A11 shows that among those who applied for an interlinked loan, the number reporting increases in chemical fertilizer use (72) far outweigh the numbers who indicate no change (27) or a decline (9). In all input categories the number of those who declared that they increased the use of the relevant input is larger than the number of those indicating a decrease. So while successfully fielding the interlinked product in only one of the intended 40 villages underlines the hurdles to implementing these contracts, a surge in input use in FW is consistent with the idea that where we can manage to relax credit and risk constraints simultaneously, input use and productivity will rise.

6. CONCLUSION

Insurance, like credit, is a product that is inherently time inconsistent in the absence of some mechanism to enforce adherence to the contract. The fundamental difference is that credit is time inconsistent for the client, while with insurance it is the firm who wishes to renege. This means that the dynamic credibility of the insurer is a critical precondition for the emergence of market demand. While Ethiopia is known across the world for the severity of its droughts and the huge toll they can take on peasant farmers, it is also an environment in which the public sector plays a central role (particularly in the cooperative sector studied here). Private sector actors in the country face an ambiguous legal environment without clear property rights or enforcement of contracts. Thus the context of this study provides a very strong environment in which to test for the impacts of reducing weather risk, but also potentially an uphill climb for private-sector insurance provision.

The effort to shift risk away from smallholder farmers and towards the international reinsurance system is one that appears to present large welfare benefits, but to achieve it through index insurance one must generate wide-scale adoption. Our study illustrates several discouraging points as to the promise of the private sector to deliver this in the Ethiopian context. First, it is clear that the standalone weather index insurance product as promoted and marketed elicited little market demand, and even an individually-targeted promotion conducted at the household level by our survey firm led to little long-term willingness to pay. If a sustainable private market it is to be achieved, good marketing & promotion will be central.

In our study, subsidies providing small free 'tester' insurance policies, generated a willingness to accept the contract but no subsequent improvement in private demand in subsequent seasons even if that insurance policy paid out in the first year. Thus, we find no support for the idea of subsidizing to build the market. Finally, we show that this free insurance did not result in meaningful increases in input investment, suggesting that the productivity argument for subsidies is also absent. Hence, marketing and subsidizing micro-insurance policies all the way down to the level of individual farmers did not relax risk constraints enough to unlock first-order increases in productivity. More positively, our results show hope that a properly developed and marketed interlinked product could meet with demand. In the cooperative where all of the steps in the chain were forged to provide interlinked insurance to farmers, uptake was strong and more than a hundred farmers were able to access interlinked, private-sector credit.

What is the takeaway message from this study? It is certainly not that risk is unimportant in agriculture; our survey data is full of evidence that this is not the case. A large share of surveyed farmers report risk as the main impediment to purchasing more inputs. That individually-marketed index insurance is not a viable commercial product is now relatively well-established. Where our results provide particular caution, however is in illustrating that in a context with indirect marketing and promotion of the insurance to farmers, even free risk protection does not trigger an agricultural response. Many of the studies that have shown production effects from subsidized risk protection have done so in the context of large-scale and well-known national programs, such as those in India (Cole et al. 2017), or China (Cai 2016), or using intensive multi-visit marketing of the insurance at the household level (Karlan et al. 2014). One can therefore interpret our results as being evidence of the fact that the construction of the infrastructure that credibly delivers an insurance-driven productivity surge among rural farmers via individually-marketed insurance is a heavy lift. Our product was both new and imperfectly understood, and in this context the hoped-for input response to risk protection did not materialize. This suggests that success in driving agricultural productivity through *ex ante* risk protection will need to happen in the context of a substantial effort to build familiarity and credibility. Otherwise the production-side benefits of *ex ante* risk protection are not realized, in which case effective *ex post* risk protection to smallholders can likely be more cost effectively provided by meso-level actors. Particular attention should be paid to innovations that sit at the nexus of credit and risk markets.

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TABLES

TABLE 1. PROMOTION AND VOUCHER EXPERIMENT IN THE PANEL SAMPLE

		Control (15 villages)	**Standalone Arm** (17 villages)	**Interlinked Arm** (17 villages)
Initial # Cooperative Households:		*270*	*306*	*306*
First Sales Season (outcomes in R3 survey)				
Households remaining in panel:		*257*	*300*	*291*
Provided Training & Promotion		0	102	102
Provided Voucher:	None	257	46	51
	100 birr ($6.94)		49	48
	200 birr ($13.89)		84	69
	300 birr ($20.83)		78	79
	400 birr ($27.78)		22	24
	500 birr ($34.72)		21	20
Second Sales Season (outcomes in R4 survey)				
Households remaining in panel:		*249*	*296*	*296*
Provided Voucher:	None	249	82	133
	100 birr ($6.94)		68	51
	200 birr ($13.89)		74	56
	300 birr ($20.83)		72	56

TABLE 2. INSURANCE UPTAKE.

Round 1:	All					
Uptake Rate:	N/A					
Round 2:	All					
Uptake Rate:	N/A					
			Voucher Treatment		**Voucher + Promotion**	
Round 3:	Control Villages	No Voucher, Treatment Villages:	Voucher only	Difference from control	Voucher + EEA Promotion	Difference from voucher only
Uptake Rate:	0	0	27.5%	0.275***	36.9%	0.0947**
Average Premium Payment	0	0	$3.77	3.772***	$7.52	3.752***
Average Own Money Spent:	0	0	$0.20	0.202	$0.45	0.251
Average Sum Insured:	0	0	$25.10	25.10***	$43.15	18.06***
Round 4:						
Uptake Rate:	0	0	47.4%	0.474***	41.0%	-0.0638
Average Premium Payment	0	0	$1.19	1.191	$3.04	1.844*
Average Own Money Spent:	0	0	$0.73	0.727	$2.19	1.464*
Average Sum Insured:	0	0	$40.40	40.40***	$36.16	-4.248

TABLE 3. PANEL DID IMPACT OF THE VOUCHER EXPERIMENT

	Covered by Insurance	Sum Insured	Any Chemical Fertilizer	KGs of Chemical Fertilizer	Number of crops using Chemical Fertilizer	Uses any Improved Seeds	Uses any Input Credit	Total Value of Inputs Used	Index of Agricultural Yields	HH Income per Capita	Area Cultivated
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Any Voucher	0.356***	39.23***	0.0309	-4.639	-0.0604	0.0779*	0.0171	-14.33	-0.0207	78.94	0.418
	(0.0498)	(7.761)	(0.0419)	(6.218)	(0.0903)	(0.0404)	(0.0436)	(19.19)	(0.0481)	(66.84)	(0.354)
Treated Village	0.00228	-1.023	-0.0448	2.550	-0.0313	-0.185**	0.0646	-6.399	-0.0713	-218.8*	-0.828
	(0.0113)	(1.596)	(0.0923)	(7.794)	(0.172)	(0.0743)	(0.0408)	(27.93)	(0.108)	(111.0)	(0.778)
Round 3 dummy	-0.000381	3.707	0.146*	11.67**	0.350**	0.0883	0.0209	-16.95	-0.0372	93.86**	-0.526
	(0.0179)	(2.753)	(0.0807)	(4.363)	(0.149)	(0.0570)	(0.0238)	(22.25)	(0.101)	(46.05)	(0.397)
Round 4 dummy	0.000387	-3.823	0.115	12.22**	0.237*	0.0362	-0.0875**	-8.733	0.0680	138.9***	-0.529
	(0.0185)	(2.842)	(0.0743)	(5.497)	(0.135)	(0.0624)	(0.0345)	(29.34)	(0.108)	(47.61)	(0.329)
Constant	5.58e-05	0.0116	0.640***	117.8***	1.383***	0.475***	0.186***	174.3***	0.0135	265.9***	2.128***
	(0.0128)	(1.716)	(0.0272)	(2.542)	(0.0476)	(0.0196)	(0.0145)	(17.90)	(0.0335)	(28.10)	(0.552)
Observations	3,446	3,446	2,544	3,280	2,571	2,544	3,416	3,416	3,191	2,561	3,423
R-squared	0.297	0.190	0.042	0.026	0.037	0.023	0.022	0.004	0.015	0.004	0.008
Number of households	882	882	881	880	882	881	882	882	875	881	882
Baseline mean	0	0	0.631	114	1.370	0.470	0.185	172.8	0.00962	265.3	2.108

Regressions are household fixed-effects analysis weighted to be representative of all cooperative members. The first two columns estimate the effect of the intervention on uptake (acceptance of the free insurance voucher). Remaining columns examine impacts on agricultural and household outcomes. Data includes two pre-treatment rounds for some variables and one for others; all variables have two post-treatment observations. Voucher treatment re-randomized at the individual level in rounds 3 and 4. Robust standard errors are reported in parentheses, clustered at the village level to account for the design effect. *** p<0.01, ** p<0.05, * p<0.1

TABLE 4. ANCOVA IMPACTS OF VOUCHERS AND TRAINING

First Sales Season

	Covered by Insurance	Any Chemical Fertilizer	KGs of Chemical Fertilizer	Number of crops using Chemical Fertilizer	Uses any Improved Seeds	Uses any Input Credit	Total Value of Inputs Used	Index of Agricultural Yields	HH Income per Capita	Area Cultivated
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Voucher Provided in Corresponding Season	0.280***	-0.0422	-37.55*	-0.252	0.0423	0.0459	-31.68	-0.0745	18.79	0.127
	(0.0689)	(0.0536)	(21.20)	(0.198)	(0.0963)	(0.0785)	(28.95)	(0.0737)	(49.99)	(0.137)
EEA-provided Training at Baseline	-0	0.00917	-34.47	-0.0946	-0.0146	0.0960	-17.23	0.0351	120.9	0.0108
	(1.26e-08)	(0.0772)	(20.64)	(0.211)	(0.122)	(0.107)	(30.14)	(0.107)	(106.2)	(0.152)
Voucher * Training	0.110**	-0.0137	41.95	0.151	0.0347	-0.00244	30.60	0.0161	-175.3	-0.0903
	(0.0448)	(0.0833)	(26.25)	(0.291)	(0.119)	(0.121)	(45.83)	(0.146)	(122.1)	(0.200)
Baseline Outcome		0.348***	0.794***	0.643***	0.513***	0.306***	0.0254	0.177***	0.324***	-0.000210
		(0.0673)	(0.0342)	(0.0522)	(0.0532)	(0.0628)	(0.0279)	(0.0598)	(0.00658)	(0.00303)
Observations	848	829	787	848	829	820	820	763	845	848
R-squared	0.188	0.175	0.598	0.400	0.262	0.079	0.018	0.037	0.643	0.005
Baseline Control Group Mean	0	0.631	114	1.370	0.470	0.185	172.8	0.00962	265.3	2.108

Second Sales Season

	Covered by Insurance	Any Chemical Fertilizer	KGs of Chemical Fertilizer	Number of crops using Chemical Fertilizer	Uses any Improved Seeds	Uses any Input Credit	Total Value of Inputs Used	Index of Agricultural Yields	HH Income per Capita	Area Cultivated
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Voucher Provided in Corresponding Season	0.420***	0.0332	15.82	-0.0477	0.113	-0.0151	-50.35*	-0.0277	7.212	-0.167
	(0.0635)	(0.0793)	(19.76)	(0.158)	(0.0771)	(0.0755)	(26.93)	(0.0719)	(44.21)	(0.184)
EEA-provided Training at Baseline	-0	0.0327	12.73	0.0436	0.0506	-0.0214	-13.28	0.0909	-51.20	0.00527
	(5.73e-09)	(0.0513)	(24.89)	(0.142)	(0.0563)	(0.0649)	(33.62)	(0.0688)	(40.49)	(0.126)
Voucher * Training	-0.0925	0.0427	-29.76	0.0852	-0.160**	0.0110	29.74	0.0129	106.7	-0.0251
	(0.0669)	(0.0789)	(25.81)	(0.184)	(0.0747)	(0.0724)	(31.22)	(0.0785)	(69.58)	(0.204)
Baseline Outcome		0.404***	0.789***	0.646***	0.407***	0.196***	0.0370	0.111**	0.446***	0.000986
		(0.0659)	(0.0619)	(0.0437)	(0.0440)	(0.0494)	(0.0308)	(0.0466)	(0.00467)	(0.00310)
Observations	841	834	765	841	834	831	831	724	833	841
R-Squared	0.242	0.204	0.562	0.437	0.186	0.052	0.023	0.034	0.616	0.011
Baseline Control Group Mean	0	0.546	91.11	1.180	0.368	0.153	147.6	0.00389	247.6	1.563

Regressions are round-by-round ANCOVA estimations weighted to be representative of all cooperative members. The first two columns estimate the effect of the intervention on uptake (acceptance of the free insurance voucher). Remaining columns examine impacts on agricultural and household outcomes. Voucher treatment re-randomized at the individual level in rounds 3 and 4. Robust standard errors are reported in parentheses, clustered at the village level to account for the design effect. *** p<0.01, ** p<0.05, * p<0.1

TABLE 5. DYNAMIC EFFECTS OF SUBSIDIES ON DEMAND

	Uptake of Insurance in Year 2				Sum Insured Year 2			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any Voucher Year 1	0.0385	0.0433	0.0439	0.0365	55.04	66.02	44.55	45.19
	(0.0446)	(0.0572)	(0.0647)	(0.0567)	(50.95)	(83.68)	(88.93)	(83.94)
Voucher Amount Year 1		-0.000203	-0.000145	-0.000385		-1.538	-1.365	-0.193
		(0.00220)	(0.00218)	(0.00219)		(3.431)	(3.381)	(3.437)
Any Voucher Year 2	0.361***	0.378***	0.371***	0.373***	422.3***	149.7	131.7	121.2
	(0.0548)	(0.0957)	(0.0979)	(0.0984)	(91.09)	(104.8)	(102.4)	(99.60)
Voucher Amount Year 2		-0.00126	-0.00111	-0.00115		19.32**	19.69**	19.83**
		(0.00593)	(0.00591)	(0.00592)		(7.909)	(7.931)	(7.894)
Panel village treatment dummy	-0.0313	-0.0321	-0.0436	-0.0378	-44.74	-30.57	-46.66	-54.33
	(0.0363)	(0.0357)	(0.0423)	(0.0353)	(41.91)	(42.08)	(47.53)	(39.99)
Insurance would have paid out Y1			0.0723	0.0385			119.4	183.1
			(0.0879)	(0.0659)			(127.0)	(139.7)
Voucher Y1 * Insurance would pay Y1			-0.0280				10.76	
			(0.0881)				(105.3)	
Insurance payout if Y1 voucher was used				5.51e-05				-0.261
				(8.59e-05)				(0.161)
Constant	0***	0***	-0.00368	-0.00196	0***	0**	-6.077	-9.322
	(0)	(0)	(0.00500)	(0.00377)	(0)	(0)	(7.450)	(10.86)
Observations	841	841	841	841	841	841	841	841
R-squared	0.237	0.237	0.240	0.241	0.135	0.152	0.161	0.164

Regressions present pooled OLS analysis, weighted to be representative of cooperative members in study villages; dependent variable is the insurance purchase decision observed using institutional data from the insurer in the second sales season. 'Insurance would have paid out' is based on location and crop grown + actual payout rates per crop and rainfall station. 'Insurance payout if Y1 voucher was used' is the amount the payout would have been given main crop if the sum insured were driven only by the randomized voucher amount in the previous season. Robust standard errors are reported in parentheses, clustered at the village level to account for the design effect. *** p<0.01, ** p<0.05, * p<0.1

FIGURES

FIGURE 1. CONSORT DIAGRAM OF STUDY RECRUITMENT AND ATTRITION.